

The Cambridge Handbook of

Biolinguistics

edited by **Cedric Boeckx**
and **Kleanthes K. Grohmann**

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The Cambridge Handbook of Biolinguistics

Biolinguistics involves the study of language from a broad perspective that embraces natural sciences, helping us better to understand the fundamentals of the faculty of language. This handbook offers the most comprehensive state-of-the-field survey of the subject available. A team of prominent scholars working in a variety of disciplines is brought together to examine language development, language evolution, and neuroscience, as well as providing overviews of the conceptual landscape of the field. The handbook includes work at the forefront of contemporary research devoted to the evidence for a language instinct, the Critical Period Hypothesis, grammatical maturation, bilingualism, the relation between mind and brain, and the role of natural selection in language evolution. It will be welcomed by graduate students and researchers in a wide range of disciplines, including linguistics, evolutionary biology, and cognitive science.

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*We would like to dedicate this handbook to
Eric Lenneberg (1921–1975), whose early efforts
set the standard for a biological science of language.*

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Introducing the volume

Kleanthes K. Grohmann and Cedric Boeckx

The *Handbook of Biolinguistics* is intended to be, first and foremost, an illustration of the range, and explanatory power, of interdisciplinarity afforded in the domain of cognition, and more specifically, linguistics, when the nature of human language is approached from a biological perspective. This biological perspective has roots that go back to the birth of modern science and natural philosophy, and it has been emphasized in modern times by Noam Chomsky, Eric Lenneberg, Morris Halle, and many others since.

Biolinguistics takes as its focus of inquiry the remarkable ability, present in all children barring severe pathologies, to develop a grammatical system (“linguistic competence”) that is both amazingly plastic (if it were not, we would not witness the linguistic variation we find among human languages) and surprisingly constrained (so many logically possible rules do not appear to be part of any human language). Although the environment in which the child grows up certainly plays a role in triggering the use of this capacity for language, it is equally obvious that the human child must be biologically equipped in a way distinct from other species if we are to explain how humans turn some of the noise around them into news.

Some sixty years ago, when they were still graduate students, Chomsky and Lenneberg decided to find out what the biological foundations of the human linguistic capacities were, and in so doing created the field of biolinguistics. As should be obvious, biolinguistics is a supremely interdisciplinary enterprise, requiring insights from many fields and collaboration among many researchers from vastly different backgrounds. This, of course, comes with its own challenges. It is all too easy for a researcher focusing on a particular problem to not only lose sight of the big picture, but also to be unable to keep track of recent developments in allied disciplines that may shed light on the particular problem at hand.

We are very fortunate to write at a time when the interdisciplinarity that is characteristic of the field of biolinguistics is making a comeback at the

forefront of many studies in the language sciences, so much so that we felt it was the right time to not only launch a new journal that strives for a rapprochement among disciplines, the open-access *Biolinguistics* (www.biolinguistics.eu), but also to provide students and experts alike with a representative sample of some of the best and most influential works in biolinguistics today. Hence this handbook.

In the mid 1970s, Salvador Luria singled out the field of biolinguistics in an AAAS keynote address as a very promising avenue for both biologists and linguists. Luria felt that the time was ripe for the two disciplines to be confronted with each other's results. Developments in genetics, developmental biology, computer science, neuroscience, experimental psychology, and theoretical linguistics in the intervening years have vastly expanded the range of results to take into account and, we think, have raised the prospects of genuine convergence – an opinion that we hope the reader of this handbook will share.

Needless to say, preparing a handbook of such a complex and varied field-in-the-making as biolinguistics is no easy task. We were confronted with difficult choices as to what to include, and we are painfully aware of the fact that some may feel that their favorite areas of study were not given pride of place in the pages that follow. We have tried to be as comprehensive as possible while remaining within reasonable size limits. Being theoretical linguists by training we decided not to make theoretical debates – as fascinating as we find them – the focus of our handbook. When it comes to theoretical assumptions, the reader will find an unusually pluralist perspective in the chapters that make up this volume. Although we personally favor certain theoretical commitments, we did not feel that it was right to be dogmatic about them in a handbook like this one. Instead, we decided to organize the handbook along several research questions that Eric Lenneberg placed at the heart of his 1967 *Biological Foundations of Language*: language development, language evolution, and the relation between mind and brain. These form the core axes of this volume.

In addition, we wanted to offer a historical and conceptual overview of the field, to help the reader relate current findings to long-standing questions. This is the purpose of the early chapters of this handbook, which includes contributions by Lyle Jenkins (the author of the first comprehensive overview of the range of questions at the heart of biolinguistics, Jenkins 2000), Massimo Piattelli-Palmarini (the person who gave the name “biolinguistics” its modern meaning and one of the strongest advocates of interdisciplinarity in the field), and James McGilvray (an expert on the philosophical foundations of modern linguistics and cognitive science).

Part I of the handbook addresses central issues in the domain of language development, many of which were introduced by Eric Lenneberg: the range of evidence in favor of a “language instinct” (Tsimpli), the

existence of a critical period for language acquisition (Meisel), the issue of maturation in the context of language acquisition (Wexler), and the impact of language on other cognitive systems (e.g., as seen in the domain of bilingualism; Hernandez, Martin, Sebastián-Galles, and Costa). **Part I** also reviews influential proposals on how the child may use the input to figure out the properties of the language to be acquired (Millotte, Cauvet, Brusini, and Christophe on the “prosodic bootstrapping” hypothesis), and the range of options (“parameters”) that biology seems to make available to the child during the course of language acquisition (Pearl and Lidz).

Part II focuses on the interplay between mind, brain, and behavior. It deals with the nature of theoretically informed experiments (Sprouse and Almeida), working memory and language processing (Wagers and McElree), modularity (Marcus and Rabaglia, and Rabagliati), language deficits (Benítez-Burraco), and pathologies (Friedmann, Biran, and Dotan). It also provides overviews of what we currently know about how basic properties of core domains of linguistic inquiry, i.e. syntax (Schlesewsky and Bornkessel-Schlesewsky), semantics (Pylkkänen, Brennan, and Bemis), morphology (Bornkessel-Schlesewsky and Schlesewsky), and phonology (Monahan, Lau, and Idsardi), may be implemented in the brain. Finally, it traces the fate of Broca’s area and its use in relating mind and brain (Hickok).

Finally, **Part III** of the handbook focuses on a range of issues relevant to the study of language evolution: the cognitive capacities of non-human primates (Zuberbühler), the abilities of non-human vocal learners (Okanoya), the potential use of fossil records to shed light on the evolution of language (Balari, Benítez-Burraco, Longa, and Lorenzo), the possible role of natural selection (Bickerton), and the insights from computational modeling in the context of language evolution (Kirby).

It goes without saying that entire volumes could well be devoted to each of the topics covered in this handbook. Accordingly, readers are asked to take the contributions that follow as solid points of departure to explore the literature. Inevitably, our contributors have emphasized certain aspects (be they theoretical assumptions, experimental results, etc.) at the cost of others, but taken as a whole we hope that the volume offers a comprehensive overview of the fruitfulness of interdisciplinarity, and of the promises of the field of biolinguistics.

2

Biolinguistics: A historical perspective

Lyle Jenkins

2.1 Introduction

In what follows I present a historical sketch of what has become called *biolinguistics*, the study of the biology of language. My intention is to provide the reader of this handbook with enough of the history of the field to provide the context in which the questions discussed here arose and hopefully to illuminate their subsequent development. I provide sources for additional reading. However, in some cases my selections are meant only to be illustrative. I am not attempting a comprehensive overview here and put aside questions of priority.

Biolinguistics, as the study of the biology of language, poses exactly the same kinds of questions as in other areas of biology; e.g., questions about form/function, ontogeny, and phylogeny (Chomsky 1976; Chomsky and Lasnik 1993).

1. What is knowledge of language?
2. How does language develop in the child?
3. How does language evolve in the species?

The shift in viewpoint from structural linguistics to the modern biolinguistic viewpoint of language (as described by a *generative grammar*) as part of an innate language faculty was marked by the circulation of a draft of Noam Chomsky's *Logical Structure of Linguistic Theory* in 1955 (Chomsky 1955/1975).

At the same time Eric Lenneberg investigated many of the areas to be explored in biolinguistics in the decades that followed, including the genetics of language acquisition and of language disorders (dyslexia, specific language disabilities), language of deaf children, "wolf children," the critical period, twin studies, family pedigrees, aphasia, and evolution of language. This work culminated in Lenneberg's *Biological Foundations of Language* to which Chomsky contributed a chapter entitled "The formal

nature of language" (Lenneberg 1967); for an analysis of the connections between Lenneberg's work and modern biolinguistics, see Boeckx and Longa (2011).

This was followed by an interdisciplinary meeting on language and biology at Dedham, Massachusetts, in 1974, sponsored by the Royaumont Center for a Science of Man. This meeting, organized by Massimo Piattelli-Palmarini, brought together researchers in linguistics and biology to discuss language and the brain, as recommended by Chomsky and the biologist and Nobel Laureate Salvador Luria. The report for this meeting referred to the topics discussed by the term "biolinguistics" (Piattelli-Palmarini 1974). After the Dedham meeting, the MIT Work Group in the Biology of Language was formed (1975–76) with the support of the Alfred P. Sloan foundation and MIT (Walker 1978).

The Royaumont Center also developed a "Communication and Cognition" project under the sponsorship of Luria and Chomsky with the assistance of others (for additional details, see Jenkins (2000)). This project, also organized by Piattelli-Palmarini, held a conference on "Ontogenetic and Phylogenetic Models of Cognitive Development" at Royaumont Abbey near Paris in October, 1975. There were discussions of many topics bearing on the biology of language at this conference which was attended by Chomsky, Piaget, and many biologists, including Jean-Pierre Changeux, François Jacob, and Jacques Monod, among others (Piattelli-Palmarini 1980); for a retrospective see also Piattelli-Palmarini (1994). Concurrently, Paris had become an important center internationally for the study of generative grammar.

In 1976 another conference, this one with a focus on evolution of language, Origins and Evolution of Language and Speech was organized by the New York Academy of Sciences. Chomsky was a speaker at that conference and one of the conference organizers, Stevan Harnad, noted in his introductory remarks that "the revolution of linguistics due to Noam Chomsky has provided a very different idea of what the nature of the 'target' for the evolutionary process might actually be."

At the same time there was a great deal of work underway on brain and language, including neurology of language and evolution of language; e.g. Geschwind and Galaburda's work on cerebral dominance and asymmetry (Geschwind and Galaburda 1984, 1987), Le May and Geschwind's work on the morphological asymmetries of the brains and skulls of non-human primates (LeMay and Geschwind 1975), to mention only a few examples. Nor was work on evolution of lateralization limited to the language areas; consider, e.g., Denenberg and colleagues' work on functional asymmetries in the rat (Denenberg 1981) as well as the numerous asymmetries in lower organisms documented by Corballis and Morgan (Corballis and Morgan 1978; Morgan and Corballis 1978).

The conference on Maturational Factors in Cognitive Development and the Biology of Language was held in 1978. One can get a flavor of the

discussions taking place among linguists and neurologists at that time in the discussion between Chomsky and the neurologist Norman Geschwind about a number of questions concerning evolution of language; among others, about the cerebral asymmetries in the great apes, auditory tasks in the left temporal lobe of the monkey, asymmetry for emotional behavior in the brain, the recognition of species-specific cries in the left hemisphere of Japanese monkeys, male-female differences in left-right asymmetry for areas involved in bird song, and so on (Caplan and Chomsky 1980).

Much of the work in these areas was soon forgotten and an attempt was made to rewrite history, at least in the media. In 1998, when Gannon and colleagues reported their findings of an asymmetry in the left planum temporale area of chimpanzee brains (Gannon *et al.* 1998), this was heralded in the press as “challenging cherished notions of how language evolved in humans and why apes cannot talk” (*New York Times*). However, left-right asymmetries in non-human primates, including the planum temporale area, had been long known (see e.g. Cunningham 1892; Fischer 1921; Yeni-Komshian and Benson 1976). The German magazine *Der Spiegel* claimed that until the study of Gannon *et al.*, it had been thought that the left and right sides of the brains of non-human primates were absolutely equal, although this had been shown twenty years earlier not to be the case by the study of Beheim-Schwarzbach (1975), who had compared the temporal regions in humans, chimpanzee, and the orangutan.

In 1977 GLOW (Generative Linguistics in the Old World) was founded by Henk van Riemsdijk and colleagues in the Netherlands, and it rapidly became vital to the development of biolinguistics throughout Europe and around the world. GLOW organizes an annual conference, periodic summer schools, and publishes a newsletter (<http://glow.uit.no>, with a link to Facebook), all of which have greatly helped to promote generative grammar. In recent years a sister organization was founded in Asia, GLOW (Asia). The annual conference in 2010 was hosted in Beijing (www.blcu.edu.cn/CLT/glow/glowasia8.html).

In 1979 the Linguistics Society of America held its first Summer Institute abroad at the Joint Linguistic Society of America and Summer Linguistics Institute at the University of Salzburg, Austria, with the theme of “Linguistics and Biology,” which included courses, seminars, and other presentations as well as discussions on linguistics and biology of language, including neurology and the evolution of language. Also around this time there were many fruitful contacts between the ethologist and evolutionary biologist, Konrad Lorenz and his colleagues in Austria and at the Max Planck Institute in Germany and generative linguists at the University of Vienna and Salzburg. In 1976 Lorenz and his colleagues participated in a symposium on language and biology at the Salzburg Summer School of Linguistics.

In 1980 the Harvard Medical School Biolinguistics Group was formed under the sponsorship of Allan Maxam’s Laboratory of Molecular Biology

to provide an interdisciplinary forum for researchers interested in the biological foundations of language (Jenkins 2000). Topics ranged over theoretical linguistics, molecular biology, learning disorders, neurobiology of animal communication, neurolinguistics, brain lateralization, neural plasticity and critical periods, aphasia, dyslexia, critical period in vision, dreams, computational linguistics, pre-linguistic speech perception in infants, chromosomal language disability, and evolution of language. Maxam and Jenkins also founded the Biolinguistics Institute in Cambridge, Massachusetts, dedicated to the promotion of interdisciplinary research into the biology of language.

Recently, there have been a number of conferences and publications sponsored by the International Biolinguistics Network (Di Sciullo 2010) (see below).

2.2 Questions in biolinguistics

Note that the three questions about biology of language above are interrelated in a particular way. The question of how language develops in the child (2) depends on understanding what the properties of the language system are, the answer to the question about what knowledge of language is (1). And the third question about how language evolved in the species, depends crucially on the answers to the first two questions. In practice, one only has partial answers to all three questions, so that it becomes necessary to study all the questions in parallel, constantly revising the answers as new empirical data becomes available.

Consider question (2) for the moment. How does language develop or “grow” in the child? This process is often visualized as the “language acquisition device” (LAD) (Chomsky 1965), which maps experience to a particular language (English, Japanese, etc.). The problem for biolinguistics is to determine what is in the box; that is, to discover what mechanisms one must assume that the child brings to language in order to map experience (“primary linguistic data”) to a particular language.

Language “grows” from an initial state, proceeds through a series of intermediate states and then attains a final state in the adult. Here the initial state corresponds to the genetic endowment and the final state corresponds to English, Japanese, etc. The linguist’s characterization of the initial state is termed *universal grammar* (UG) and that of the final state, *grammar*.

The answers to questions (1)-(3) have in turn stimulated investigation into the deeper “why” question; i.e. why are the principles of language what they are? – the basis for the “minimalist” program (Chomsky 1995b). The answers to all of these questions will provide insight into the “unification problem”; i.e. how the study of language can be integrated with the rest of the natural sciences (Chomsky 1994).

In studying any of these questions, one must keep in mind that properties of an acquired language result from an interaction of three factors (Chomsky 2005):

- (a) genetic endowment
- (b) environment and
- (c) principles not specific to the faculty of language.

A conceptual breakthrough was achieved with the development of the principles and parameters approach to language acquisition (Chomsky 1981a; see additional references there). Here genetic endowment provided universal principles (UG), which were parameterized, with environment providing particular values for each parameter; e.g., head-initial or head-final. Then a particular language, like English, can be regarded as a collection of parametric choices for the grammar, in addition to a list of vocabulary items, which must (in part) be learned. This provided a resolution for the acquisition paradox; i.e. that it seemed that “languages could differ from each other without limit and in unpredictable ways,” as the structural linguist Martin Joos put it (Joos 1957), and at the same time, the universal format for language needed to be highly restricted to account for the ease and speed of acquisition as well as for poverty-of-stimulus considerations (Chomsky 2009). Chomsky noted that the Nobel Laureate François Jacob was posing similar questions in the area of molecular biology, asking, “what accounts for the difference between a butterfly and a lion, a chicken and a fly, or a worm and a whale.” Jacob concluded that it wasn’t biochemical innovation, but new regulatory circuits (Jacob 1978). Chomsky remarked that “in a system that is sufficiently intricate in structure, small changes at particular points can lead to substantial differences in outcome” and that the principles and parameters approach had the right properties to account for acquisition from this perspective (Chomsky 1980/2005).

The “cartographic project,” has focused inquiry on syntactic configurations (including phrases with such syntactic elements as complementizer, tense, inflection, determiner, etc.) to map out the fine-structure of the areas around these elements, uncovering additional invariant structural properties of language (Cinque 1999, 2002; Rizzi 2004; Belletti 2004; Cinque and Rizzi 2010 and references there). Much research has also focused on the nature of parameters; e.g., their locus in the lexicon, in particular, as properties of functional elements, macroparameters vs. microparameters (Cinque and Kayne 2005), parameters and (non)pronunciation (Kayne 2010), etc.

Chomsky has suggested that principles of efficient computation, such as minimal search are principles that might not be specific to the faculty of language (c) and provide part of the answer to the question of “why” language is like it is (Chomsky 2009). Another source of principles might be dynamical system constraints such as symmetry breaking

(Jenkins 2000, 2011). A third might be probabilistic or stochastic constraints. Similar “why” questions can be asked about any biological system – viruses, bacterial cell division, protein folding, sunflowers, bumblebees, falling cats, wiring of circuits in nervous systems, etc.

These design properties may be non-domain specific or non-species specific. (Here, of course, biological design is meant, not the “intelligent design” of creationism.) For example, properties of human language might (or might not) turn out to be specific to language, or may be operative in other cognitive domains, such as mathematics, vision, etc. Other properties might (or might not) be shown to be specific to humans or might be found in other non-human species as well (Hauser, Chomsky, and Fitch 2002). To answer such questions, one needs in-depth studies of other cognitive domains (Boeckx 2009), such as mathematics (Dehaene *et al.* 2006) and cross-species comparative work (Christiansen and Kirby 2003a; Fitch 2010).

The questions above, including the study of the three factors, must be posed in any biolinguistic approach to biology (see Di Sciullo *et al.* 2010; Di Sciullo and Boeckx 2011). Moreover, they have been studied since the earliest days of modern biolinguistics and are currently under investigation in the *minimalist program* (*Minimalism*) (and its predecessors) (Chomsky 1995b; Boeckx 2006, 2008, 2011). For some other perspectives on biolinguistics, see Larson, Déprez, and Yamakido (2010).

2.3 Current research

Research into biolinguistics since the early 1950s has drawn on many kinds of evidence: theoretical linguistics (including universal and comparative grammar, syntax, semantics, morphology, phonology, and articulatory and acoustic phonetics; language acquisition and perception; language change (Radford, Atkinson, Britain, Clahsen, and Spencer 2009; Hogan 2011), sign language (Brentari 2010), language contact (Hickey 2010), linguistic savants (Smith and Tsimpli 1995; Smith *et al.* 2011); genetic language disorders (Marcus and Fisher 2003; Fisher and Marcus 2006) and agrammatism (Grodzinsky and Amunts 2006); neurology of language, including expressive and receptive aphasias, imaging and the electrical activity of the brain (Stemmer and Whitaker 2008); studies of split brain patients (Gazzaniga 2005), comparative ethology and evolution (Christiansen and Kirby 2003a); mathematical modeling and dynamical systems (Nowak, Komarova, and Niyogi 2002; Niyogi 2006), language and mathematics (Dehaene *et al.* 2007), etc.

In recent years there have been a number of conferences, workshops and summer schools focused on the biolinguistic perspective. In 2001 Henk van Riemsdijk and Riny Huybregts organized a stimulating interdisciplinary conference on *The Genetics of Language* at Tilburg University,

The Netherlands, to bring together researchers from many areas of biolinguistics (Jenkins 2004). More recently there was a conference *Biolinguistics: Acquisition and Language Evolution* (University of York, UK, July 2–4, 2008) and a course *Of Minds and Language: A Conversation with Noam Chomsky*, 2006, 25th Anniversary of the European Summer Courses of the Basque Country (Piattelli-Palmarini, Uriagereka, and Salaburu 2009). There was also a workshop on *Advances in Biolinguistics*, held at the 44th Annual Meeting of the Societas Linguistica Europaea (September 10–11, 2011).

In addition, every two years a conference focused on issues of evolution, the International Conference on the Evolution of Language (EvoLang) has been held at various locations (Edinburgh, London, Paris, Cambridge, MA, Leipzig, Rome, Barcelona, Utrecht); for coverage of EvoLang 8 at Utrecht, see Balter (2010). EvoLang 9 was held at Kyoto (March 13–16, 2012, <http://kyoto.evolang.org>). More information on previous conferences is available on the conference websites archived at www.ling.ed.ac.uk/evolang.

Another important development for biolinguistics was the founding by Cedric Boeckx and Kleanthes K. Grohmann of the *Biolinguistics* journal, a peer-reviewed interactive online journal devoted to articles, interviews, and editorials on current issues in biolinguistics (www.biolinguistics.eu) (Boeckx and Grohmann 2007). Since the journal was established in 2007, the subscribed base has grown to over 2,500 readers.

In addition to the journal, information on current conferences and other news items of interest to biolinguists may be found on the *Biolinguistics* blog (<http://biolingblog.blogspot.com>, @biolinguistics on Twitter), set up by the journal editors and maintained by Bridget Samuels, Hiroki Narita, and Txuss Martin. In addition, a Biolinguistics Workshop (in conjunction with the Special Interest Group (SIG) on Biolinguistics of the Linguistic Society of America) was organized at the 2012 LSA Annual Meeting (Portland, January 5–8). This SIG is coordinated by Kleanthes K. Grohmann and Bridget Samuels. Additional information relevant to biolinguistics, as well as for topics outside of biology and language, may be found on the LINGUIST List (<http://linguistlist.org>).

In addition, the *International Biolinguistics Network* (IBN) was organized by Anna Maria Di Sciullo and colleagues to encourage collaboration between groups doing research on biolinguistics. The IBN (www.biolinguistics.uqam.ca/) provides information on ongoing projects, conferences, and links to other resources in biolinguistics (Di Sciullo 2010). Several conferences have been arranged under the auspices of the IBN: the *Biolinguistics Network Inaugural Conference*, University of Arizona, Tucson (February 22–24, 2008) and *The Language Design*, University of Quebec at Montreal (May 27–29, 2010). Two other conferences were held earlier: *Biolinguistic Investigations*, Santo Domingo, Dominican Republic (February 23–25, 2007) and *Biolinguistics: Language Evolution and Variation*, University of Venice (June 2–4, 2007) (Di Sciullo et al. 2010; Di Sciullo and Boeckx 2011).

I have mentioned here only a few conferences that have an explicit focus on interdisciplinary perspectives on language and biology. However, many, if not most of the ongoing conferences in linguistics have an implicit biolinguistic component, even it is not always immediately transparent. As Fitch (2009b) put it “I have been working in ‘biolinguistics’ (without knowing it) for the last 15 years, since my decision as a young marine biologist to refocus my efforts on the evolution of language” (p. 286).

When one considers the progress that has been made in the field of biolinguistics, as documented here in the *Handbook of Biolinguistics*, the prediction that Chomsky made over a quarter of a century ago, has been amply justified:

The study of the biological basis for human language capacities may prove to be one of the most exciting frontiers of science in coming years.

(Chomsky 1976)

Nor has the excitement abated, as the next generation of biolinguists tackles the new problems posed about the biological foundations of language.

3

Biolinguistics yesterday, today, and tomorrow

Massimo Piattelli-Palmarini

3.1 Introduction

Much too generously, Noam Chomsky (Chomsky 2007c, 2009) credits me for having coined the term biolinguistics, and for having organized, in May 1974, at Endicott House in Massachusetts (Piattelli-Palmarini 1974), the first meeting in this (then) new field. He is too generous on two counts, because I did not think I was coining a new term, but rather that I was just spelling out in an obvious way what existed already, at MIT, and because Noam and Salva Luria organized that meeting at least as much as I did (Chomsky 2007c). Chomsky and Luria (a Nobel laureate and one of the main architects of modern molecular genetics) had organized a weekly seminar at MIT where biologists and linguists engaged in a constructive exchange of ideas. What could that have been, if not bio-linguistics?

Later that month, during a grand tour of private foundations in the US, trying to raise funds for the Royaumont Center in Paris, of which I was the director and Jacques Monod (French Nobel laureate and a main participant in the Endicott House meeting) the president, I learned that one of those foundations only gave grants to institutions in the US, but not to entities outside the US. I mentioned the biolinguistics venture started at MIT and the officer in charge instantly manifested great interest in it and the concrete possibility of giving a grant. I contacted Luria and suggested that a grant application be sent. It was sent and it was accepted, and it fueled research, fellowships, and meetings in biolinguistics at MIT for several years to come.¹

One episode that happened the following year is, I think, symptomatic of the early development of the field. While on a short visit of two weeks to the Department of Linguistics and Philosophy of MIT, an undergraduate student in biology came to see me and said he was greatly interested in working on biolinguistics. I suggested that he contact Luria, and he did. He told me a few days later, to my astonishment,

that Luria had discouraged him from entering the field. I then asked Luria why he had been so dismissive, and he told me candidly that no results were to be expected for many years to come, the problems were too complex, the possibility of real experiments too remote, and a young researcher was destined to be disillusioned. Luria added (in Italian, I am translating): “When you start a career as a scientist, you want to tackle a problem that has a good chance to be solved in three or four years, not in decades. It’s ok for old schnooks like myself to have a side interest in big problems, but a young researcher must not be encouraged to tread into those waters.” Being then young myself, and thoroughly enthusiastic about the new field, I was disappointed, but Luria did not budge from his pessimistic forecast. Nonetheless, soon after, he and Chomsky sent me a glowing letter of reference for Mary Louise Kean, an MIT post-doc who had done seminal work on the phonological aspect of aphasia, and who would be spending a semester in Paris, thanks to the grant I mentioned above. So, after all, young researchers could get results in a short time. I was delighted.

There was, however, wisdom in what Luria told me. In hindsight, almost 40 years later, one must confess that the development of biolinguistics has indeed been slow, though fascinating, and that it’s still an inchoate field of inquiry. Inevitably, as time went by, it has been conditioned by several major changes in biology, in linguistics, and in the difficulty in bridging the gap between them.

3.2 Biolinguistics: The early years (Lenneberg’s dream)

The pioneering monograph by Eric Lenneberg on the biological foundations of language (Lenneberg 1967) reviewed what was then known, and sketched a “dream” (Wexler 2003) of the integration between biology and linguistics. In the mid 1970s, biology was starting an exploration of the regulatory mechanisms that determine the activation and inactivation of genes. The pioneering work of Jacques Monod and François Jacob in the 1960s had startled the field by revealing that genes could be switched on or off by other genes, that one string of DNA acted as a controller of the expression of another string of DNA. For a long time, historically, genes had been conceptualized as catalysts. Indeed, when genes code for enzymes, their products, though not the genes themselves, are literally catalysts. That idea had long passed its time, in the early 1960s, but the notion that there existed genes acting as switches was indeed revolutionary (the 1965 Nobel Lectures of Jacob and Monod retrace this experimental as well as conceptual development). Moreover, Jacob had explicitly stressed the universality of the basic molecular components of all living organisms, from the fruitfly to the elephant, and the crucial role in evolution of the different ways to recombine and shuffle the fundamental

bricks. His masterful metaphor was evolution as tinkering (*bricolage*). (Jacob 1977).

This suggested to Chomsky the very idea of parametric change as a model for linguistic variation (Chomsky 2009), which was to find its way first in his Kant lecture at Stanford in 1978 and then in *Rules and Representations* (1980). The technical developments of linguistic theory in those years pointed to increasingly abstract principles, covering apparently heterogeneous phenomena in a variety of languages and dialects. The requirement of explanatory adequacy became a *sine qua non*, grounding all linguistic hypotheses onto what is naturally accessible to the child, that is, at least implicitly, on the biology of our species. A further interesting parallel between biology and linguistics in those years is to be found in what can count as the syntactic equivalent of switches, independently developed. New analyses of phenomena like syllabification, the assignment of tonic accent, and prosody (Bromberger and Halle 1994; Chomsky and Halle 1968) and affix hopping (for a later review and historical reconstruction see Lasnik 2007), steered syntactic theory away from the traditional notion of applying rules to that of highly constrained computations, activated (in a sense, therefore, switched on) by the very nature of the input. The notion of syntactic movement was progressively transformed, from an optional transformation, to a mandatory computation, driven by the need to locally assign thematic roles and Case.

As a result of this progress, the very idea that communication might have been the shaping factor in language evolution faded more and more. Growing emphasis was given to the poverty of the stimulus, internal computational constraints, modularity and innateness (for a recent update and counters to criticism see Berwick *et al.* in press). Other ways to account for language evolution seemed inevitable, but as yet poorly defined.

The study of language pathologies, notably aphasia, became increasingly relevant and researchers with a solid background in linguistic theory started having a closer look. Agrammatic aphasia became a field of inquiry and it was possible to collect evidence from pathology bearing on rather specific syntactic hypotheses. (For a later review, see Grodzinsky 1985.)

The study of sign languages from the perspective of universal grammar soon came of age (Klima and Bellugi 1979), showing that the fundamental principles of language design were largely independent of the modality of externalization.

Early studies in language acquisition contributed to divorce the spontaneous growth of language in the child from any kind of inductive trial-and-error mechanism. Remarkable discontinuities in the acquisition of syntactic processes and the identification of critical periods, uniform across children and across languages, stressed the crucial role of brain maturation, making the traditional notion of learning inapplicable. An early anthology of such studies (Wanner and Gleitman 1982) made all this

transparent. As to the genetics of language, detailed studies of the differences between monozygotic and fraternal twins for different components of language (notably lexicon versus syntax) did show a clear pattern of heritability (corroborated by the rediscovery of early studies, published in the 1930s, by Ella J. Day (Day 1932) (for a later comprehensive review, see Stromswold, 2001, 2006). The identification of language-related genes, however, was yet to come.

In view of what we will be seeing here below, it needs to be stressed that the kind of genetics on which linguists based their collaboration was of a rather traditional, Mendelian kind. In hindsight, we see that the attempt was to grasp (in Sean Carroll's felicitous expression (Carroll 2005a) "the low-hanging fruits of the tree"). That is, the one-mutation/one phenotype model, so prevalent until very recently.

3.3 The middle period (circa 1985–2000: the low-hanging fruits of the tree)

The genetic regulation of higher cells, unlike that of bacteria (the domain studied by Monod and Jacob) soon appeared to present an altogether higher order of complexity (Britten and Davidson 1969). Over and above "operator" genes controlling other genes, there were inducers, integrators, sensors, promoters, batteries of genes. Further regulatory mechanisms were to come, but it was already acknowledged that this complexity in gene regulation was bound to have decisive and innovative consequences for evolution. Regulatory elements or factors were rapidly turning into whole regulatory networks (Davidson 2006, 2010). Evolution appeared increasingly to have been the evolution of these networks. The intimate integration between the study of evolution and the study of development (later to be called evo-devo) was rapidly developing, on its way to becoming a whole new field (Raff 2000).

Linguistic theory had consolidated further and had found a comprehensive framework (the Theory of Government and Binding). The idea of parametric variation had been refined and had found several applications. A restricted number of syntactic modules had been identified and their fine inter-digititation explained the nature of distinct levels of representation, and the licensed operations on these. The detailed analysis of agrammatism revealed specific disruptions, establishing solid bridges between linguistic theory and neuroscience (Grodzinsky 2000). It was becoming more and more frequent, and more and more relevant, to find evidence pro or con specific syntactic hypotheses in brain data and in data on language acquisition (Wexler 1994). Continuing progress in brain imaging (PET, fMRI, EEG, then MEG) started producing numerous publications on brain and language, of unequal level of interest.

Basically, however, it became clear that distinct linguistic tasks were differently localized in the brain, confirming what had been known all along, but on different bases.

Language pathologies other than aphasia were discovered and finely analyzed: Specific Language Impairment, Williams Syndrome, Turner Syndrome, Spina Bifida, and the case of savants. Cumulatively, these data corroborated the notion that language is modular, both at the “large” level and the “fine” level (for an impressive recent review see Curtiss (in press)). On the front of the genetics of language, a new case opened up, that of *FOXP2*, raising both legitimate interest and exaggerated hopes (Piattelli-Palmarini and Uriagereka 2011).

The rise of the Minimalist Program (ever since Chomsky 1995b) created new opportunities for a biolinguistics concerned with more abstract properties of language and their plausible correlates in brain and in acquisition. The mandatory character of linguistic computations and of their natural constraints (criteria of no-tampering, last resort, strict locality) became evident, further corroborating an implicit parallel with the biological notion of switches and regulation. This opened the present period.

Painting with a broad brush the scene of the intermediate period, I think that we have witnessed the end of any lingering doubts about the legitimacy and the interest of exploring the biological correlates of language. The initial program of biolinguistics had become, as a matter of fact, a full domain of inquiry, with several anthologies and, later on, its own scholarly journal (*Biolinguistics*). Evidence from different sources had converged onto a better picture of modularity, maturation, and the poverty of the stimulus. The “low-hanging fruits” had been collected. A very fine summary of this period and its main successes is to be found in a book by Lyle Jenkins and in a volume edited by him (Jenkins 2000, 2004), terminating with a most illuminating chapter: a synthesis and a brief historical reconstruction by Chomsky (for a comprehensive recent panorama see Di Sciullo and Boeckx 2011).

3.4 The present period (ever since circa 2000: the higher fruits)

Biology is undergoing a further revolution. Not only is the study of gene-environment interactions becoming a puzzle of increasing difficulty, but the issue of “missing heritability” (Maher 2008; Manolio *et al.* 2009) lingers and is, for the time being, unresolved. This means, in extremely simple terms, that the classical Mendelian picture of one gene/one phenotype is rather the exception than the rule, and that, typically, the identification of dozens of genes related to a disease (the best studied case, for obvious reasons) only accounts for 3% or 7% of the genetic risk for that disease.

No one has doubts that there is a genetic component to the disease, but the complete picture of the interaction is presently “missing.” The key appears to be several complex interactions between common variants and rare ones for each of those genes, and how those genes interact with the rest of the genome.

Over 25 genes have been identified already as being, in one way or another, linked to language, some of which regulate other genes also linked to language (as is the case of *FOXP2*). Moreover, epigenetics is actually a field in full expansion, and it’s far from evident how this will one day connect to linguistics (Vercelli 2009). Collecting the higher fruits of the tree seems to require a significantly greater effort than ever imagined.

The panorama of evolutionary theory is slowly but significantly moving away from classical neo-Darwinism (Fodor and Piattelli-Palmarini 2011), though there is great resistance to admitting that it is so, and this momentous change, destined to accelerate in the years to come, is conveniently and prudently presented as an “extension” of the classical theory (Pigliucci 2009).

The increasing success of evo-devo suggests entirely new ways of looking at the evolutionary relation between genes, mostly paying attention to regulatory genes, across species and phyla (Carroll 2005a). The invariance of the fundamental bricks in evolution goes way beyond what Jacob had intuited, now covering whole developmental and evolutionary modules that we find highly conserved even across distant species and phyla (Schlosser 2004; Wagner 2005). A consequence is that, in the domain of language, as in other domains, it is perfectly legitimate, and often very productive, to compare genes, phenotypes, and patterns of regulation in distant species. For instance humans and songbirds for the development of the control of complex vocalizations (Scharff and Haesler 2005; White *et al.* 2006), just as is the case for vertebrates and jellyfish for the development of globular eyes (Piatigorsky and Kozmik 2004). Moving away from the poor analogies between human language and the painful learning of sign languages by chimps, surely close relatives, we may want to explore more productive comparisons with songbirds, of which we are only distant relatives.

In fact, one consequence of the modularity of development and the modularity of mind, is that one module, once activated, may well remain encapsulated, regardless of its utility for the species, and regardless of the lack of other organs that may profitably interface with it. The many complete globular eyes of the box jellyfish encounter no optic nerve and no brain, though they each have a lens, a cornea, and a retina. The genetic explanation of what has happened in evolution to produce this anomaly is well reconstructed. Further surprises of this kind are likely to come from evo-devo. Evolution and development will probably be better understood in the future one module at a time.

Brain imaging has reached greater sophistication and new methods are available (such as MEG and near infrared imaging). We may, thus, hope to go beyond the brain confirmations of long-held hypotheses, interesting as these confirmations are. It has been reassuring to ascertain, via separate brain activations, that real syntactic computations are different from superficially similar ones, only possible as an abstract exercise (Musso *et al.* 2003) and that real sentences, as well as Jabberwocky sentences, activate brain areas that are quite distinct from those activated by a list of words (Pallier, Devauchelle, and Dehaene 2011). In the future, it will be even more interesting to learn, perhaps, that linguistic tasks about which we are not so sure (deriving a phase versus a nonphase) are indeed different, or are not, at the brain level. But, just as only the revolution in the more basic science (physics) has once produced a revolution in the less basic one (chemistry), so we can expect that only a revolution in the neurosciences will produce a major one in biolinguistics. No one knows at present why the different brain modules, whose existence leaves no doubt, do different things. All we can do at present is to link, phenomenologically, one brain region to a cognitive task, but that's where the buck stops. Unlike, say, the eye or the kidneys, whose different functions can be seen directly in their different structure, the brain looks to us much the same everywhere. Surely this is not the case, deep down, but we do not know why. Possibly the key will be the synchronicity of signals, phase slippages, selective group coordination, micro-cascades of excitations, or the reverberation of quantum phenomena inside neurons. The younger generations will one day know.

On a different front, quite encouraging are, in my opinion the studies of the abstract invariants of language, at different levels, from the syllable to phases. (Medeiros, 2008; Piattelli-Palmarini and Uriagereka 2008). These are the “third factor” in language design (Chomsky 2005), more akin to a physico-linguistics than to biolinguistics. The road to travel is still long, but we have the impression of having opened a Pandora’s box. More and more, such deep regularities, instances of optimization, do appear in language as we look into it (Medeiros 2012; Uriagereka 2012). Synergistically, though for the time being mostly independently, deep invariants and instances of optimization emerge also elsewhere in biology (Cherniak *et al.* 2004) (for a brief census, see Part 1 of Fodor, and Piattelli-Palmarini 2011 reviving a tradition of search for structural invariants that dates back to D’Arcy Thompson).

A question comes to mind, when witnessing that so much of biological structure is derived “for free, from physics” (as Chris Cherniak likes to say) and that there is so much that is explained by a “non-genomic nativism.” The question, simply and radically put, is: why do we also need DNA and proteins? This is a very hard question to answer, for anyone. In fact, one of the modern pioneers in this field, Stuart Kauffman,

acutely says (Kauffman 1993) that “no body of knowledge” today can combine the physical and computational deep invariants of biological systems with the mechanisms of genetics and development. Kauffman’s characterization is important: no body of knowledge does it, a much more serious default than that of just lacking a theory or hypothesis, or even a whole discipline. The future generations, bless them, will one day fill this great gap.

3.5 Design for a curriculum in biolinguistics

Speaking of future generations, when hopefully biolinguistics will turn into a domain such that BS or BA majors will exist in some universities, I will indulge in designing a curriculum, in broad terms. Most of these courses will have to be created from scratch, as the telegraphic description I am giving may make clear. So, it’s an ambitious plan, as the new discipline requires.

3.5.1 Basic courses

Introduction to language. Covering the main components: phonology, morpho-lexicon, syntax, and semantics. With special emphasis on invariants, universal grammar, and parametric change.

Introduction to biology. With special emphasis on common Baupläne, gene regulatory networks, developmental, and genetic modules.

Introduction to computational mechanisms. Quite different from the standard computer science basic courses (where students are mostly taught to program in Java). Mechanisms for divide-and-conquer and recursion; memorization (remembering the results of repeated computations); dynamic programming and data organization (for efficient access and search, indexing, graphs); parallelism (process communication, embarrassingly parallel problems, map-reduce); functions and their inverses (one-to-many, many-to-one, one-way, cryptographic basis); finite state machines, stacks, tapes; Turing computability (how to think about these mechanisms or building blocks). Key relevant concepts include: determinism/non-determinism, computational complexity, etc.

Introduction to physics. Basically, the physics needed to understand the nervous impulse, and the principles of functioning of MRI, fMRI, MEG, EEG, and near infrared.

Basic calculus. The analysis of functions, elementary differential equations, maximization and minimization, variational calculus.

Introduction to logic. Sets, operations on sets, propositional calculus, predicate calculus, quantifiers, scope.

3.5.2 Intermediate level courses

Neurobiology. Special emphasis on the modular evolution of the nervous system. Synaptogenesis, stability and instability of neuron networks, interactions of synaptic potentials. Neuroimaging techniques.

Human genetics. History of the domain, from inborn errors of metabolism to the present scene. Special emphasis on the different kinds of genotype-phenotype relations and on gene-environment interactions. The study of twins. Epigenetics, the case of missing heritability, main techniques of gene identification and of estimating variation, common variants versus rare variants, estimates of genetic risk.

Brain imaging. The different techniques, their advantages and their limitations with special attention to language-related data. Critical evaluation of the statistical analyses adopted and of the conclusions they allow or do not allow.

Language pathologies. Various kinds of aphasia, SLI, Williams Syndrome, the case of savants, Turner Syndrome, Spina Bifida. All these cases analyzed in the light of precise linguistic hypotheses, as evidence pro or con such hypotheses.

Linguistics. Intermediate courses, with special emphasis on syntactic theory and the semantics of natural language.

Language acquisition. With special emphasis on the relation between linguistic hypotheses and longitudinal data on acquisition, in several languages.

3.5.3 Advanced courses

Theories of evolution. A critical historical panorama of evolutionary theories, from early Darwinism to neo-Darwinism and the modern synthesis. Evo-devo and its consequences. Modularity, robustness, and evolvability. The search for the laws of form, deep invariants, instances of optimization.

Neuroscience. Principles of brain evolution. The physics of neurons and neuronal networks. Comparative ontogenesis of the brain across phyla. Ablation, regeneration, gene regulatory mechanisms. Computer models of brain modules.

Philosophy of linguistic theories. Critical historical reconstruction of the central concepts in linguistic theory. Representations, derivations, constraints, locality. Principles and parameters yesterday and today. Formal analyses (the hierarchy of automata) versus virtual conceptual necessity. Explanatory adequacy and beyond.

Quantum physics. Room at the bottom, inside the neuron. Microtubules and quantum coherence. Fundamentals of quantum computation and its biological correlates.

The mathematics of life. Fractals, attractors, strange attractors. Fibonacci series. Bifurcations, instabilities, pattern formation. Scale invariants. Biolinguistics. Bringing all this together. Revisiting the canonical cases with a new eye. Classics of biolinguistic inquiry, old and new problems, puzzles, and mysteries.

4

The philosophical foundations of biolinguistics

James McGilvray

4.1 What is biolinguistics?

There are various ways to study how language is related to biology. Few, however, proceed on the assumption that language is a biologically based “organ” inside the head of humans that perhaps results from a single mutation, an organ that develops automatically in normal humans to – apparently – yield a virtually “perfect” solution to the issue of how to take “atomic” concepts and combine them to yield the elements and structures of complex thoughts. This is the approach of the minimalist research program of Chomsky and others. Its ultimate success is, of course, an empirical issue. But it is by no means an outré program. It is a reasonable naturalistic and mentalistic research program, representing the most advanced study of a part of the mind in a successful line of naturalistic, internalist, and nativist research programs (Chomsky 2007a; Chomsky and McGilvray 2012; Jenkins 2000).

Before trying to speak to what the philosophical foundations for biolinguistics are, it is important to specify what kind of “entity” biolinguistics is. It is a research *program* that encompasses several specific lines of research, all of which proceed on the assumption that human language is, at least in significant measure, a domain-specific biologically based system, a cognitive “organ” of the mind that interchanges specific forms of “information” with other mental systems. This research program in turn proceeds within the scope of a justified research *strategy* for the study of mind and its various components, including language. And this strategy is in turn again a variety of a proven naturalistic scientific *methodology* that is appropriate for all forms of naturalistic research. These levels of description of scientific study move from the more specific to the more general, but even at the most general level – that which constitutes a methodology for constructing a (natural) science – they do not by any

means encompass the full range of human cognitive capacity or, indeed, what is for most people now and an even greater majority before around 1600, the only cognitive capacity they need deploy. Common sense suffices for getting along in the world, and by far the great majority of people was, is and probably will continue to be content to employ common sense and its ways of understanding the world alone, however different the view of the world they yield might for theoretical and explanatory purposes be. Indeed, for most of their lives, naturalistic scientists default to these commonsense tools too.

In the following, I speak to the issue of what the philosophical underpinnings of biolinguistics are in reverse order. That is also the chronological order. Natural science as we understand it had its beginnings early in the seventeenth century with the work of Galileo, Descartes, and to an extent Francis Bacon and others. Others might wish to trace science back to Leonardo da Vinci or John Bacon, perhaps even earlier to Aristotle, the atomists, Plato with his mathematization of substances and the world order, and the Pythagoreans who wanted to geometrize the world. It was not until the seventeenth century, however, that the kinds of explanation offered by natural science began to be possible with the quantification and formalization in theories of natural processes and the rejection of final causes.

In detailing the origins of natural science methodology, I focus on Descartes. Galileo's contributions to methodology were significant too, but not – I believe – as systematic as Descartes's, nor as far-reaching. There is another reason to focus on Descartes. Unlike Galileo or any other early “natural philosopher,” he also made a significant contribution to the strategy to adopt in studying the mind, especially with regard to language. And he even made suggestions concerning “innate ideas” that suggest adopting what became a biological research program. So his work serves to unify the discussion. I should remark that I do not see my task as trying to faithfully represent Descartes's actual views: while I think I can show that he did hold what I attribute to him, he was also a creature of his time and held views about the mind, language, and method that should be abandoned. These include his mind–body substance dualism, which was, however, a reasonable view to maintain, given the options Descartes had available.

4.2 The methodology of naturalistic research

As suggested above, human cognitive (“problem-solving”) capacities appear to fall into two general areas – commonsense understanding on the one hand and what Chomsky calls “science formation” (constructing sciences in accord with a naturalistic methodology) on the other (Chomsky 1975, 1995a, 2000a, 2007b). All humans mobilize the first kind of capacity;

they must, in order to survive. It is rather easy for them to do so because humans who exercise commonsense understanding can rely heavily on a rich and apparently innate resource of concepts such as PERSON, TREE, STONE, RIVER, TABLE and many thousands of others, plus the innate combinatory resources of a natural language.

These concepts are of little to no use in advanced formal sciences. Formal sciences are, of course, the kind of sciences the biolinguist aims to construct. They cannot employ the concepts of common sense. That is because these seem to be designed to serve human anthropocentric interests, not objective description. That provides benefits, of course: concepts such as these allow for acceptable and reasonably swift and interest-sensitive solutions to the problems of our practical (sometimes called “moral”) life – those of walking the dog, constructing states and other social institutions, and so on, plus those of assessing the value of cooperative and individual actions, of economic distributions, of art – in general, virtually everything having to do with what it is to be an agent or actor and an assessor of actions and the results of actions. They do not provide for the objective descriptions and explanations of events and “things” that one hopes to offer in the sciences.

From this perspective, the social sciences are not among the natural sciences, for they continue to use at least the central concepts of what is sometimes called “folk psychology,” such as belief and desire. Efforts to formalize and quantify applications of concepts such as these do not yield context-independent, frame-independent, and non-anthropocentric descriptions and explanations in the way that applications of ELECTRON or HOX GENE do, however much one might want them to.

The distinction between common sense and science, and the need to make it, goes back to the beginnings of the scientific revolution as we understand it. The distinction and reasons to maintain it are found in one of Descartes’s major works on what he calls “the method,” specifically, his *Discourse on the Method of Rightly Conducting the Reason, and Seeking Truth in the Sciences*, first published in 1637.¹ In the first paragraph of the main text he notes that what he calls “bon sens” or “the power of distinguishing truth from error” is available to all. To be available to all it must, of course, rely on innate (readily available/readily triggered) concepts (for Descartes’s plausible view of innateness, see below). That is necessary to allow the concepts/ideas to be largely shared, without undergoing extensive training or education. Judgments presuppose the capacity to distinguish truth from error, and to be able to judge is to be able to put concepts together in sentential form and – adding to these “forces” such as assertion – to make judgments.² The capacity to distinguish truth from error is revealed in employing judgments that are appropriate to the circumstances in which they are applied. As the discussion of the use of language at the end of Part V of the *Discourse* clearly indicates, these circumstances include discourse (and surely coherent thought) circumstances, not just those present to

hand. It is this complex capacity to employ language to put together and make judgments, apparently, that comes “for free” to everyone. The results are context-dependent and sensitive to human interests and sensory capacities, but still truths.

This readily available capacity and its exercise cannot, though, be what is involved in science, for in Descartes’s view and in ours, scientific concepts and the capacity to make scientific judgments by relying on the resources of a theory are by no means readily available “to all.” If they were, Descartes would have recognized that the method that he was so pleased to have developed, a method that he believed to reliably through the construction of sciences (scientific theories) and their concepts yield interest-independent objective truths about natural events, not merely practical truths, was already not just readily available to all, but often employed by at least some. Indeed, if it were readily available, it would have to be because concepts such as MUON and the combinatory operations of the standard model of elementary particle physics (or of optics, to take an example of an area in which Descartes made a contribution) were innate and readily activated, and the theories in which they are lodged readily acquired. That is, of course, not the case at all. It took centuries of efforts to make progress to reach the point we have even in the advanced science of physics, a science that is still far from complete. Perhaps the capacity to do science *after* or *in* conforming to “the method” is available to all; that is, by honoring the method, anyone can come to develop reliable theories. But the method was definitely not followed by very many and at the time there was little sympathy for its results, which were almost universally rejected by stalwart defenders – clerics, philosophers, politicians – of the misconceptions about what is “out there” that common sense can lead to. (Note that often still one finds defense of commonsense notions of mind, and especially language, in the work of various varieties of cognitive scientists.) None of this is surprising: commonsense notions are attractive because they are readily understood and applied (not readily defined, but readily understood). And as noted above, humans can and have survived and sometimes thrived for millennia without any more insight into the world and other people and their minds than that afforded by unaided common sense and its innate concepts and flexible and often appropriate judgments.

What, then, is the method as Descartes saw it? I begin by outlining what I suggest are the distinctive features of the method of naturalistic scientific investigation as understood now, and then return to Descartes (and Galileo, where appropriate)³ to point to strains in his thought about method and its application that could plausibly have developed into current understanding of scientific method. I ignore Francis Bacon entirely, for while he was influential in developing something that these days is often associated with John Stuart Mill (“Mill’s Methods”), his thought on the matter was less comprehensive than what is found in Descartes, and he

did not place Descartes's emphasis on employing formalization (mathematization) and seeking simplicity, coherence, and comprehensiveness in the construction of what we would now call empirical theories. In ignoring Bacon (and Mill), I am also going against what in the popular mind these days is thought of as "the scientific method." For many, that method is fairly straightforward: observe, note contrasts and similarities, construct hypotheses/models, test ... Something called "induction" is given a central place, even in the task of producing hypotheses, for which (as Charles Saunders Peirce emphasized) one really needs "abduction" instead. Generally, while something like Bacon's and Mill's prescriptions might have roles in (say) developing generalizations that a theory can explain and in gathering data and evidence in a fair and statistically adequate way, they say little substantive about the nature and role of theory and fail to address central matters of theoretical adequacy. They do little but point in the direction of seeking good observational evidence, posing problems for theory to explain, and offering reasonably reliable observational generalizations.

We can describe the method of naturalistic scientific theory-construction by outlining the desiderata for a naturalistic theory – what makes a theory a good one. Natural scientists when constructing theories are engaged on reaching a reasonably well-understood and uniform – although not necessarily articulated – set of goals. Listing these goals indicates what such a scientist is looking to construct/discover when they engage in constructing and providing evidence for a set of hypotheses/theory. By outlining those goals, we can also – given the theory's subject matter – come to understand what a scientist must do in order to satisfy the desiderata. In compressed form, I suggest that the relevant aims/desiderata are these: to seek descriptive and explanatory adequacy; employ explicit, formal symbols; aim towards simplicity in the theory and look for it in the theory's subject matter; ensure objectivity in the theory and its characterization of its subject matter (avoid anthropocentricity, perspective-dependence, and dependence on context); aim your efforts at constructing a theory towards ensuring that it can be accommodated to other relevant – and usually more basic – theories that bear on your subject matter; and finally, never assume that your theory is complete and – in connection with that – ensure that your efforts at constructing a theory make progress in better satisfying one or more of the desiderata mentioned above.

Descriptive adequacy and explanatory adequacy need comment. In any mathematical/formal science and/or a science that deals with infinite domains (such as grammars, or theories of language) descriptive adequacy has to do with adequately enumerating and describing all the "elements" that constitute the "things" or "events" of the theory's domain and assigning them the "right" properties or features, where none of the properties or elements need be observable in any everyday sense. Natural sciences in

advanced forms are generally speaking postulational sciences – that is, their theories postulate or hypothesize an unobserved system of entities with various theory-assigned properties or features. It is these entities, structures, properties, and features that need to be adequately described. No doubt one of the tasks for the postulated system(s) is to in some way “account for” relevant observations and evidence. They might do so by indicating how microstructures in various states reliably lead to specified observed phenomena, or by indicating how sequences of events linked by laws within an unobserved microsystem reliably lead to changes in observed phenomena. (By “observed” and “observable” I have in mind what the human sensory systems and the deployment of commonsense concepts accomplish and can accomplish. That suffices for current purposes.) In the case of the science of language, one set of observations a postulated system should account for is the poverty of the stimulus observations; another is the creative aspect of language use observations.

As for explanatory adequacy: it includes much more than yielding reliable predictions, even where it is a causal theory at stake. It can include ensuring that puzzle cases are resolved, clarifying the bounds of a theory (its assumptions about background and boundary conditions), identifying and adequately speaking to “crucial experiments,” suggesting fruitful ways to look for evidence and engage new data, attempting to say why things are the way they are, reconciling differences with other theories, ensuring coherence, dealing with “global” matters, and the like. That is a very full slate, but as a rule, coming up with a good answer to one issue can lead to coming up with a good answer to others. Sometimes, however, focusing on a single difficult issue and providing an inadequate account can block consideration of other issues.

To illustrate both of these points from the science of language: while coherence and an effort to deal with puzzle cases, among other factors, played roles in the *Aspects*' (Chomsky 1965) view of explanatory adequacy, the core of the explanatory adequacy issue was seen at the time to be that of coming up with a solution to the grammar acquisition issue in a way that made sense of the poverty of the stimulus observations. There is nothing unusual in this, as indicated above: any natural scientist tries – typically by postulating a hidden system or structure – to “explain the appearances,” which in this case amount to the facts of acquisition, the poverty of the stimulus observations. Chomsky's (1965) proposed solution to that problem, a proposal found also in his earlier work (Chomsky 1951/1979, 1957, 1955/1975), employed an “evaluation” procedure that “chose” between alternative grammars for the input to a child's mind (that is, linguistic experience) by appealing to a postulated innate mathematically based measure of the “internal” simplicity of a theory/grammar. The postulated measure yields an “evaluation procedure,” a way to assess the relative complexity of two grammars to which the measure is applied. Like many proposals in the early days of a science, it was the best available at

the time, all things considered. But it also cried out for a better solution. Not only did it do only a fair job of making sense of the observed poverty facts, but because it made it difficult to see how to deal with other explanatory matters, it blocked efforts to deal with them.

Remember that the aim of the scientist of language is to satisfy as well as possible the goals of the natural scientist. Arguably for Chomsky from the early 1950s, and certainly for him (see, for example, Chomsky 1972b/2006, originally published in 1968) and for Eric Lenneberg and Morris Halle from the mid 1960s on (Chomsky 2007a; Chomsky and McGilvray 2012), it was apparent that the study of language would have to somehow be coordinated with the study of biology. If there were to be an adequate account of the poverty of the stimulus facts, that account would have to somehow be based on an account of biologically based growth and development. Coordination with biology would help provide answers to other fundamental explanatory questions and also provide a route to accommodate the science of language to another science, to make the science of language objective, and better meet other methodological desiderata for natural science. You can, for example, get at least a partial answer to the issue of why language is the way it is if you have available a defensible account of what language is (descriptive adequacy) and language's evolution. "God did it" might have satisfied Descartes (although I doubt it really did), but no natural scientist since should be satisfied with that answer, at least in his or her working life. Yet a defensible account of language evolution (and not merely a just-so story) appeared to be well out of sight in the 1960s because the solution to the acquisition question offered in Chomsky (1965) and earlier depended on assuming a rich and complex biologically based innate "format" for any possible language. You need something like that to – at the very least – provide a way to make sense of how the child's mind manages to construct alternative grammars to which an evaluation procedure could apply. The format's richness and complexity, however, made it all but impossible to conceive of how the acquisition of language could depend on an evolved biological growth procedure. For how could evolution within a single species without precedent in homologues or even analogues come to yield a genomic specification for any one of many languages conforming to a complex and rich format? And the matter was complicated further by the fact that – as Lewontin (1998) points out – no evidence for the gradual evolution of language was available, and given what was known about the evolution of the human species which uniquely has language (Hauser, Chomsky, and Fitch 2002), it appeared that it was very unlikely that any could be found. There was no plausible answer to the issue of how a complex format could have evolved, nor, as a consequence, were there answers to related questions, such as why language is the way it is. To even begin to conceive of how to get an answer to the evolutionary issue of a sort that made sense of what was known about the evolution of the human species, and the human species alone,

biology's (the genome's) contribution to grammar growth (to the development of a grammar in a child's head) would on the face of it have to be greatly simplified, and ideally, reduced sufficiently that language – or at least the basic combinatorial system – could have come to be introduced as the result of a single mutation in a single humanoid individual.

One key to opening the door to coordination with biology and to dealing with other explanatory issues came in the early 1980s with the Principles and Parameters picture of acquisition (Chomsky 1981a, 1986b). That picture made options available for alternative grammars where these options were seen as subject not to a relative evaluation procedure of already available grammars, but to an innate and automatic “selection,” perhaps even “decision” procedure. In effect, the child's mind (not the linguist's) has available to it a “mechanical” way of providing what in the early days of Chomsky's work was called a “discovery procedure” that (quickly) creates a grammar from a set of inputs or data. The set of grammatical options is conceived to be fixed (perhaps not just in UG, but also in other nature-based “formative” factors constraining growth that need not be under genomic control), and acquisition is portrayed not as a construction and comparison procedure, but as merely a procedure of setting “switches” or toggling between fixed options. The child's mind does not hypothesize alternative grammars, but just grows a single one. A second key was provided by the minimalist program's insistence on meeting solely “bare phrase structure” requirements and eliminating all “artifacts” such as DS and SS from grammars, making grammars as simple as possible (Chomsky 1993, 1995b). The apparent need for a complex architecture for grammars disappeared. A third key came with the introduction of Merge and Move as the sole grammatical operations and – thereafter – further simplification with the inclusion of Move within (Internal) Merge accompanied by the “copy” account of movement, which isn't movement at all, followed then by phases (Chomsky 2001, 2002b, 2008). There were other keys too, but the ones mentioned suffice for illustration.

There were also apparent obstacles. For one, the introduction of micro-parameters and the possibility that there are an infinite number of them appeared to complicate the descriptive task and made a “switching” procedure involving only a finite and preferably small set of parametric options look unrealistic. Two proposals contributed to overcoming this block, and doing more besides. First, parameters came more and more plausibly to be seen as restricted to the computational path that led to the production of sound/sign (essentially, to “linearizing” an expression in such a way that it could be presented in temporal sequence), and not involved on the path to the “semantic interface,” allowing – in principle – for a uniform computational procedure for all I-languages on the path to the semantic interface, if not the phonetic one. That allowed for the possibility that evolution of the combinatorial system of language began with the introduction of the capacity to generate complex thoughts, with

this capacity amounting only to a simple form of recursion like that found in the natural number system. Links to sound/sign might have come later. And in a connected vein, second, it allowed for the possibility that parameters (including non-finite scalars?), or at least most of them, could be removed entirely from the genome (UG) and all located instead in “third factor” (Chomsky 2005) constraints on growth. Taken together, these proposals placed the apparent complexity and diversity of language in the externalization or linearization path and removed the apparent need to clutter UG with a large number of options. It was also congenial to some insights of the growing biological science of evo-devo (Carroll 2005b, Fodor and Piattelli-Palmarini 2011; see also Waddington 1940, 1942 and his views on canalization). And it allowed for the possibility that UG consists in Merge alone. If so – and also by placing the acquisition of phonological/phonetic features and semantic ones too outside the scope of the job UG had to perform – the evolution of language could be conceived to have occurred as the result of a single mutation in a single individual that was capable of producing progeny with the mutation intact, with that mutation consisting in the introduction of Merge and thereby a very simple but apparently human-unique version of recursion (Berwick and Chomsky 2011; Chomsky and McGilvray, 2012; see also Hauser, Chomsky, and Fitch 2002; Chomsky 2007a,b,c; for a dissenting view, see Pinker and Jackendoff 2005) that in the first instance joins concepts to each other to yield thoughts or what Chomsky (1995a, 2000) prefers to call “perspectives.” Recursion leading to these complexes of “semantic information” affords considerable survival value, of course, although that is of little interest as such. Selection does not build structure; it merely notes the continuation of a structure made available by mutation. And perhaps the recursive procedure central to language’s combinatory capacity is somehow “anticipated” or allowed for in biophysical options (Thompson 1917/1942/1992; Turing 1992; Fodor and Piattelli-Palmarini, 2011).

The saltational account of the evolution of language that this story allows for might even – unlike virtually every other story told about the evolution of language and more generally cognitive capacities (Lewontin 1998; one of his targets is Pinker and Bloom, 1990) – prove to be one for which one can find evidence in (among other things) archaeological and anthropological studies. Obvious examples include various pieces of evidence of “the great leap forward” (as Jared Diamond called it) in human cognitive range. Plausibly, the extraordinary degree to which humans enjoy cognitive advantages compared to their close primate relatives is due primarily and perhaps exclusively to the introduction of language, or at least recursion over concepts, which appears to be unique to humans.

Other explanatory issues that can now at least begin to be addressed include the “design” issue and why language appears to be a “perfect” (or at least nearly perfect) answer to it. There is also the issue of “words” and why they have the “edge” characteristics that they appear to have

(Chomsky 2008). Even just opening up ways to begin to address these and other issues represents progress of the sort that one hopes for in constructing a natural science – quite remarkable progress in a science of language that has been in place for only a little more than half a century, I suggest.

Returning now to the other desiderata for a (natural) science and not illustrating in anything like the detail found above, formalization or insisting on employing formal, explicit theoretical “language” in a theory should need little comment. It is a general characteristic of all well-developed mathematized theories and it is often connected with matters of quantification of values. But it is more than that. In the case of grammars, it has little to do with quantification, but a lot to do with generativity; and one reason lexical study is still in its infancy is that we lack a reasonably good account of the theoretical terms to employ when dealing with semantic features.

Then there is *simplicity*, which is a recognizable trait in a theory, although the subject of much discussion because it is an elusive desideratum of scientific methodology. It appears in several forms, some examples of which appear above: elegant formalization, efficiency and effectiveness, minimizing primitives, efforts to eliminate extraneous artifacts, computational minimalization, and the like. While some minimalist program practices already mentioned reflect some of these, it is important to remember that both “theory general” simplicity and what Chomsky in *Aspects* called “internal” simplicity have played prominent roles in grammar construction efforts for decades. Similar strains of thought are found in Chomsky’s (1955/1975) *Logical Structure of Linguistic Theory*, in different form in Chomsky (1957), and in different form again in his very early Goodman- and Quine-inspired efforts to explore the prospects of nominalism, now on a distant back burner. Improvements in simplicity (of the relevant sorts) are signs of a theory making progress. That is actually quite a surprising fact: there is no guarantee that nature itself is simple.

The demand for *objectivity* is, or should be, obvious: eliminating appeals to anthropocentric concerns has been a central feature of efforts to construct theories from the beginning of the scientific revolution. It can mislead – as, for example, with Frege and his assumption that the right way to ensure objectivity (as with what he called “senses”) is to make them into abstract objects. Arguably, it mattered little in his case because he was constructing a semantics for mathematics, not for a natural language. It would be a much more serious mistake to assume that it would be an appropriate way to ensure objectivity in constructing a naturalistic theory of language, taking this (and its meanings) to be a natural object.

As for *accommodation* to other theories, I use Chomsky’s (1995a, 2000a) term “accommodation” to – as he does – employ a term that encompasses various specific ways to relate one theory and its primitives and principles to another. Reduction is one such relation, but only one. Accommodation in some form has long been a desideratum of science; a twentieth-century

example of how important it is found in the effort to launch and carry out a series of publications beginning in 1938 with Charles Morris's *Foundations of the Theory of Signs* and later including several central texts in the philosophy of science. The motivation for the series is obvious in its title, the *International Encyclopedia of Unified Sciences*. Accommodation is clearly related to other desiderata, such as simplicity and explanatory adequacy, but it also deserves independent mention.

Finally, there is *progress*. This – progress in one or more of the desiderata mentioned above – is a hallmark of a good theory. It distinguishes theories from conjectures following conjectures with no evident progress in the one that follows another, and distinguishes it too from virtually all philosophical and like “theories.” Progress of the relevant sorts is a sign that a theory does to at least a degree “cut nature at its joints.”

With what is, I hope, a reasonable sketch of the methods and desiderata of naturalistic research as conceived today in mind, I return to Descartes. The *Discourse* compresses Descartes’s “method” into four rules. The first insists that one never accept as true anything that does not meet a test of certainty, where the test consists in allowing nothing but that which “presented itself to my mind so clearly and so distinctly that I had not occasion to doubt it” (Descartes 1984–5, vol. I: 120). This passage has received a great deal of highly critical attention, and usually for good reason: it has little merit as a general principle, and is presented as a subjective psychological demand, not one that somehow constrains the workings of the human mind. Looked at in the context of how to go about constructing a theory, though, one can make sense of it. Certainty has to go; that is irrelevant to science. But clarity and distinctness make some sense when thought of as desiderata for scientific theory-construction, where they can be seen as mind-imposed (not personally or subjectively imposed) demands to break things down into “simples,” employ precise formalization, and seek simplicity in theories. Perhaps something like this is what Colin McGinn (1993) has in mind in his CALM principle (combinatorial atomism and lawlike mappings). He proposed it as a limitation on human (scientific) thought – although he is after quite different targets.

The demand to satisfy “clarity and distinctness” does not typically generalize outside of science. Trying to satisfy these demands in the common-sense domain typically distorts. As Wittgenstein asked, what do you “analyze” a broom into? A handle and a brush? How about each bristle/straw/whatever makes up the brush? Also the binding material? Do any of this “analysis,” and the broom and its well-understood functions – ones that virtually define what it is to be a broom – disappear? In contrast, “analysis” works, and is needed, in the sciences. Similar points apply to Descartes’s view that in science one be guided by the “light of nature”; this is much like Galileo’s view that one assumes that nature is “perfect” and well arranged, along with his view that idealization and simplification that focus on elemental processes are necessary in order to construct sciences.

Also consider Descartes's use of skepticism in arguing for his method. It is important, I think, to look at Descartes's skepticism in light of the fact that it is probably motivated, in part, by his realization that the world as it appears to the senses and to commonsense understanding is not the world as it is understood by using the different route to understanding offered by science. Descartes's skepticism (and his solution to it) was unlike traditional versions including those of the academic skeptics and the Pyrrhonians such as Sextus Empiricus. It was rather aimed at indicating that by adopting a method that human beings were capable of developing and applying, one could come to know more about the nature of a world that differed a great deal from that accessible through *bon sens*. No doubt aspects of his efforts to avoid skeptical doubt, such as his reliance upon an all-perfect and good God, can only be taken seriously when reinterpreted in terms of something like Galileo's suggestion that the scientist think of nature as perfect. But by way of partially excusing Descartes, keep in mind that he had to live in a world with clerics and compliant governments that were not averse to using physical and mental punishments to get individuals to conform to official dogma. Descartes was well aware of Galileo's fate.

The second rule is "to divide each of the difficulties I examined into as many parts as possible as may be required in order to resolve them better" (Vol. I: 120). Treating this again as not a global recommendation but one that might yield knowledge in the domain(s) of science, it looks essentially like an addendum to the way I suggested construing the first rule. It needs little further comment. The third rule is "to direct my thoughts in an orderly manner, by beginning with the simplest and most easily known objects in order to ascend, little by little, step by step, to knowledge of the most complex, and by supposing some order even among objects that have no natural order of precedence" (Vol. I: 120). This rule is more problematic. It clearly shows the influence of Descartes's notion of proof in geometry and logic and it is fundamental to his foundationalist project, one that is difficult to take seriously today, given the failures of logical positivism's efforts, as well as the failures of like projects.

That aspect of the third rule is of little current interest, however important it might have been to Descartes in his effort to indicate that mathematized-formal "connections" and theories are central to science, and despite the role it might have played in his criticism of Galileo.⁴ The more important (for our purposes) aspect of the third rule appears in the final phrase, "by supposing some order even among objects that have no natural order of precedence." Construed as an early suggestion that the various sciences and their subject matters need to be unified in some way, and the demand that one look for "first principles" that can unify and support a coherent and comprehensive theory, it is both interesting and apropos. It is relevant here that Descartes had no doubt himself that he had in his contact mechanics offered what would these days be thought of as a theory of everything except for – as we will see – the mind and its free

actions. As he rather foolishly believed, unification of different theories of nature would not be needed; he already had something that in his view encompassed everything from neural signaling to the orbits of solar and planetary satellites. Perhaps that is another reason he thought order of argument to be closely linked to comprehensiveness-completeness-unification.

And finally, the fourth: “throughout to make enumerations so complete, and reviews so comprehensive, that I could be sure of leaving nothing out” (Vol. I: 120). This looks like a demand for descriptive and explanatory adequacy and, once again, comprehensiveness and unity or coherence. As noted, he thought that his contact mechanics left no room for any other theories, so the task of theory accommodation that is so obvious today would not have arisen. That said, it is still plausible, I think, to suggest that he believed unity and comprehensiveness to be a crucial desideratum for scientific investigation.

On the whole, I believe Descartes constructed and advocated a method for the naturalistic scientist that with some charity can be seen as anticipating modern practices. I have left two of the stated desiderata out. One is the demand for objectivity. That can, however, be taken as presupposed in the distinction between common sense and science. There were already in place in Galileo’s and Copernicus’s works significant challenges to Aristotle’s anthropocentric picture of the cosmos and of causation, and already atomism or corpuscularianism were dominant themes of “natural philosophy.” Boyle exploited their resources when he developed his chemistry shortly after Descartes. The other is progress. In that regard, had Descartes lived long enough, he would have found that his supposedly all-encompassing contact mechanics was fundamentally flawed. Among other things, its account of gravitation – as Newton’s inverse square law showed – was hopeless. That would have indicated that a contact mechanics is neither descriptively nor explanatorily adequate, and far from comprehensive. It would also have demanded a complete re-assessment of what Descartes took “objects” to be, and more fundamentally, challenged his notion of a physical science and his view of nature to the exclusion of the human mind (all other minds – God’s and angels’ excepted, no doubt, could – he thought – be captured by his contact mechanics, including its account of neural signaling and coordination).

Some aspects of Descartes’s scientific work survive. One is his view of neural structure and signaling. He thought of the nervous system as a system of interconnecting and valved/gated pipes that carried not electrical signals in the form of spikes, but something like a fluid (“animal spirits”) under pressure. Means apart, however, Descartes’s account is close to contemporary views. Another bears on the limits of scientific theory-construction. Stipulations, misguided assumptions, and expressions of hope aside, his observations on language use and its creative aspects in the fifth part of the *Discourse* remain in place essentially

unchallenged.⁵ It is quite clear that what can also be called “free will” remains outside the bounds of scientific investigation. That topic comes up below in my outline of the philosophical background of the research strategy that biolinguistics adheres to.

4.3 A research strategy for the naturalistic study of mind

Moving now from naturalistic methodology to an appropriate research strategy for the naturalistic study of mind and of language in particular, the study of mental systems should be both nativist and internalist. Here is why.

The naturalistic science methodology outlined above applies to the subject matters of all naturalistic scientific research (Chomsky speaks of “methodological monism”). No distinction is made between studies of systems inside the head or mind as opposed to those outside, nor are there other subject-matter-dependent restrictions. Perhaps if it turned out that studies of what is inside the head were not successful – as successful as one can expect any naturalistic science to be, that is: not every puzzle or mystery responds to the methodology in question – there would be need for a distinction. But so far, that is not the case. Philosophers might complain that natural science does not yield an account of the person’s point of view. But that is irrelevant. That is not what a study aimed towards objectivity can be asked to do (cf. Russell 1948). For that purpose, go to literature, poetry, and perhaps other forms of art, not natural science.

Natural sciences seem to work where their subject matters are “fixed by nature.” They have little to offer in the domain of human artifacts, including states, economies, and other institutions – and even less (in fact, likely nothing) in the case of individual human actions, including those actions that we call “using language.” These are products of choice, meeting interests and desires. Excluded from dealing with choice and action, the natural scientist of mind looks for the natural systems and connections between them that underlie and allow for choice. In effect, then, when applied to the study of mind and mental systems, natural science methodology assumes nativism. The nativism in question is a nativism of “machinery.” Assuming that humans are biological organisms, the machinery is the machinery of development and growth – morphogenesis and ontogeny. Biolinguists proceed on nativist assumptions of this sort and include among the issues that they have to address the details of the mechanisms that yield what is aptly called “the language organ.” They also seek to answer how this mechanism and its product came to be introduced into the human species (evolution), why there are obvious differences in languages (parameters and microparameters), plus how the organ functions (satisfying descriptive adequacy in an objective way), and in that vein, whether this functioning represents “good design” (whether

it is a near-perfect “solution” to the “task” it performs as the organ of language) or is rather the product of biological “tinkering.” There has been some progress on all these fronts; there is some illustration of this above, and other contributions to this volume provide considerably more detail. For my purposes, the central point is that naturalistic methods applied to the mind presuppose a form of nativism, and that nativism is one of at least two basic research assumptions of what is often called a “rationalist” approach to the study of mind.

Not everyone is willing to assume that language (apart from its use) is a natural object. For some – the “common man” and many cognitive scientists still – it is primarily a social institution invented by humans over millennia and – a few rather minor concessions to nativism aside – acquiring a language is not a matter of automatic growth or development, but a matter of “learning” that proceeds by getting a child’s linguistic “practices” found in his or her use of language to conform to those of a “speech community.” For those who believe this is the correct view of language and its learning, strong nativist assumptions are rejected and the aim is to get a child to conform in its “speech habits” with those in his or her “linguistic community.” This anti-nativism is characteristic of what is often called the “empiricist” approach to minds and their contents.⁶ Empiricists are anti-nativist. In placing language outside the head, moreover, they are “externalist”: language is designed by humans, and its form and rules are fixed not by nature, but by the shapes given it by the humans who create it.⁷ Now, if empiricists are anti-nativist and externalist in their view of how to go about studying a crucial mental system, one might expect that in contrast, rationalists are not just nativist but also internalist in their strategies for the study of mental systems. Generally, they are. There are independent reasons for adopting this strategy, but Descartes led the way by divorcing the study of whatever in the mind is involved in language from the use to which a person puts his or her language.

Descartes and other rationalists built nativist assumptions into studies of mind well before anyone undertook to apply naturalistic research techniques to the study of mind and its components (see Chomsky 1966/2002/2009 and my introduction to the 2009 edition, Chomsky 1980/2005, 1995a, 2000a). That is not surprising. It was not until later that the mathematical-formal tools needed to construct computational theories of the operations of the language faculty were invented, and also not until later that misconceptions about language and mind that include empiricist ones – but also at least one that Descartes himself introduced – were overcome. A central argument of the *Discourse* (part V) was instrumental in introducing Descartes’s misconception of study of the mind. Since – besides nativism and some hints of a computational approach mentioned later – this argument was also Descartes’s one positive contribution to the study of language (and mind) and because it helps emphasize why Descartes and other rationalists take an internalist approach to mind so seriously, I look at it in some

detail. The argument turns on some observations of how ordinary people routinely use language. These observations constitute what Chomsky calls “the creative aspect of language use.”

The observations are found near the end of the fifth part of Descartes’s *Discourse*. Earlier in that part, he touted what he thought to be the descriptive and explanatory advantages of his contact mechanics. Reading what he had to say, it is obvious he believed that this mechanics applied to everything in the “material” world. The creativity observations at the end offer his grounds for not including the mind (“reason”) in the scope of his “mechanical philosophy.” In compressed form they are as follows: neither machine nor non-human animal, he held, “could [ever] use words, or put together other signs, as we do in order to express our thoughts to others. . . . [For] it is not conceivable that such a machine [or animal] should produce different arrangements of words so as to give an appropriately meaningful answer to whatever is said in its presence, as the dullest of men can do.” These compressed observations are expanded upon in Cordemoy’s more detailed study, but the relevant features of what Chomsky calls the “creative aspect of language use” are found in the Cartesian text, so I focus on that alone. Decompressing: language use is not tied to circumstance nor caused – it is stimulus-free. It is also unbounded: respondents to questions can and do produce “different arrangements of words” with no restrictions on which, or on how many, or on their combinations. And yet even while neither tied to circumstance nor limited in number of words and their combinations, the responses produced by even “the dullest of men” remain “appropriately meaningful” with regard to the (discourse) context in question. In contrast, if they were caused “mechanically,” one could expect fixed or random responses to questions, not just one of an unbounded set of different but still appropriate responses. And while a fixed response might under certain assumptions be appropriate to circumstance, that is as the observations indicate irrelevant, for again, it is any of an indefinitely large number of different responses that can be appropriate. In effect, the creative aspect of language use creates a puzzle that a contact mechanics or for that matter any deterministic story about external or internal causation cannot address. One could perhaps speak of what a person produces in the way of an answer to a question as having been “rationally caused.” But that is a cheat. While people “produce” the relevant answers, this is not causation of a sort that is subject to any kind of treatment as the result of an algorithm that links what is said to a specific circumstance, contextually or discourse-fixed. You can get stimulus freedom by isolating the system that yields judgments – for present purposes, the language faculty, which can be conceived of as modular. And you can get novelty by introducing a randomizing mechanism in a device that yields strings that we (reading them) interpret as expressions. But isolation and randomness do not in any way yield appropriateness to discourse circumstances of the form observed. Descartes’s conclusion from all this

depended on his (misguided) belief that his contact mechanics exhausted the scope of naturalistic science. Since contact mechanics could not explain appropriate language use, Descartes assigned the job of producing the facts of appropriate use observed to an entity out of reach of his contact mechanics. That entity was a non-physical mind. While not to be taken seriously today or even 50 or so years after the *Discourse* appeared in 1637,⁸ his grounds for making this move were reasonable, given his assumptions and the options available to him.

As suggested, Descartes's observations had temporary and unproductive and also continuing and salutary effects. Given that his "explanation" for ordinary creative language use excluded a science he assumed to exhaust naturalistic explanation, it is no surprise that Descartes resisted the idea that one could offer a naturalistic theory of the mind, or of any of its components. That commitment could no longer be supported some 50 years later. As Chomsky (1995a, 2000a) points out, Descartes's account of matter ("body") and "the physical" went out the window when Newton introduced the inverse square law of gravitation. When that happened, contact mechanics became hopeless and theories of mechanics had to depend on forces that are for Descartes "natural science" and for common sense mysterious. After Newton's law, "the physical" could no longer mean "the material." After Newton (although with considerable resistance still) natural science (called "natural philosophy" at the time) took on a different cast: the "natural" is whatever the best naturalistic theory says, however mysterious nature might appear to be to commonsense intuitions about "body" and "the physical."

The continuing salutary effect was to divorce the study of language in particular from its use or application by people to the world, a move that instituted and continues to support an internalist commitment to the study of mind. Much later, and when joined to efforts to construct naturalistic theories of mind, it likely played a role in suggesting that one pursue modular approaches to not just language, but to other components of the mind. Descartes himself seems in some of what he writes to have thought the mind to be unitary, although that may be deceptive. Not only did he allow for distinctions between perception, imagination, will, and "understanding," and in addition acknowledge distinctions between the powers of the senses, but his discussions of a unitary mind tended to appear in contexts where he treats the mind taken as a whole as a unitary agent thinking and acting, not as a subject for study. Further, in his discussion of how the blind can sense depth, he came close to developing a computational theory of vision that attributes to this "faculty" unique forms of computation – an important aspect of a plausible notion of modularity. I do not pursue the matter further here.⁹ The important and salutary consequence of Descartes's observations was to divorce the study of whatever in the mind makes the use of language possible from the study of its use or application to the world and other forms of "problem solving."

By now, the view that the naturalistic study of the mind must focus on mind-internal systems, and on modular forms of them at that, can also be justified pragmatically. On the face of it, the only successful naturalistic sciences of the mind thus far have a computational form and do in fact focus on internal systems that operate in accord with their own “laws” (principles, algorithms, rules) over unique “inputs” that yield system-unique results. These systems are *prima facie* modular: uniqueness in principles, inputs, and outputs suffices to make them so without (pace Fodor) being encapsulated, or peripheral, or highly localized in the brain. Marr’s theory of vision and various linguistic and biolinguistic computational theories of language are among these modular, internalist sciences. The fact that so far success in constructing naturalistic sciences of mind has these characteristics, and that attempts to offer anything like “general” theories of mind never get beyond an initial stage, suggests that at the least, anything that will count for us as a naturalistic theory of mind is likely to have this form. It also seems to undermine the idea that anything like a generalized learning procedure could suffice to yield a system that naturalistic scientific methodology can deal with. And it suggests that the mind must operate by relying on the “cooperation” of several systems that operate in accord with very different computational procedures.

I emphasize that while unlike Descartes we now recognize that the mind can be the topic of naturalistic research, the facts of the creative aspect of language use remain unchallenged; see note 5. A plausible conclusion to draw from their persistence is not just that empiricist assumptions and efforts are unlikely to yield a science of language, and in addition that language is both modular and inside the head, but also that language use is not only now, but probably always will be out of reach of the methods and mental strategies of naturalistic scientific research. This should not be a surprise. The use of language is a form of human action, and like all human action, it resists naturalistic scientific study. No doubt by using the resources of folk psychology we can make reasonably good judgments concerning people’s intentions and offer reasonably reliable guesses about what they will do. But we do not do that by employing the tools of natural science. Descartes, Cudworth, the romantics, von Humboldt, and Chomsky recognize that. You can put a label on the problem that seems to explain why there is a barrier: “free will.” And you can say what it involves: the exercise of a “rational cause.” You can even in the case of language point to some of the factors that make it possible: a modular faculty of language that can yield indefinitely many ways of understanding and proceeding. But you do not get to have a natural science of the phenomenon.

It is an open question whether this barrier to natural science is to be explained metaphysically by appeal to the nature of “free will,” whatever that might be, or rather explained by appeal to biologically based facts about the natures of our minds and the kinds of ways of understanding the

world and ourselves that it makes available. The latter is the more interesting, or at least the much more readily addressed way to approach the matter. It is more readily addressed first because – despite Descartes's misguided belief that reason (presumably in the form of science) provides us a universal problem-solver – it is *prima facie* plausible that our minds have limitations. Not just the problem of free will but other apparently well-understood and not merely confused or specious issues escape our grasps. These limitations appear to be built into our species: individual minds may differ in talents, but some barriers appear to be insurmountable, no matter how talented or gifted a scientist. So it is surely likely that these limitations are built into our biological natures. Those natures provide us a flexible and readily available common sense that, however, solves no scientific problems. And they provide us with a fruitful but apparently limited scientific methodology that can deal with causal systems or with randomness, but not with “free will.” It can be no accident that we are the only organisms to have not just language, which allows us to put innate commonsense concepts together in various ways, but the capacity to “do science” too. If the limits are biological, they can in principle be investigated. That is the second reason it is easier to address the issue of the limitations of the human mind by adopting an internalist approach and looking for facts about our cognitive systems that impose the limitations.

4.4 The biolinguistic research program

Finally, let us turn to the biolinguistic research *program*. So far, we have looked at the philosophical underpinnings of naturalistic scientific methodology and those of naturalistic methodology applied to the mind, where natural science appears to yield scientifically respectable results when dealing with a mental system that is inside the head, is innate, and is modular, or at least is – and likely must be – treated as modular in the sense specified above. Now we look at the philosophical foundations for employing the tools of the science of biology in the study of language and other internal, modular, innate systems.

Surprisingly, given that there was nothing like a science of biology during his time, Descartes has a role to play even here. He suggests that for a mental entity or system to be innate is for it to result on being offered appropriate input in the growth or development of the mental entity or system. Specifically, in his 1648 *Comments on a Certain Broadsheet* in speaking of errors that an interpreter of his called Regius had made in understanding his work, Descartes explains what he has in mind by an innate idea. He says: “This is the same sense as that in which we say that generosity is ‘innate’ in certain families, or that certain diseases such as gout or stones are innate in others: it is not so much that the babies of such families suffer from these diseases in their mother’s womb, but simply

that they are born with a certain ‘faculty’ or tendency to contract them.” This explanation clearly calls for some sort of mechanism by which a condition is transmissible from members of a family to another member. He did not have anything like modern biology in mind, of course, but it is difficult to understand the mechanism he envisaged unless it is seen in a way that invites what we would call genetic and epigenetic transmission of a characteristic that on being appropriately stimulated or “triggered,” by either distal or proximal means, develops in a way “anticipated,”¹⁰ in the relevant developmental machinery. Moreover, an at least similar understanding of innateness of a mental “entity,” one that calls for a mechanism linked to inheritance, appears to be called for by Cudworth when in his extensive and still relevant discussion of “innate ideas” in his *Treatise Concerning Eternal and Immutable Morality*, begun in 1662 but not published until 1731, well after his death in 1688, he spoke of an “innate cognoscitive power” that on an “occasion” presented with some input to the system or “power,” automatically and immediately yields an “idea” relevant to the understanding of the occasioning (we would say triggering) occasion. This “power” is one that all creatures share to an extent, although human beings have a special form of it that is available to no other creatures. We, unlike other creatures, quickly and automatically develop ideas like MECHANISM, WATCH, TRIANGLE, and HOUSE. No doubt he (and Descartes too) attributed this cognoscitive power to God, not biology; that was to be expected at the time. But even assuming that this explanation were taken seriously, occasionalists aside, most assumed that God would not him-, it-, or herself bother to do the deed each time a person needed a new “idea” of some relevant sort, but would have built into human minds in some way a mechanism to provide the relevant ones when needed, thereafter to be stored in memory. Innate ideas in this sense (DOG, RIVER, WATER, etc.) are not as popular these days as innate general principles, such as Merge in its several forms. It is not clear why, however. Many of our “ideas” or concepts – those that constitute the conceptual range of commonsense understanding – appear to develop as automatically and at least as quickly as the combinatory principle Merge. The pre-verbal child already has available a rich and rather diverse set of concepts (Hirsh-Pasek and Golinkoff 1996; Woodward 2008), and generally speaking, the child’s ready acquisition of “ideas” as revealed in their swift and appropriate application offers thousands of examples of poverty of the stimulus observations. The issue should not be whether many of our “ideas” are innate; given the observations, we should assume that they are, unless shown otherwise. The issue is what these ideas or concepts are, what the mechanism(s) that yields them is, how they come to be associated with linguistic “sounds” (for which there must also be an acquisition mechanism), and how they are “represented” in the mind-brain. They do, after all, represent “information” that when employed through the use of language is applied by ordinary speakers in all sorts of appropriate

ways. They constitute an important part of the knowledge of language that a person with an I-language has available.

In the same essay in which Descartes appealed to some kind of innate “mechanism” to make sense of how ideas develop, he also introduced a tripartite distinction between what he called “innate” ideas (those “thoughts within me which neither came to me from external objects nor were determined by my will, but which came solely from the power of thinking within me”), “adventitious” ideas, and those that are “made up.” In a letter to Mersenne in June of 1641 he gives examples of each. The innate include GOD, MIND, BODY, and TRIANGLE. The one adventitious idea he mentions is “the idea we commonly have of the sun.” And for a made up or constructed idea, he suggests “the idea which the astronomers construct of the sun by their reasoning” (all examples at Descartes 1991: 183). The distinction between the “made up” ideas and the other two is clear: it is the distinction between scientific concepts that are introduced by scientists through “reasonings,” presumably systematic – that is, through constructing theories that among other things place the sun and not the earth at the center of the solar system and, more recently, attribute the radiation the sun produces to fusion reactions. These do not figure in the adventitious everyday idea of the sun, which we can take to be the one that is triggered during development. Adventitious ideas include virtually all of our commonsense concepts. The puzzle is why Descartes introduced the distinction he does between innate and adventitious ideas. In the essay denouncing Regius’s misunderstandings again, and immediately after he introduces his tripartite distinction (Descartes 1984–5, Vol. I: 304), Descartes places all “common notions” (thus, the “idea we commonly have of the sun” as a body that moves around the earth, etc.) inside the mind, and thus places them among the innate ideas, albeit needing triggering or what Descartes calls an “occasion to form these ideas by means of the faculty innate to it.” Compounding the puzzle, it is difficult to understand why ideas such as GOD and MIND do *not* need triggering. A cynical interpretation is that these “ideas” just happen to be central to Descartes’s philosophical work, so he gives them a special status. Or perhaps he thought that these ideas were so “clear and distinct” that they could not possibly have required experience or “input” of any sort. Fortunately, I do not need to pursue the issue. For our purposes, the “occasioned” ideas are innate, and they are distinct from those introduced in the sciences. We find, then, a clear connection in a founder of the rationalist tradition between the innateness of (commonsense) ideas and their exclusion from the concepts or ideas developed in the sciences.

What about the notion of a Euclidean triangle, though, should it not be in the toolbox of the scientist? Plausibly, the idea of a *Euclidean* triangle is not only in science’s toolbox, but is a distinctively scientific concept, for to emphasize “Euclidean” presupposes a distinction between Euclidean triangles and non-Euclidean forms that require different notions of parallels.

That there are distinct forms of triangle (Euclidean, hyperbolic, and elliptic) does not come automatically. Nor, perhaps, does even the idea that the sum of the internal angles of a triangle must equal 180 degrees. Alternatively, of course, one could argue that it is in fact innate, but that does not prevent its use as a tool in constructing a science, and it is the object of a science only if one attempts a science of human concepts and how they develop.

Later work too assumed some kind of “mechanism” built into humans at birth that yield “common ideas” and even the structure of specific mental systems and the overall organization of the mind. That theme is found throughout the seventeenth century in the Cambridge Platonists. I have already mentioned Cudworth. Another was Herbert of Cherbury. Herbert in 1624 anticipated contemporary work on an innate moral faculty that is now being done by John Mikhail (2010) and others. Herbert insisted that innate “common notions” must underlie our capacity to make moral judgments, shaping them and preventing “vacant forms, prodigies, and fearful images” from dominating. As for language, and now extending into the eighteenth century, Chomsky in *Cartesian Linguistics* (Chomsky 1966/2002/2009) emphasizes the Port-Royal Grammarians’ work on the internal rule-based organization of language and thought. While they were influenced not only by Descartes but by others – Sanctius among them – they remained well within the Cartesian rationalist tradition by virtue of an effort to construct a “Philosophical” (scientific and universal) grammar with its “immutable and general principles” (Beauzée quoted at Chomsky 1966/2002/2009: 93 [page references are to the 2009 edition]) that clearly called for these principles to be innate in some kind of device. This kind of grammar could go beyond the descriptive to offer “explanation,” providing what D’Alembert (94) called the “true metaphysics of grammar.” That can be understood, using contemporary terminology, as an effort to offer a science of a part of the human mind – moving from “natural history” to “natural philosophy” or what we think of as natural science (97) of the mind. Their success is another matter, but for the time with its limited scientific resources, and against background assumptions about the relation of thought and language plus lingering medieval views about the nature of reason that were bound to mislead, their accomplishments were remarkable. Little progress could be made unless confusions and misunderstandings such as those found also in Descartes’s work were removed and, as mentioned above, only with the introduction of formal methods such as recursive functions and their application – along with the rest of the machinery of naturalistic scientific research – in an effort to construct an explicit formal theory of universal grammar that met descriptive and explanatory demands. Von Humboldt, for example, while he offered some extremely suggestive and interesting observations, never managed to construct a theory of the “internal mechanism” (or mechanisms) that must provide the resources of sound, meaning, and combinatorial operations on which language relies. Thus, it was not until Chomsky

and others working within the assumptions of naturalistic method and rationalist strategy for the study of mind that a naturalistic theory of language could begin to emerge. And as found later, attempting to address and satisfy the full range of naturalistic methodology desiderata could only really begin once the issue of acquisition could be seen as sufficiently well addressed. That began with the introduction of the principles and parameters model. Only with it and other considerations in place, and with the naturalistic scientist's effort to simplify cashed out in a "minimalist" program that was also now capable of addressing "third factor" contributions to evolution and ontogenesis, could accommodation to biology begin to be taken seriously. The linguist could now attend to matters such as finding a clear and explicit account of the function of the language organ (rather, of its core: meeting bare output conditions) and addressing the "design" issue of how well the actual system carried out this function (the matter of perfection). Liaisons with developments in biology, found especially in what is called "evo-devo," could now be explored, genetic coding matters could now be put on the table, and so on. After principles and parameters, and with the aid of minimalism and recognition of third factor considerations, the specific lines of research that linguists pursued could begin to yield theories that more nearly satisfied the full range of desiderata and thereby make more progress than was possible before.

It is not particularly surprising that linguists began to recognize the importance of "third factor" considerations – constraints on growth and development that go beyond experience and genetic coding, and recognition that evolution involves contributions from physical, chemical, and other structuring sources – only recently. It might now seem an obvious point: surely, for example, there cannot be enough information in the genome to determine the growth pattern of every nerve cell's axons, nor ensure that each connects in the "right" way to ensure signal transmission efficiency (cf. Cherniak 2005, Cherniak *et al.* 2004). But while it is now a common theme of evo-devo research, it took a while to sink in even among biologists, and it sank in only rather slowly. In the twentieth century, some of the credit should go to mathematical biologist D'Arcy Thompson (1917/1942/1992) and philosopher-polymath Alan Turing (1992) in their insistence that not just growth, but evolution is constrained by factors other than what Turing disparagingly called "history" – by, among other things, constraints imposed by physical and chemical law, including general constraints on the possible forms that natural objects and events can take. Credit also goes to the work of Waddington in the 1940s on growth. For a summary of more recent developments, see Carroll (2005b). Some background is available in Gould's monumental 2002 book.

As for earlier origins: arguably, Johann von Goethe first broached the core ideas in his pursuit of the "Urpflanze," the formula that he thought could predict all possible shapes of annual flowers. In a letter to Herder in 1787, he told of what he declared to be a marvelous invention of his, an

Urform or basic formula with which he believed he could predict the shapes or morphologies not just of existing annual flowers, but of all naturally possible such flowers (Chomsky 1966/2002/2009: 72). A plausible analogy to what Goethe had in mind is found in using the Fibonacci series to predict the shapes of sunflower seeds in the flower. Goethe also seems to have assumed that his basic approach could yield a means by which the morphologies of all naturally possible plants could be predicted. It is possible that his contribution led to some of the “formal” work done on biology in Europe; for discussion, see Gould (2002: 271f.).

It is worth keeping in mind that when third factor considerations are coordinated with Darwinian and neo-Darwinian views of natural selection, we find a rather different view of evolution. No doubt only some mathematically and formally possible organisms and shapes have “survival value,” so only some will be found among extant species, now and before.¹¹ But that might be Darwinian selection’s *only* role in structure-building. Its contribution in a specific case could reduce to “selecting” the shapes or forms that can in a specific kind of environment or environments persist. At the least, Darwinian selection with or without genomic supplementation by no means fully explains the “shapes” of organic entities or – as in the case of language – the organs within organisms, including their possible variants. And as for differences between languages: as suggested before, physically (and computationally, etc.) possible non-genomic considerations might fully explain variation in languages, effectively constituting the parametric options of the principles and parameters approach (cf. Chomsky and McGilvray 2012).

With that, I conclude discussion of philosophical precedents and foundations for biolinguistics. I know of no philosophical antecedents for the now-plausible assumption that language – or at least, what is needed for the computational mapping from “atomic” concepts to SEMs within phases – developed as the result of a single transmissible mutation that turned out to be useful, for it provided humans the capacity to think complex, structured thoughts, even if they were not conscious of them before linking to sounds or signs came to be the case. Nevertheless, there were – as indicated – some precedents for looking to biology to make sense of how language and the concepts it expresses (and sounds it employs) could be seen to be not just inside the head, but innate. And the minimalist version of biolinguistics can be traced to the natural scientist’s demand for simplicity.

4.5 Conclusion

I have tried to outline scientific methods and research strategies for the study of mind that have yielded successful naturalistic scientific programs in the last few centuries, and indicated their philosophical origins and

underpinnings. Given this, and given that the biolinguistics program launched in recent years as the result of work by Chomsky and others appears to conform to the successful methods and strategies for the naturalistic scientific study of mind, it is plausible, I think, to expect that this program is on the right course to continue to develop successful sciences of language in the human mind.

On the whole, it is unlikely, I think, that there will be major changes in naturalistic scientific methodology, and I suspect that a nativist and internalist strategy for the study of mind will continue to yield good naturalistic theories of components of the mind. I am reasonably confident about these predictions because I suspect that the method and strategy outlined have the characters that they do because their characters reflect facts about the way in which the human mind works – and fails to work. As for the biolinguistic research program, it is already obvious that the term is a bit of a misnomer. Perhaps a better, although very awkward one might be “bio-chemico-physico-compulinguistics.” Because of the need to reach into and accommodate to several non-biological areas, it is, I think, likely to undergo several changes and to explore unanticipated directions for research. But it should remain a viable program; it has a good provenance. And certainly at the moment it has exciting prospects.

Part I

Language
development

5

(Evidence for) the language instinct

Ianthi Maria Tsimpli

5.1 Introduction

One of the most influential aspects of modern formal linguistic theory is the shift of focus from language as a product of social or behavioral habits to language as a mental state of the human individual. The implications of this shift have extended to all areas of linguistic research including those that are interdisciplinary by name and nature (sociolinguistics, psycholinguistics, and pragmatics) but also those concerned with levels of linguistic analysis, from phonology to semantics. In essence, formal linguistics is viewed as a discipline concerned with human cognition and brain structures. In this respect, syntax and semantics aim to account for mental representations in the same way that psycholinguistics dealing with linguistic behavior in typical and atypical circumstances aims to describe mental representations which underlie and dictate this behavior.

This shift in linguistics from epiphenomena and external factors to mental architecture and properties of human cognition coincided with a similar shift in many other areas of scientific research which attempt to account for knowledge, abilities, and developmental deficits, primarily of a non-linguistic nature (Novick *et al.* 2005, 2010; Dibbets *et al.* 2006). This more general shift of perspective is related to and probably accounted for by rapid developments in brain and cognitive sciences as well as in genetics (e.g. Sakai *et al.* 2002; Kinno *et al.* 2008; Lai *et al.* 2003).

The investigation of language as a mental construct does not exhaust the fundamentals of Chomskyan linguistics, however. It is the special status of Language in human cognition as an innate, autonomous, domain-specific, and, partly, modular system that justifies the revolutionary flavor of the shift of focus described above (Chomsky 1986b; Fodor 1983; cf. Karmiloff-Smith 1992). The stronger the claim for innateness the more controversial and falsifiable it becomes; at the same time, the stronger this claim is the more appropriate it becomes to speak of a *language instinct* (Pinker 1994).

5.2 Instincts and language

Instincts characterize a species as a whole: they presuppose biologically determined predispositions in the form of genetic programming and they trigger implicit and inevitable development which does not involve any “learning” process. We speak of the instinct for survival, reproduction, etc., all of which are based on facts of species evolution as well as on research on brain properties related to hormonal changes in maturation, aging, and pathologies. Instincts underlie biologically triggered development and determine shared aspects of species behavior (Birney and Teevan 1961). In humans, instinctive behavior may not be directly recognized as such given that the power of higher cognition in the form of conscious or subconscious thinking may mask the instincts’ workings. In this respect, instinctive behavior may be quite complex which is one of the reasons why the notion of an “instinct” in psychology seems insufficiently constrained to be genetically identified. Similar objections to the lack of precision in defining instincts are made in connection to the usual, and occasionally sloppy, links between instincts, innateness, and genetics. Finally, instincts have been compared and confused with habits although the role of experience may not be causally involved in either (Darwin 1859). In the case of language, experience is indispensable. In fact, it is the crucial role of experience in language development that has been put forward as the major counterargument to language being an instinct (cf. Tomasello 1995).

Instincts should, by definition, be part of the genetic code of the species. Accordingly, the idea of a language instinct invites discussions on the role of genetics, such as the relatively recent debate about the “gene for language.” Discussions on the FOXP2 gene in the work of Enard *et al.* (2002) and Lai *et al.* (2003) initially associated this gene with speech. It was later suggested that the FOXP2 is also relevant to language and communication. The empirical support came from the extensive study of three generations in a family known as the KE family, in which half of the family members suffered from speech and language disorders while the remaining individuals were unaffected by the deficit (Gopnik 1990a). The deficit was characterized as being language specific and the affected members as individuals with Specific Language Impairment (SLI), an inherited disorder. Lai *et al.* (2001) linked this deficient behavior with a part of the non-sexual chromosome of the FOXP2 gene, giving rise to interpretations of this finding as the discovery of the gene for language (and for syntax, in particular).

However, further research led to the abandonment of the idea that FOXP2 or any other gene could be the “gene for language” (Vargha-Khadem *et al.* 1995; Marcus and Fisher 2003). It was established that the language problems in the affected members of the KE family were not

exclusively with syntax and grammar: mouth and face dyspraxia (leading to articulation problems) as well as other aspects of language were affected. Furthermore, the role of FOXP2 as a transcription factor is to regulate the amount of a particular protein produced. This has cascade effects on other proteins in its pathway making it impossible to isolate the FOXP2 as the gene for language, although, according to Newbury and Monaco (2002), “it is feasible that another gene within the FOXP2 pathway will be involved in the SLI phenotype.” As a result, the question of whether there could be “a gene for language,” let alone whether it has been identified, has been dismissed as simplistic (Smith 2005). Nevertheless, the link between genetics and language is still sought and considered highly relevant to current research. What needs to be established is a set of plausible alternatives for the genetic coding of language based on what biology and genetics can offer. We consider some of these alternatives, suggested in Moro (2008), below.

Can instincts be selectively impaired? Language, arguably, can, as in cases of developmental language disorders (Leonard 1998; Clahsen 1989; Kail 1994, Miller *et al.* 2001) but also in acquired disorders such as agrammatism (Friedmann and Grodzinsky 1997; Grodzinsky 1995). The question remains however as to whether in these pathologies individuals have no language instinct to start with, or lose it as an acquired effect. In other words, the link between language pathology and language instinct could be established if linguistic representations in pathology altogether lack whatever neurological structure genetically defined underlies the language instinct. If the answer to this is negative, then the alternative possibility suggests that only certain aspects of language can be impaired, sometimes massively. With respect to agrammatism, the answer is easier to provide: since partial or full recovery is possible, the language instinct should be in place irrespective of impairment. If we consider instincts other than language, it is difficult to show that any developmental disorder, even of the most severe type, can be reduced to a selective, genetically encoded disorder thereof. In other words, when the genetic code is so atypical that basic properties of the species – defined as instincts – cannot be identified, the affected member (embryo or newborn) may not be viable anyway (cf. Moro 2008).

The possibility of selective impairment is one of the criteria proposed for those cognitive systems which are not only domain-specific but modular as well (Fodor 1983). The fact that aspects of language can be selectively impaired in inherited, developmental disorders implies that linguistic properties are indeed biologically, hence genetically, encoded. As argued above, though, the dedicated status of one or more genes to language properties has not been substantiated; if anything, the opposite holds, in that the genes related to language problems do not exclusively code speech or language. The criticism in fact extends to the innateness of linguistic modularity although this notion primarily refers to cognitive rather than

neurological descriptions, neuropsychology attempting to bridge the gap (cf. Karmiloff-Smith 1992).

The selectively intact status of language in individuals with impaired non-verbal cognition presents the opposite side of the coin. A small number of case studies have been reported in the literature (Yamada 1990; Smith and Tsimpli 1995; Smith *et al.* 2011) showing that disorders affecting a whole spectrum of cognitive abilities may leave language unaffected. Looking more closely into the case of Christopher, for example, allows us to illustrate the point. Christopher is an individual who has been institutionalized all his adult life because he is unable to look after himself. On standardized measures of non-verbal cognition he scores between 40 and 70 while his verbal abilities are within the upper range of the scale (O'Connor and Hermelin 1994; Smith and Tsimpli 1995 and references therein). Furthermore, Christopher is an atypical autist, showing an inconsistent pattern of performance on standard "false-belief" tasks while, at the same time, he consistently fails to interpret non-literal language in expressions of metaphor, irony, humor, and rhetorical questions. Christopher's profile becomes unique when one turns to language. Apart from English, his native language, Christopher speaks and/or understands twenty other languages to different degrees. His language learning abilities exhibit an extremely fast and accurate pattern mostly for languages which have a written form, although his ability to learn signed languages lacking written feedback, although overall weaker, still reveals a special talent for language compared to non-verbal abilities (Smith *et al.* 2011).

Nevertheless, a valid criticism of the asymmetrical pattern attested between cognition and language can be made at least for Christopher's case: an in-depth investigation of his linguistic abilities reveals asymmetries within his native language: while morphology and vocabulary are intact, syntactic abilities show a diverse pattern. Specifically, although subordination, in the form of relative and adverbial clauses, interrogatives and parasitic gaps, is clearly part of Christopher's grammar, topicalization and left-dislocation are not. Other aspects of language performance are also affected; Christopher's translations into English (from a variety of languages) occasionally fail to meet the criteria of coherence and pragmatic plausibility. Similarly, recovery from locally ambiguous structures exhibiting garden-path effects is very hard for Christopher while interpreting non-literal language is distressing.

This picture reveals that, within Christopher's native language, certain areas are impaired while others are spared. In fact, if we draw a comparison between Christopher and the SLI individuals mentioned above, the loci of the contrast are in the domain of morphology and language-specific rules of syntax. What about discourse-conditioned structures in SLI individuals, which seem to be impaired in Christopher? Performance varies (Bishop and Adams 1992; van der Lely 1997) and the problems in morpho-

syntax are bound to confound the results. What we are left with, then, is some demarcation of internal boundaries in the spectrum of language abilities distinguishing between more and less discourse-sensitive language use. In other words, the attested contrast lies in the spared or impaired status of formal aspects of language, narrowing them down to Syntax and excluding other aspects of linguistic performance which may be affected or unaffected for independent reasons, e.g. at the interface of language and discourse (i.e. when communication disorders are also at stake, as is the case with Christopher's atypical autistic profile). However, even having narrowed down the relevant domain of language which shows selective impairment or development, to Syntax, we are still far from being specific enough in identifying the purported distinctive feature of language that is the best candidate for expressing the language instinct.

Syntax, the derivational or computational component of language (Chomsky 1995b, 2000b), is characterized by a single operation, Merge, responsible for structure-building, otherwise known as recursion. Recursion is an intrinsic, not learnable, fundamental property of human language which is blind to semantic categorization or communicative constraints. Recursion operates on syntactic objects which can be categories, words, or features. As such, it constitutes a defining property of any natural language in the sense that no variation across languages is expected to be found with respect to the availability of recursion. As soon as linguistic representations are formed in the child's cognition, recursion determines the nature of the representation. Moreover, recursion is assumed to be the invariable but also unique option available to natural languages. Accordingly, recursion satisfies the definition of instinctive behavior – shared by every member of the species – and, in this respect, could be a good candidate for the language instinct.

Two problems arise with this suggestion: first, it has not been convincingly shown that there are pathological grammars, as in Specific Language Impairment, which can be so fundamentally impaired that they lack recursion or constraints associated with it, such as locality (cf. van der Lely 1996; Rice 2000). Although SLI may be characterized by problems with processing object relative clauses, object clefts, or passive formation (van der Lely and Harris 1990; van der Lely 1996), the selective pattern of impairment points to difficulties in establishing the dependencies formed but not to the implementation of processing strategies beyond natural language options, such as linearly based dependencies.

Furthermore, and more importantly, the property of recursion has been shown to characterize other cognitive abilities, such as music, arithmetic, theory of mind, and visuo-spatial processing (Hauser *et al.* 2002). Accordingly, recursion is neither modular nor domain-specific. Along similar lines, Tettamanti *et al.* (2009) provide evidence from fMRI studies on healthy adults processing structural dependencies formed between visuo-spatial symbols. The structures were designed to bear similarities with

dependencies formed in natural language. On the assumption that all syntactic structures in any natural language involve non-rigid, i.e. not linearly fixed, distances between words involved in any dependency, Tettamanti *et al.* (2009) designed such dependencies using visuo-spatial symbols rather than words. Their findings show that Broca's area is crucially involved in the processing and the acquisition of both visuo-spatial and linguistic structures, thus arguing that the nature of the task, i.e. sequential processing, rather than domain-specificity, can account for the brain region involved in input processing and acquisition. The more general argument in this research is that non-rigid structural dependencies characterize human cognition in a unique way, distinguishing human from non-human primates. On these grounds, and consistent with Chomsky's hierarchically structured symbolic sequencing constitutes a good candidate for the defining property of human cognition, thus, also, of a specifically human instinct; but *not* of a language instinct as such.

To summarize the discussion so far, human language fulfills some of the properties attributed to instincts, such as the inevitable, implicit, and universal status it enjoys in the species. The quest for a dedicated gene for language, however, has not been fruitful and may eventually prove to be futile. In relation to this question, Moro (2008) presents a number of alternative reasons for why there is no "Mendelian" linguistics as yet. The first has to do with the possibility that genes which contribute to the cortical structures crucial for language are also involved in the building of other vital organs, the implication being that any malformation will lead to the non-viability of the affected member. The second possibility Moro considers is that language is a necessary by-product of other, selected features, which have nothing to do with language (borrowing the term "spandrel" from Gould 1997). The third possibility is even more radical: language is perceived by humans as a real object, although it does not really exist as such. By language, Moro refers to the rule-governed status of grammar which we perceive as having an independent existence, although this may be just a Gestalt phenomenon.

In summary, the claim for a language instinct cannot as yet be substantiated by genetics in the form of having isolated genes specialized for language. Furthermore, it appears that what constitutes the quintessential property of language in the formal sense, namely recursion, is not a defining characteristic of language alone, but of other aspects of human cognition too.

Nevertheless, the search for the genetic basis for language is still actively open. There is sufficient evidence from various scientific fields which sustains the metaphor Chomsky proposed for language being an "organ" (and Darwin's (1859) view that language is an instinct): (a) language is localized in the human brain, (b) language "grows" in the human mind through a deterministic and implicit process of acquisition, (c) the nature

of experience required for language development is sufficient for language “growth” but not for language “learning,” in the sense of building knowledge through a general-purpose learning mechanism. Thus, there are at least three domains of inquiry which provide indirect evidence for a language instinct: research on the neural correlates for language, on language acquisition, and on language learnability in the face of under-determined input. In the following sections we will consider some of the relevant evidence from these domains.

5.3 The locus of syntax in the brain

In recent years, the use of functional neuro-imaging in brain studies has included the study of language knowledge and language processing as well. Although there is a considerable body of functional imaging research looking into lexical retrieval and processing (Damasio *et al.* 1996; Blumstein 2011), phonology (Heim and Friederici 2003), prosody as well as semantic memory and retrieval (Martin and Chao 2001, for a review), we will concentrate on studies investigating brain regions activated in syntactic representations and processing. The reason for this rather restricted choice has hopefully become apparent from the discussion in the previous section: in our search of evidence for the language (or, more generally, the human) instinct, we concentrate on the basis of linguistic computation, namely recursion.

The processing of syntactic dependencies either in structures which include movement of an element (displacement) as in interrogatives, relatives or passives, or in binding dependencies where antecedent-anaphor dependencies are established, have been investigated. Syntactic theory considers displacement structures, also referred to as “filler-gap” dependencies, as distinct from binding relations where no real “gap” is included. Moreover, differences between passives on one hand and interrogatives on the other are also postulated on the grounds that passives rely more heavily on lexical relations expressed in terms of argument structure alternations.

Behavioral data on the processing of filler-gap dependencies such as interrogatives have repeatedly shown that such dependencies are psychologically real in terms of their online computation (Nicol and Swinney 1989; Traxler and Pickering 1996). The processing of passives in adult monolinguals has also shown online effects differentiating them from active sentences (Ferreira 2003; Rohde and Gibson 2003). Crucially, the online differences extend to other populations such as child vs. adult and monolingual vs. L2 learners. Thus, L1 children and L2 adult learners have been shown to differ from monolingual adults in the processing of interrogatives or passives. The differences revealed are both quantitative in terms of processing speed but also qualitative in decision

making for grammaticality preferences or judgments (Marinis *et al.* 2005; Marinis 2007; Felser *et al.* 2003; Felser and Roberts 2007; Stromswold *et al.* 1996). Online processing data from agrammatism examining the processing of structural dependencies of both the filler-gap and the binding type have also been used to establish the locus of the deficit in impaired language processing (Friedmann *et al.* 2006; Thompson and Shapiro 2005; Dickey, Choy, and Thompson 2007; Choy and Thompson 2010; Dickey and Thompson 2009). Interestingly, eye-tracking studies by Dickey *et al.* (2007) and Dickey and Thompson (2009) point to a real-time filler-gap strategy for both impaired and unimpaired adults in the comprehension of object displacement in wh-movement structures but not in the case of passives.

Language processing data from functional neuro-imaging in healthy adults converge on the finding that Broca's area is crucially involved in the processing of syntactic dependencies (Moro *et al.* 2001; Musso *et al.* 2003, Grodzinsky and Friederici 2006; Santi and Grodzinsky 2007b). In addition, as shown by Santi and Grodzinsky's (2007b) study, structural dependencies formed by displacement/movement activate a different part of the Broca area (BA44) from binding dependencies (BA45/47). The two types of dependencies differ further in that binding also activates a right-hemisphere region, the right middle frontal gyrus, while each type of dependency activates different regions in Wernicke's area. These findings are important for two reasons; first, they point to a convergence between formal linguistic theory and neurological structures in the human brain, substantiating the shift in the focus of inquiry from language use to the cognitive representation of linguistic knowledge. The second reason is that they can compromise differences between theoretical constructs mapped onto neurological structures of language "knowledge" and how this may be affected as evidenced in lesion studies, and those neuro-anatomical areas of the brain that implement this knowledge in real-time language use (Grodzinsky and Friederici 2006). Syntactic processing assumes three phases of computation: local phrase-structure building, expressed by the Merge operation in minimalist terms, the computation of syntactic dependencies involving displacement but also linearization strategies, and finally the syntactic integration phase where, for example, recovery from garden-path effects takes place (Friederici 2002; Friederici *et al.* 2006b). The brain area activated in the third phase, i.e. during syntactic integration, is the left posterior superior temporal gyrus while in the first phase, local phrase-structure building, the left inferior frontal gyrus. Overlap in the brain regions activated during language processing and those identified in lesion studies strengthen the picture of dedicated domains for language loci. On the other hand, divergent findings might imply that more than one function (e.g. working memory as well as structural dependencies) is neurally localized in the same region (Santi and Grodzinsky 2007b).

The dedicated status of particular brain regions for structural dependencies of movement/displacement is further supported by fMRI evidence obtained from the processing of embedded wh-questions and topicalizations (Ben-Shachar *et al.* 2004). In both cases, a set of brain regions was activated which was not involved in the processing of sentences without movement dependencies, such as the dative shift alternation. There is thus converging evidence supporting the existence of neural correlates for processing filler-gap dependencies in language.

Independent, and convergent, evidence from neuro-imaging studies is drawn from language learning paradigms. Tettamanti *et al.* (2002) investigated the neural correlates of (second) language acquisition in a group of healthy adults whose task was to discover novel syntactic rules in a language phonologically similar to their native language (Italian). The use of pseudo-words allowed researchers to exclude lexical semantic effects and concentrate on the process of syntactic acquisition. The experimental conditions distinguished between the target rule being structure-dependent or not, i.e. falling inside or outside natural language borders. The results were robust and particularly encouraging for establishing neural correlates of natural language acquisition of syntactic structures. In particular, Broca's area was selectively activated in the acquisition of the structure-dependent rule only. Moreover, the comparison between high- and low-proficiency learners revealed an increase in the activation of the left perisylvian cortex, previously shown to be implicated in syntactic processing. In Musso *et al.* (2003) adult native speakers of German were exposed to Italian and Japanese in two experiments consisting of a series of five learning sessions. One learning experiment involved samples of these two languages which instantiated "real" grammar rules while the other experiment included "unreal" rules for language, namely rules which were based on linear rather than hierarchical relations. The behavioral results obtained through grammaticality judgments and accuracy rates in the learning curve showed that all subjects were able to learn both types of rules. The reaction times measured throughout the learning sessions showed a decrease for both natural and unnatural rules. However, the reaction times in judgments were significantly faster for the "real" rules. Turning to the functional imaging results the two experiments showed an interesting neural discrepancy in that Broca's region was activated only during the acquisition of "real" rules while it was gradually disengaged in the learning of the "unreal" Japanese or Italian rules. This finding is consistent with Tettamanti *et al.* (2002) reported above. A significant negative correlation between the blood oxygen-level dependent signal in the left inferior frontal gyrus and the level of accuracy in the "unreal" rule condition was attested while a positive correlation was found in the "real" rule condition.

The findings of Tettamanti *et al.* (2002) and Musso *et al.* (2003) have important implications for foreign language learning as well: the conditions

in which learning took place in the experimental session clearly involved the implicit or explicit discovery of rules of language(s). The brain region activated in both learning conditions was in the right inferior frontal gyrus which has been independently shown to be relevant to episodic memory recognition and retrieval. The fact that this region was activated in both conditions indicates that in explicit learning, any type of dependency (linear or hierarchical) is monitored; however, acquisition of language competence cannot rely on this type of monitoring. In all, the relevance of these experiments to foreign language learning vs. first language acquisition implies that hierarchical dependencies of the natural language type are automatically and implicitly processed in Broca's area.

It is important to point out that Musso *et al.*'s experiments which revealed a selective engagement of Broca's area in the "real" rule type, is reminiscent of a language learning study, conducted offline, with the polyglot-savant Christopher and a control group of first year linguistics undergraduates (Smith and Tsimpli 1995). An artificial language, Epun, was presented to the participants in a series of learning sessions, through the written form primarily (since this was Christopher's preferred modality for language input). Epun included rules involving hierarchical structures but also some "unreal," linearly based rules. Although this was a behavioral study, the results revealed that structure-independent operations were not mastered by Christopher but were mastered by the control participants who presumably used general-purpose learning mechanisms to deduce the rule on the basis of the controlled input presented. On the grounds that Christopher has, independently assessed, deficits in central cognition while, at the same time, his language system is intact insofar as the formal component is concerned, the results obtained can be "translated" into the brain imaging results that Musso *et al.* (2003) report in healthy adult foreign language learning.

The convergence of the results from behavioral and brain-imaging studies is particularly encouraging for identifying a plausible domain for the language instinct, namely the implementation of discovery mechanisms (in learning or acquisition) specific to language. Combining this with the findings from lesion studies and from functional imaging data on language processing by healthy adults, we can also conclude that natural language sentences involving hierarchically based dependencies implicate a specific brain region, namely Broca's area. On the other hand, the findings reviewed by Grodzinsky and Friederici (2006) and the study of Santi and Grodzinsky (2007b) establish different brain regions activated for syntactic processing depending on the nature of the dependency involved (movement *versus* anaphoric dependencies) with working memory effects specific to filler-gap dependencies only. Local structure-building operations (namely Merge), and integration processes of lexical and syntactic information also activate partly different brain regions, a finding which appears to reconcile, to some extent, theories of localization of syntactic

knowledge in the brain with theories concerned with real-time language processing. In all, neuropsychological findings appear to support the exclusive role of Broca's area in the representation, processing, and acquisition of structural dependencies, particularly those which involve displacement (a shared property of all natural languages).

Is this conclusion then sufficient to identify the language "organ" as the main biological construct of the language instinct? The answer is, for the moment, inconclusive. Having identified one or more brain regions which are critically involved in language processes does not establish neuro-anatomical separability of these regions. What can be established is that, for some computational aspects of language, there is neural specificity in Broca's region, reflecting the cognitive notion of domain-specificity for language. However, in the absence of cases of individuals with spared cognition but language abilities radically impaired with respect to the fundamental property of recursion (see [Section 5.2](#) above), we can remain sceptical about the claim for neural specificity of one or more brain regions for language.

5.4 The instinct for syntactic categories

As discussed in [Sections 5.2](#) and [5.3](#) above, recursion is not specific to language alone. It characterizes other aspects of human cognition such as music, arithmetic, and theory of mind ([Hauser et al. 2002](#)). For example, the parallelism between language and music structures ([Lerdahl and Jackendoff 1983](#); [Jackendoff 2009](#)) involves their hierarchical and headed nature – evincing the application of Merge – but extends to structural recursion as well. In this respect, both local phrase-structure building and higher-level computations as in structural dependencies are common to music and language.

This parallelism is further analyzed on the grounds of similar processing resources which have received supporting evidence from neuroimaging studies ([Maess et al. 2001](#); [Patel et al. 1998](#); [Levitin and Menon 2003](#)). Specifically, harmonic music processing has been shown to involve Broca's area and its right hemisphere homologue. On the other hand, neuropsychological evidence reports many cases of acquired or developmental impairment in music (amusia) not accompanied by language impairment ([Peretz et al. 1994](#); [Aydote et al. 2000](#)). Similarly, the polyglot-savant Christopher showing intact syntactic processing abilities in, overall, impaired cognition shows no special abilities or disabilities in music perception ([Smith et al. 2011](#)).

To accommodate the contradictory facts, [Patel \(2003\)](#) proposed the Shared Syntactic Integration Resource Hypothesis (SSIRH) arguing for a distinction between brain regions responsible for processing as opposed to those responsible for knowledge representation. This distinction is

reminiscent of the quest for overlapping regions in the brain map of syntactic knowledge as compared to syntactic processing (Grodzinsky and Friederici 2006), discussed in Section 5.3 above. The SSIRH suggests that language and music processing share frontal brain regions responsible for the computation of hierachically structured sound sequences while the distinct representational systems for music and language involve posterior brain regions (Patel 2003, 2008). The attempt is thus to compromise the neuropsychological evidence that reveals dissociations between language and music in impairment with the brain imaging evidence from processing regions by identifying differences at the representational level.

A plausible candidate for representational differences is the building blocks of the representation, i.e. the units which trigger the operation Merge in each domain: tones in music and grammatical features, morphemes and/or words in language (Pinker and Jackendoff 2005; Jackendoff 2009). Abstracting away from morphological expressions of linguistic units, one defining feature of natural languages is the nature of the categories which participate in syntactic structures. Based on behavioral data from acquisition and impairment, two general classes of categories were proposed, the open-class (content, lexical) and the closed-class (functional).

The distinction between lexical and functional categories was based on a number of criteria such as semantic content and productivity, while typical language acquisition data from a large number of languages usually showed that the earliest utterances consist mostly of open-class items (Gleitman and Wanner 1982). In the SLI literature it has been repeatedly shown that functional categories are primarily affected both in terms of omission and commission errors in production, and in terms of incomplete or deficient syntactic analysis in sentence comprehension tasks (e.g. Schuele and Tolbert 2001; van der Lely and Harris 1990). These descriptions are largely indisputable although the theoretical approaches proposed to account for the attested linguistic behavior as well as the underlying causes may be fundamentally different (Leonard 1989; Rice and Wexler 1996; Clahsen 1989). Finally, agrammatism in Broca's aphasia crucially affects morpho-syntactic production in that speech dysfluency is accompanied by errors (omission or substitution) affecting functional categories primarily. Although in both SLI and agrammatism word-finding problems are also attested, the primary cause of the deficit is associated with the developmental delay or inaccessibility of the set of functional categories and features (Gopnik 1990a, 1990b; Grodzinsky 2000).

Since Borer's (1983) seminal work on the exclusive role of the inflectional system in parametric variation, formal linguistic theory shifted its attention to functional categories and the parametric values encoded on their respective lexical entries (Chomsky 1986b, 1991). Syntactic variation was therefore regarded as variation in the formal features of individual

lexical items belonging to the functional category set, rather than to properties of constructions as such. The Minimalist Program (Chomsky 1995b) moved several steps further with respect to syntactic computations; distinctions between (abstract) morphological features, such as +/- interpretable, +/- intrinsic, and +/- valued (Chomsky 2001) motivate the formation of dependencies, their legitimacy, and the status of the derivation at the two interface levels, the phonetic (sensory-motor) and the semantic (conceptual-intentional). Accordingly, each lexical item is specified for a cluster of features, not all of which are instrumental for the derivation: interpretable features do not trigger operations but they participate in dependencies formed with uninterpretable features for the latter's benefit.

The shift from functional and lexical categories to features specified on lexical items has allowed linguists to examine in far more detail subtle properties of syntactic structures, cross-linguistic differences as well as atypical grammars such as first or second language grammars, bilingualism, developmental or acquired language disorders. This shift also allows us to escape from the usual problems with the classification of many lexical items as functional or lexical. For example, prepositions in many languages but also within the same language exhibit a range of "lexicality" (Kayne 1994); even within the traditionally lexical categories of verbs and nouns there are "light" members which are more readily classified as functional on the basis of their semantic content and syntactic role (Kayne 2009).

This level of sophistication in the linguistic analysis, however, is not easily translated into neurophysiology or brain imaging findings. There has been extensive research on the processing and impairment patterns of the major syntactic categories, verbs and nouns, which has revealed a consistent advantage for nouns in typical first language development (Fenson *et al.* 1994; Nelson 1973) – although there are exceptions (Korean; Choi and Gopnik 1995 and Mandarin; Tardiff 1996) – and SLI (Conti-Ramsden and Jones 1997; Black and Chiat 2002), while double dissociations have been found in acquired language disorders (McCarthy and Warrington 1985; Miceli *et al.* 1984). In SLI children, verbs are the affected category in terms of exhibiting reduced production compared to nouns and in being used uninflected. Moreover, verb production by the SLI children in Conti-Ramsden and Jones's (1997) study was heavily input dependent in that the verbs children produced were used in the same session by the parents.

Impairment with verbs and nouns (anomia) doubly dissociates in acquired language disorders, as shown by various studies. Verb retrieval difficulties are usually associated with non-fluent aphasia (Broca type) while noun retrieval difficulties typically characterize fluent aphasics, although language-specific effects of, for example, morphological transparency or complexity on nouns and verbs may play a role (Kehayia and

Jarema 1991; Kambanaros 2008). In general, evidence in favor of a link between noun *versus* verb impairment and lesion site has been found: lesions in posterior areas of the left hemisphere usually characterize anomia while lesions in frontal areas characterize verb retrieval difficulties (Goodglass and Kaplan 1983; Daniele *et al.* 1993, 1994; Damasio and Tranel 1993). Nevertheless, these links are not without exceptions as shown by other studies (Tranel *et al.* 2001).

Differences between nouns and verbs in acquisition and impairment are intriguing for various reasons. First, they challenge the notion of a homogeneous class of lexical categories. Second, they trigger questions about universal properties of verbs and nouns across languages, regardless of morphological features specified on each category in language-specific ways. Third, they trigger questions as to whether the problems with verb or noun retrieval are problems with syntactic categories or problems with different types of information attached to the lexical entry of a verb or a noun – phonological, semantic, or syntactic.

It is well known that certain semantic and perceptual features can create saliency and accessibility distinctions within the noun class (Kiehl *et al.* 1999), such as concreteness and imageability. Similarly, in the verb class, action verbs differ from psychological or mental state verbs in both concreteness and imageability. One of the open questions is whether these features are expected to create differences in the representation of syntactic categories of nouns and verbs or in their processing and retrieval (this is the question we considered earlier with respect to Syntax). There are proposals which capitalize on the semantic (Bird *et al.* 2000), the syntactic, or the lexical differences (Caramazza and Hillis 1991). The more general question is whether these differences between nouns and verbs reflect grammatical category differences or some of the semantic, perceptual, or other features of lexical items (Shapiro and Caramazza 2003).

There is some evidence that points to the independence of semantic features (e.g. imageability) from grammatical category distinctions (Berndt *et al.* 2002). Furthermore, Shapiro *et al.*'s (2000) study of two patients exhibiting double dissociation in noun and verb production failed to use nonce words of the affected category in a sentence-completion task. Since lexical semantics was not involved in this case, the suggestion is that at the level of syntactic computation, grammatical category is processed independently of meaning. This claim is compatible with the minimalist idea that Merge should include categorial features of lexical items drawn from the Numeration, but at the same time predicts that categorial features become accessible earlier than other morphological, semantic, or thematic features.

To summarize the verb–noun distinction, the findings are on one hand revealing in showing that verbs and nouns are discriminated in pathology, acquired or developmental, although the pattern in SLI is consistently in favor of noun production. In acquired language disorders, either category

may be affected although the locus of the lesion and the deficit in noun or verb retrieval are related. The question as to whether the grammatical category distinction itself is the driving force of the dissociation or whether semantic and perceptual feature distinctions in nouns and verbs are responsible for the effects is not resolved yet. However, there is evidence from a few studies showing that grammatical category distinctions are “real” neuropsychological notions, relevant at the level of syntactic processing.

Going back to the traditional distinction between lexical and functional categories, it was shown to have been abandoned by the current version of generative theory in favor of a feature-driven analysis of lexical items and syntactic derivations. It is interesting to note that evidence from an event-related functional magnetic resonance imaging study undermines the lexical-functional distinction as well. Friederici *et al.* (2000) examined this distinction using concreteness and task demand (syntactic versus semantic) as added variables. Functional categories included prepositions (locative and abstract) and adverbs (of location and abstract) while lexical categories included concrete and abstract nouns. An issue of prototypicality also arises, since abstract content (lexical) categories are non-prototypical in the same way that concrete function words are non-prototypical. The main findings supported the distinction in brain regions based on the nature of the task, i.e. semantic vs. syntactic, rather than on the lexical-functional distinction itself, although prototypicality effects were also attested: BA 44 which has been shown to be active during syntactic processing in other studies (Stromswold *et al.* 1996) showed increased activation in the syntactic task of Friederici’s study in the processing of abstract nouns. This finding is explained by reference to the fact that syntactic processing involves processing of grammatical categories and as such non-prototypical members of a grammatical category induce higher activation. The relevance of this study to our discussion is primarily that it undermines the functional-lexical distinction but, on the other hand, it suggests that some semantic features such as concreteness are processed together with grammatical category information during syntactic processing.

In all, although there is some evidence for specific brain regions being responsible for the processing and representation of grammatical categories, this evidence is not conclusive insofar as the noun–verb or the functional–lexical distinction is based on syntactic or semantic features or a combination of the two. What is clear is that syntactic categories are processed as such in the brain region (Broca’s area) activated during syntactic processing (discussed in Section 5.3). In this respect, the co-occurrence of categorial with other features on lexical items prevents us from isolating the possibly unique status they enjoy in human cognition.

A related but different possibility would be to generalize further and abstract away from noun–verb, lexical–functional, concrete–abstract and

so on, despite the evidence for double dissociations and/or activation of distinct brain regions in the processing of these features. Instead, we can concentrate on the fact that any word in natural languages is specified for a number of features which, nevertheless, do not appear to be decomposed by the child learner during the process of lexical acquisition. Thus, although certain features on words do seem to take priority over others in the schedule of development (e.g. concrete before abstract, nouns before verbs etc), lexical acquisition has been argued to exhibit the more general property of “fast mapping” (Carey and Bartlett 1978). Fast mapping refers to the ability to learn aspects of meaning of a word (primarily of concrete nouns or colors) on the basis of very few exposures without training or feedback. Although fast mapping has been considered a domain-general ability (Tomasello 1999; Bloom 2001), others suggest that a more elaborate version of it which includes both “easy” and “hard” words is tuned to language alone (Gleitman *et al.* 2005). This property at the lexical level arguably encompasses the spirit of the poverty of the stimulus (PoS) argument; PoS is the nativist answer to language learnability questions and perhaps, as discussed in the following section, the most powerful candidate for a language instinct (Pinker 1994).

5.5 The poverty of the stimulus argument and the language instinct: Lexical acquisition

One of the main arguments for linguistic nativism concerns the outcome of language acquisition: the language acquired is underdetermined by the input, i.e. the data available to the child (Chomsky 1980). In essence, the claim is that native speaker competence cannot be accounted for by reference to linguistic experience that the learner is exposed to throughout development, exclusively. The PoS argument has been discussed both with respect to the acquisition of grammar and the acquisition of the lexicon (although the lion’s share has been given to the acquisition of syntax; Lightfoot 1989; Crain 1991; Wexler 1991). In order to test the PoS argument many studies have concentrated on various syntactic (Kimball 1973; Baker 1978; Crain and Nakayama 1987; Lidz *et al.* 2003) phenomena, with few studies discussing morphological word-formation (Gordon 1986).

Abstracting away from the search for PoS evidence for specific syntactic phenomena, language acquisition on the whole implies the ability to produce and understand an infinite number of sentences most of which have not been encountered in the learner’s previous linguistic experience (input). Thus defined, the PoS argument is hard to counteract while, at the same time, it is generalizable to other types of knowledge humans acquire which happens also to be underdetermined by experience. In this respect, domain-general innate properties of human cognition could be held responsible for acquisition (Berwick *et al.* 2011).

The PoS argument can be empirically evaluated on the grounds of four notions (Pullum and Scholz 2002). First, one needs to establish the *acquirendum*, i.e. to define the phenomenon that needs to be learned. Second, the notion of indispensability defines the critical input which will be necessary for the learner to acquire the phenomenon in question. The third point is the inaccessibility question, i.e. that the necessary input is not really accessible to the learner, thus creating a poverty of the stimulus learning context. This is where most of the PoS reasoning stems from: the degeneracy, incompleteness, and paucity of the input (Hornstein and Lightfoot 1981; Haegeman 1994; Seidenberg 1997). Finally, the acquisition of the phenomenon discussed should be identified at the earliest possible stage, thus signaling the active involvement of inherent constraints which aid the learner to form a hypothesis space and consider a restricted set of alternatives within that space; this is the learnability question (Laurence and Margolis 2001; Stromswold 1999; Yang 2000).

Most of the criticism against the PoS argument for language acquisition comes from empiricist-based approaches which seek to establish the close (if not causal) link between the statistical analysis of the input (probability counts) and the learner's outcome (Tomasello 2003; Bates and McWhinney 1989). In most of these studies, the emphasis is placed on syntax since the generative PoS argument was primarily established or exemplified by reference to syntactic phenomena, such as the distinction between Aux and V (Stromswold 1999) or auxiliary raising in yes-no interrogatives (Crain and Nakayama 1987; Berwick *et al.* 2011). The Lexicon is not *prima facie* a candidate for the PoS argument, since the mapping between concepts and word labels can include a process of association between an observable object and its name uttered during the observation. Still, questions as to how many instances of this exposure and whether cross-situational learning for statistical analysis are needed, pertain to PoS: if it is established that lexical acquisition in the cases of observable objects can be accomplished on the basis of limited exposure and in a context where unambiguous reference is hard to establish (the word-to-world mapping problem), the PoS argument holds for lexical acquisition as well (Gleitman *et al.* 2005). Furthermore, the acquisition of words which are not concrete, observable objects, such as actions, events, mental state verbs, adjectives, and adverbs challenges the claim of the PoS argument as impertinent to lexical acquisition.

The acquisition of the lexicon has been argued to involve external factors such as social interaction in the form of joint attention (Tomasello 1992), internal factors such as a conceptual repertoire with the potential to include a variety of objects and events (Spelke 1983), and a machinery responsible for statistical, cross-situational learning (Yu and Smith 2007; Vouloumanos 2008). If these factors suffice, there

is no need to invoke a stimulus poverty argument for lexical acquisition. Furthermore, if these factors are indeed necessary for lexical acquisition, then the lexicon would appear to be more dependent on cognitive prerequisites and domain-general properties of human cognition than grammar acquisition. To some extent, the distinction between the development of grammar and the development of the lexicon could indeed be different insofar as the age factor, in some weak or strong version of the Critical Period hypothesis, does not seem to be relevant (Markson and Bloom 1997). Nevertheless, this lack of an age effect may be related to the fact that newly acquired words are immediately integrated into already established lexical representations comprising syntactic, semantic, and morphological features which enter into the relevant paradigms. This “scaffolding” could in turn be responsible for the lifelong ability to learn new words since the way they are learned is based on a restricted set of distributional criteria within language (syntax) and a set of cognitive principles which characterize pragmatics and relevance (Sperber and Wilson 1995).

The question to ask then is whether lexical development is qualitatively different in early stages compared to adulthood and if so, in what way. The answer to the first part of the question is positive: children acquire nouns earlier than verbs regardless of the fact that adult input consists of full clauses and, in certain languages, verbs are much more richly inflected and salient than in English (Gleitman and Newport 1995). Moreover, of the nouns children acquire, concrete take priority over abstract. Further distinctions within the verb category are relevant so that mental verbs are delayed compared to action verbs and so on. We should now turn to the second part of the question: in what way is early lexical development different from adult acquisition of new lexical items?

With respect to social prerequisites, such as interaction and joint attention, these are required, observed, and necessary for the concrete nouns more than for unobservable events, emotions, or mental states; since children acquire concrete nouns earlier than other categories (syntactic or semantic) they do not appear to differ from adults in these social prerequisites. To the extent that statistical learning is essential for lexical acquisition, it should be so for the early words as much as for the later ones. What we are left with is the possibility that the conceptual repertoire of children is incomplete (Carey 1994; Merriman and Tomasello 1995); thus, new labels for concepts which are cognitively unavailable cannot be acquired due to the incomplete mapping process.

This suggestion, as well as the claim that lexical acquisition relies on statistical learning have been challenged by recent experimental work exploiting the Human Simulation Paradigm (Gillette *et al.* 1999; Medina *et al.* 2011). Adult learners were exposed to muted, natural, rather than artificially created word-learning contexts (cf. Smith *et al.* 2010), extracted from mother-child interactions. In Gillette *et al.* (1999) the

rationale was to test whether observation in cross-situational learning can establish lexical acquisition (in the form of guessing) thus simulating child lexical acquisition. If adults performed differently from children in the pattern of word guessing, the implication would be that the indisputable differences between adults and children in cognitive maturity, hence in conceptual apparatus, are responsible for the pattern of “easy” words first, “hard” words later, discussed above (Gleitman *et al.* 2005). If, on the other hand, adults revealed a pattern similar to that of children (nouns > verbs), then the conceptual difference hypothesis would be undermined. The results obtained reveal a vast advantage for nouns over verbs in all experimental conditions. Notably, the condition in which accuracy in verb guessing was significantly higher was when the syntactic frame in which the word occurred was presented (written) while the extralinguistic context and noun knowledge were withheld. Moreover, a crucial difference between verbs denoting actions and those denoting mental states was also found: the learning contexts in which the former were identified were those in which mental state verbs could not be identified, indicating that syntactic frames are necessary and sufficient for the acquisition of verbs which do not denote observable actions. These results provide further evidence for the restricted scope of word-to-world mapping in early vocabulary acquisition and the subsequent, syntactic bootstrapping referred to as structure-to-world mapping (Gleitman 1990). Crucially, the claim is that verb acquisition presupposes some syntactic development – in the form of hierarchical representations – so that structural constraints guide the learner to identify argument structure properties of verbs (Lidz and Gleitman 2004).

In Medina *et al.*'s (2011) study, the challenge addresses statistical approaches to language learning which are based on domain-general learning mechanisms. According to these approaches, lexical acquisition operates in terms of cross-situational learning based on probability counts of occurrences of lexical items which are stored and evaluated. The experiments in Medina *et al.* show that “fast mapping” can be really fast: lexical learning can be achieved on a single exposure with neither alternative conjectures nor past learning contexts retained in memory.

In all, lexical acquisition reveals an interesting asymmetry in the developmental pattern: an early, pre-syntactic mapping and a later, structure-based building of the lexicon which makes lexical acquisition of any syntactic category possible. We can thus reconsider the minimalist, unidirectional mode of structure-building, namely from lexical choices (enumeration) to derivations, through the evidence which points to the critical role of structure for shaping language units in the lexicon.

Where does this take us in our search for the language instinct? To the discovery of a cognitive ability which exclusively combines hierarchical representations with units whose substance can only be fully conceived within the system that creates them.

5.6 In lieu of an epilogue: Christopher's instinct

Christopher, an atypical autistic individual, with a strong asymmetry between verbal and non-verbal abilities, apraxic, and institutionalized throughout his adult life, has intact lexical and syntactic abilities in his first language, English (Smith and Tsimpli 1995; Smith *et al.* 2011). Christopher is also an atypical foreign language learner: he can learn vocabulary and morphology of new languages very fast with minimal exposure. His syntactic abilities in the non-native languages he is familiar with are challenged in areas where differences between the native and the “other” languages are found, primarily at the level of word-order variation. Cross-linguistic differences in terms of morphology and the lexicon are easily and accurately accommodated and used. He is able to process, produce, and offer judgments on syntactic dependencies of various levels of complexity and embedding, provided they do not challenge his processing resources, as in the case of garden-path structures.

The rationale in this chapter has been to look for the language instinct in computations and properties attributed and possibly dedicated to Language. Having considered recursion in structure-building, syntactic dependencies, and processing, the search shifted to the units which trigger the relevant operations, namely syntactic categories and, eventually, words. Unsurprisingly perhaps, words cannot be defined independently of syntax. Moreover, and more importantly, words cannot be acquired independently of syntax. Christopher's instinct for language fits this interdependence; it remains to be seen whether this is a way of approaching the claim for a language instinct as well.

6

Sensitive phases in successive language acquisition: The critical period hypothesis revisited

Jürgen M. Meisel

6.1 Biological foundations of the language making capacity

This chapter is about the human Language Making Capacity (LMC) and its fate across the lifetimes of individuals. The starting assumption is that the language capacity is rooted in biology and transmitted genetically, although its phylogenetic evolution and the ontogenetic development of its specific manifestations are shaped in substantial ways by cultural factors. The LMC is furthermore assumed to be subserved by species-specific biological and cognitive properties as well as by properties shared with other species. The issue to be discussed in the present contribution concerns the way in which this capacity determines language acquisition in the individual. Although “innate,” it becomes available to the individual as a result of neural maturation, provided it is activated during specific developmental periods by exposure to language in communicative settings. This kind of interaction between biological and environmental factors is familiar from the development of behavioral capacities in other species, and it has also been observed in first language acquisition. Second language acquisition, however, is unique to humans, and we can therefore not turn to research investigating other species in order to learn about the fate of the language capacity in successive language acquisition. In what follows, I will argue that in spite of its activation in first language development, the LMC does not remain fully accessible beyond certain age ranges and that first and second language acquisition therefore exhibit a number of crucial differences.

The LMC can be conceived as a cognitive system consisting of multiple components. Since only some of them are arguably domain-specific, it is useful to distinguish between the LMC, comprising domain-specific as well as domain-general cognitive operations, and the LAD (Language Acquisition Device) which assembles only domain-specific principles and mechanisms, subserving exclusively the acquisition and processing of formal properties of human languages. Although the debate on the role of the LAD in language acquisition has focused largely on principles of Universal Grammar (UG), it should be obvious that the LAD consists of more than just UG, considered as the central component of this capacity but not as *the* acquisition device; see Carroll (2001). Rather, the LAD comprises, in addition to *representational knowledge* (UG principles) guiding language development, domain-specific *discovery* and *processing* mechanisms, the former bootstrapping the child into the grammatical system; cf. Meisel (2011, Chapter 2). This claim is best illustrated by referring to the impressive research carried out over the past 20 years demonstrating that children, already during the first year of their lives and arguably even on prenatal exposure to language, focus on just those properties of linguistic utterances which are crucial for the discovery of formal aspects of language, in general, and of the specific properties of their individual target languages, more specifically; see Guasti (2002, Chapter 2). In sum, it is useful to distinguish between the Language Making Capacity and the Language Acquisition Device, the latter comprising UG as a central component. The present contribution will then focus on an inquiry as to how the LAD fares across the lifespan of individual learners.

The fact that children exposed to one (L1) or more (2L1) languages acquire the underlying grammatical system(s) of their first language(s) successfully (ultimate achievement), within a surprisingly short period of time (developmental rate), proceeding through the same, strictly ordered developmental sequences (*uniformity* of development), corroborates the claim that the LMC indeed guides L1 acquisition. Research carried out within the theory of UG, has, in fact, focused almost exclusively on the role of UG or of the LAD in this process.

Importantly, when compared to monolingual L1 learners, children growing up with two or more languages simultaneously do not exhibit qualitative differences in the course of acquisition of their languages or in the kind of grammatical knowledge attained. Simultaneous acquisition of languages has therefore been qualified as *multiple first language acquisition*; for a summary of research results on early bilingualism see De Houwer (1995). This conclusion is primarily based on the following observations: (1) linguistic systems are differentiated from early on, (2) grammatical development proceeds through the same developmental sequences as in monolingual acquisition, and (3) the grammatical knowledge ultimately attained in each of the languages of multilingual children is identical in nature to that of their monolingual counterparts. Consequently, these properties of 2L1, just

as in L1, can be attributed to the availability of the LAD; i.e. both bilingual and monolingual L1 acquisition are guided by principles of UG, and the presence of another language in the environment and in the minds of learners does not result in substantive differences between 2L1 and monolingual L1 development – at least not when languages are acquired simultaneously from birth. The LAD is thus an endowment for multilingualism which, however, can be argued to fail when it is not activated during the appropriate developmental period.

6.2 The Critical Period Hypothesis for language acquisition

One fundamental property characterizing both monolingual and multilingual first language acquisition is that children are exposed to the target language(s) from birth. In fact, there is growing evidence that prenatal exposure to the sounds of language already triggers the acquisition process. The claim that first language acquisition is always successful – leaving pathological cases apart – thus refers to a scenario in which children interact with caretakers and peers during the first years of their lives. One can therefore surmise that the genetically transmitted human language capacity initiates the process of linguistic development and, if stimulated by exposure to the primary linguistic data in an adequate social setting, guides it during the subsequent course of acquisition. The question at stake then is whether the LMC remains available indefinitely, independently of whether it is activated during a child's early years or not. There are reasons to believe that this is not the case, and a number of factors can be hypothesized to cause this change, or possibly the deterioration of this faculty. Most importantly in the present context, neural maturation seems to open but subsequently also close windows of opportunities for the language-acquiring child. A causal relationship of this sort between maturational changes and changes in the language acquisition capacity is, of course, what is postulated by the *Critical Period Hypothesis* (CPH) first proposed by Penfield and Roberts (1959) and elaborated in some detail in the seminal work of Lenneberg (1967).

There can indeed be little doubt that (monolingual) first language acquisition is subject to critical period effects, as becomes evident in the careful review of the literature on this topic by Hyltenstam and Abrahamsson (2003). The same conclusion can, in fact, be drawn from the discussion by Gleitman and Newport (1995) who present an overview of different types of L1 development including cases of partial or total deprivation from input and of late exposure, as for example with deaf children of hearing parents who were exposed to sign language only at later stages of development. Importantly, these authors provide evidence for the robustness of the language acquisition device, even in cases where

exposure to language is drastically reduced or onset of acquisition is delayed. But they also emphasize the effects of a biologically determined maturational schedule.

As for the age range during which such changes happen, the limited empirical evidence currently available makes it difficult to define this precisely. Yet the age around puberty, suggested by Lenneberg (1967), clearly sets the cut-off point too late. In fact, Penfield and Roberts (1959: 236) already proposed the age span from birth to around age 9 as the critical one, during which children are able to acquire a first language by exposure to the primary linguistic data. They argued that “the human brain becomes progressively stiff and rigid after the age of nine.” If, however, we ask when such effects begin to emerge, we find that Gleitman and Newport (1995) report on subtle effects as early as of an age of onset of acquisition (AO) at 4 years, and massive effects after AO 6, e.g. in delayed L1 acquisition of American Sign Language (ASL). These findings are in line with what we know about the dramatic cases of children who were partially or totally isolated during early childhood. When AO is delayed beyond age 6 or 7, language acquisition differs substantially from that of normally developing children. The well-documented case study of Genie provided particularly strong evidence in support of the assumption of a critical period for first language acquisition since it revealed that she never acquired a grammatical competence remotely similar to that of native speakers; see Curtiss (1977). Genie’s case does not, however, allow us to delimit the critical age range more precisely because she was isolated during her second year of life through the age of 13 years and seven months.

Subsequent research has demonstrated that the original version of the Critical Period Hypothesis needs to be revised not only with respect to the critical age range but also in a number of further aspects, e.g. concerning the alleged causal role of the functional lateralization of the brain; see Hyltenstam and Abrahamsson (2003) for a state-of-the-art discussion. These revisions do not, however, affect the fundamental concept of the CPH, and yet it has met with much scepticism among acquisition researchers. Admittedly, empirical studies produced conflicting results. But those arguing against a CP in successive acquisition typically refer to only some aspects of grammar which appear to be used in a native-like fashion; see Abrahamsson and Hyltenstam (2009: 253). Moreover, much of the criticism seems to be due to the fact that conceptualizations of the CPH frequently suffer from insufficiently precise definitions of the notion of “critical period” (cf. Eubank and Gregg 1999), occasionally covering several distinct hypotheses, as observed by Birdsong (1999). In what follows, I will attempt to define this notion of “critical period” more restrictively, in accordance with recent insights gained by research in linguistics as well as in the neurosciences. This will hopefully contribute to a somewhat less controversial debate.

6.3 Successive acquisition: Differences between first and second language acquisition

Successive language acquisition is clearly not characterized by the above-mentioned properties attributed to monolingual and bilingual first language acquisition. Rate of L2 acquisition is typically protracted, and, contrary to the uniformity of L1 across children, one finds a broad range of *variation* in L2, across individuals and within learners over time. Most importantly, it is obviously not the case that all L2 learners are *successful*. Whether it is in principle impossible to acquire native competence in a second language is a matter of controversy, but near-native or nativelike learners are clearly the exception. In fact, nativelikeness can apparently only be attained if AO happens in childhood, possibly before age 8, but never by adult learners; cf. Abrahamsson and Hyltenstam (2009).

The claim that first and second language acquisition differ in these respects is in itself not a controversial one. However, whether the observed differences are caused by age-related maturational changes and whether they reflect substantive differences in the acquired grammatical knowledge are indeed questions leading to much controversy. Concerning the first one, I should at least mention the frequently cited study by Johnson and Newport (1989). Based on an evaluation of grammaticality judgments, they argued that age of onset can be considered as the most crucial variable predicting L2 proficiency, rather than, for example, length of exposure. For a discussion of these and related findings, I want to refer again to the state-of-the-art summary by Hyltenstam and Abrahamsson (2003) and to the overview offered by DeKeyser and Larson-Hall (2005). Since some of the more serious objections raised against the conclusions by Johnson and Newport (1989) relate to methodological problems, it should be noted that DeKeyser (2000) carried out a replication study taking these methodological concerns into account, adding to this a language learning aptitude test. He was able to confirm that age of onset must be considered as a crucial variable determining attainable L2 proficiency (AO before or after 16). In fact, DeKeyser and Larson-Hall (2005) concluded that a maturational decline in the language learning capacity indeed exists, arguing that adults rely largely on explicit learning, whereas children tend to learn implicitly. But these authors emphasize that effects of age of onset are probably not caused by maturational changes alone. This is also the underlying assumption of the present discussion, and I will return to this issue below.

The second question concerns the issue of whether observed differences across acquisition types reflect different kinds of grammatical knowledge. According to the Fundamental Difference Hypothesis (FDH), first proposed by Bley-Vroman (1990), this is indeed the case, and UG is said not to be fully accessible for L2 learners. Alternatively, it has been claimed that the LAD

remains completely accessible and that the observed differences can be explained in terms of secondary factors influencing the course of acquisition; see, for example, the Full Transfer/ Full Access hypothesis propagated by Schwartz and Sprouse (1996). This scenario predicts massive transfer of grammatical knowledge from the L1 at the initial state of L2 acquisition. In terms of parameter theory, this entails the necessity of “resetting” those parameters for which the two target systems require different settings and which are set to the L1 value at the L2 initial state. Yet “resetting” is probably not possible for principled reasons, as has been argued by Clahsen (1991) and others. If this objection is correct, it entails that L2 learners need to resort to other cognitive resources in instances where they cannot rely on knowledge provided by UG like children acquiring their L1.

Importantly, assuming partial inaccessibility of UG does not exclude the possibility for some L2 learners to acquire a near-native proficiency of the target L2. Rather, it accounts for the fact that L2 learners differ strongly in how successful they are in achieving this goal. Moreover, in cases where they cannot activate UG knowledge, they need to rely more heavily on inductive learning. As a result, L2 acquisition processes are more protracted and less uniform than L1 development, exhibiting, for example, effects of hypothesis-testing by trial and error over several months in grammatical domains where L1 children succeed within a short period of time, sometimes within a couple of weeks; see Meisel (2011: 144 ff.) for a more detailed discussion. The lack of abrupt change in the linguistic behavior of L2 learners and its occurrence in L1 development not only constitute one of the empirically testable pieces of evidence supporting the claim that the two types of acquisition rely on different acquisition mechanisms, this also suggests that the acquired knowledge is different in the two cases. In other words, these observable differences are not merely properties of a particular type of language use, they reflect rather differences in the nature of the underlying linguistic knowledge; see Meisel (2011: 191 ff.).

Note that the FDH, as it is advocated here, does not claim that L2 learners are not guided by the LAD at all. Instead, it argues that some of the principles and mechanisms which shape L1 become inaccessible in the course of development and that these changes are partly due to neural maturation. This concerns primarily access to principles of UG. In fact, Smith and Tsimpli (1995) provide evidence suggesting that only *parameterized* principles are subject to maturation, thus establishing a relationship between linguistic and neural maturation and emphasizing the modular organization of mind and grammar. In other words, L2 learners are predicted not to have direct access any more to options provided by parameterized UG principles, and although they can make use of previously acquired grammatical knowledge, they cannot fix the value of a parameter not instantiated in L1, nor can they “reset” those parameter values in which the two grammars differ. Where triggering of implicit knowledge has become

impossible, learners have to rely on inductive learning. In doing so, they may resort to cognitive resources other than the domain-specific ones offered by the LAD in order to compensate for the ones which have become inaccessible. Non-parameterized principles of UG, on the other hand, constrain L2 acquisition in essentially the same way as in (2)L1. Consequently, L2 knowledge conforms only in part to principles of UG, whereas other parts are not constrained by domain-specific cognitive principles but are the result of domain-general operations.

Maturational changes can, however, affect other mechanisms of the LAD as well, not just parameterized principles of UG. As pointed out by Bley-Vroman (1990: 14), the domain-specific learning procedures are also subject to such changes. They enable a child to develop the grammar of the target language when exposed to the primary linguistic data. In other words, the discovery principles and learning mechanisms contained in the LAD, cf. [Section 6.1](#), are likely to be affected by changes in the course of development as well. This, however, need not be the result of changes caused by neural maturation. This should not come as a surprise because the Language Making Capacity is quite obviously also affected by other age-related changes. These, however, are not our concern here.

6.4 The Critical Period Hypothesis revisited: Sensitive phases in language development

6.4.1 Revisiting the Critical Period Hypothesis

In view of the insights gained by research on successive acquisition of languages mentioned in the previous section, the extension of the CPH to second language acquisition seems to be well motivated. This is of course not a logical necessity, not even if we are correct in assuming that the capacity for language acquisition fades out in first language development if it is not activated during the first months or years of life. In principle, the LMC might be protected from effects of maturational changes once it has been activated and some linguistic knowledge is represented in the mind of an individual. In other words, it is at least logically possible that, once activated, the language capacity remains available indefinitely; cf. Johnson and Newport (1989). Findings from L2 research, however, provide sufficient reasons to assume that this is not the case. Note that this is not to say that all L1-L2 differences are likely to be caused by neural maturation. Rather, as alluded to before, it is likely that a number of additional factors come into play here as well, and some of the observed differences can undoubtedly be accounted for in terms of other types of developmental changes, like for example the more mature cognitive abilities of learners at later ages of onset, their socio-psychological development, or reliance on previously acquired linguistic knowledge. In the present context, however, the focus is on neural maturation as a causal factor affecting the

availability of the LAD in successive acquisition of languages, an issue already raised by Lenneberg (1967: 176) who himself extended the CPH to L2 acquisition:

automatic acquisition from mere exposure to a given language seems to disappear [after puberty], and foreign languages have to be taught and learned through a conscious and labored effort. Foreign accents cannot be overcome easily after puberty. However, a person *can* learn to communicate at the age of forty. This does not trouble our basic hypothesis.

As becomes apparent from this quote, he emphasizes a point also made in the previous section, namely that the question is not whether language acquisition continues to be possible but why L2 learners are only partially successful. The answer proposed here is that the ability to develop a full grammatical competence by mere exposure disappears in the course of development. But Lenneberg's statement further implies that the various domains of grammar are affected to different degrees by this change, an important specification contributing to a better understanding of the CPH as applied to successive language acquisition.

In what follows, I will take both these considerations into account when attempting to further specify and clarify the notion of "critical period." The first point to be addressed is that it is not "language" which is affected by maturational changes but certain *domains of grammar*. The acquisition of lexical knowledge, for example, is not concerned at all. Consequently, the discussion of whether successive language acquisition is characterized by a critical period after which a native grammatical competence cannot be acquired should refer to evidence relating to just those domains of grammar which are hypothesized to be subject to maturational changes. If the approach summarized in the previous section is correct, *parameterized* principles of UG are the ones primarily concerned. Whether this is indeed correct and whether this is the only grammatical domain concerned or whether such changes affect other grammatical properties as well, these are questions which need to be pursued further. Yet independently of what the ultimate answers will be, the validity of the Critical Period Hypothesis can be tested empirically by focusing on the acquisition of linguistic phenomena related to parameterized principles. If first and second language acquisition result in qualitative differences in this domain, as I claim to be the case, this provides strong support for the assumption that critical period effects characterize successive acquisition in a similar fashion as first language development. This does not, of course, exclude the possibility that other domains of grammar are subject to such constraints as well.

A second point which needs to be considered when attempting to define more precisely the notion of "critical period" concerns the fact that no current theorizing, either in linguistics or in cognitive psychology or the neurosciences, obliges us to assume that the grammatical domains subject to maturational changes are all affected simultaneously, during a single

age period. Quite to the contrary, past research demonstrates that phonology, morphology, and syntax follow distinct developmental agendas; see, for example Long (1990) or Eubank and Gregg (1999). In fact, assuming that linguistic development proceeds through *sensitive phases* during which the LAD is optimally prepared for the integration of new information into developing grammars, we must expect to find asynchronous development even within these subcomponents of grammar. Consequently, rather than postulating one critical period for language acquisition, a more plausible hypothesis is that grammatical development is characterized by several *sensitive phases*; see Locke (1997), among others. If it can be shown that sensitive phases cluster at different points of linguistic development, each of these clusters can be argued to constitute the equivalent of one of multiple critical periods; cf. Seliger (1978).

The third point concerns the nature of these sensitive phases. Notions like “critical period” or “sensitive phase” do not necessarily imply an abrupt loss of a previously available capacity, as if this capacity was suddenly switched off; cf. Hyltenstam and Abrahamsson (2003: 556). Rather, a phase can be characterized by an optimal period for the acquisition of the phenomenon in question which then fades out gradually, possibly over an extended period of time, although it typically becomes accessible within a brief time span. In other words, we may expect to find a pattern characterized by a relatively *short onset*, followed by an *extended optimal period* and a *gradual offset*. This protracted offset makes it difficult to determine with desirable precision when exactly in the course of development such changes occur. My suggestion is to focus on the time when optimal periods begin to fade out, i.e. to attempt to determine the moment or the time span when the optimal period ends.

This latter remark alludes to another crucial issue, the developmental agenda determined by maturational changes of the LAD. Having argued that a “critical period” in linguistic development consists of a cluster of sensitive phases, each characterized by an optimal period for the acquisition of one out of a set of grammatical phenomena, we should try to identify the approximate developmental period, possibly in terms of age spans, of every sensitive phase, and also the periods during which such phases cluster. Note that there is no a priori reason for why phases should cluster rather than occurring independently. Yet past research has assembled empirical evidence demonstrating that periods of heightened activity do exist in language acquisition and that the course of acquisition is not always characterized by a continuous process of gradual increase of grammatical knowledge. Assuming that this is at least in part an effect of neural maturation, this observation justifies the hypothesis that sensitive phases do cluster at certain points of development. As for when this happens, we should not expect to be able to identify precise age ranges, given that the rate of acquisition varies considerably across individuals, but we can attempt to give approximate age spans.

6.4.2 Sensitive phases: Possible age ranges

This brings us to the notoriously difficult problem concerning the *age ranges* during which maturationally caused changes happen in the course of successive language acquisition. Much of the discussion of this issue refers to Lenneberg's (1967) suggestion of puberty as the relevant developmental period, ignoring the claim by Penfield and Roberts (1959) that decreasing brain plasticity justifies the assumption that such changes occur at around age 9. This widespread adoption of the hypothesis of a CP at around puberty, even in recent publications, is all the more surprising since the developmental correlation between brain lateralization and critical period effects, hypothesized by Lenneberg (1967), has long been demonstrated not to hold. Moreover, subsequent acquisition research revealed that some aspects of grammar are definitely affected much earlier; see Long (1990).

In their comprehensive state-of-the-art survey, Hyltenstam and Abrahamsson (2003: 575) arrive at the conclusion that "At least up to AOs 6 or 7, all learners will automatically reach levels that allow them to pass as native speakers – provided that there is sufficient input and that the learning circumstances are not deficient." After this age, social-psychological factors play an increasingly important role in L2 acquisition, whereas their influence is negligible during early childhood. Yet "to pass as native speakers" does not necessarily imply that learners have acquired native grammatical competences. Quite to the contrary, although the authors of this report are extremely careful when it comes to drawing conclusions concerning cut-off points in linguistic development, they do present evidence suggesting that a native grammatical competence may never be attainable in successive language acquisition; see also Abrahamsson and Hyltenstam (2009). In fact, they suggest that maturational effects happen much earlier, "perhaps as early as 12 months" (Hyltenstam and Abrahamsson 2003: 570) referring to phonological development, and they observe that maturational effects on language development are noticeable as of birth and up to approximately age 15 when, according to them, the maturational period ends.

In sum, studies reviewing the currently available knowledge on this topic mention the age span at around age 7 (here quoted as 6 to 7, elsewhere as 7–8) as one during which significant changes happen and AO of approximately 15–16 (cf. also DeKeyser, Alfi-Shabtay, and Ravid 2010) as the age period after which no age related changes are detected anymore. Moreover, recent research, referred to below, supports the finding by Johnson and Newport (1989) who argued that critical age limits affecting morphosyntax may lie before age 7, possibly between ages 4–6. In other words, if age of onset in successive language acquisition falls into this age span, the optimal period for the acquisition of some grammatical phenomena may already have begun to fade out. Consequently, critical period

effects should be detectable at different points of development between the ages of approximately 4 and 16 years.

The claim that maturationally related changes may happen as early as around age 4 is primarily based on insights gained by recent research analyzing early successive language acquisition, a previously under-researched area of investigation. In fact, studies of first exposure to a second language during the first three years are still scarce, but successive acquisition at AO between age 3 and 6 has recently been investigated more frequently; cf. Unsworth (2005). The results of these studies suggest that early successive acquisition resembles adult L2 acquisition (aL2) in a number of aspects, not only in phonology but also in morphosyntax. To avoid misunderstandings, this does not mean that the language of these children is identical in all respects to that of adult L2 learners but rather that one can find some grammatical properties which are not attested in L1 but in aL2. There is still uncertainty and disagreement on which domains of grammar are most likely to resemble aL2, see for example Schwartz (2004) or Meisel (2008); I will briefly return to this issue below. But the claim that L2 properties begin to emerge shortly before or after age 4, see Meisel (2004) or Schwartz (2004), has been corroborated in several recent studies investigating the acquisition of various language pairs in different settings; see for example Meisel (2009), Preißler (2011), Schlyter (2011), and Sopata (2009, 2011).

Importantly, corroboration of the hypothesis that L1–L2 differences are primarily due to neural maturation and that the age at around 4 years is of special importance in this respect, is also provided by research investigating changes in the functional organization of the brain. This is crucial not only because a hypothesis referring to both linguistic development and neural maturation quite obviously cannot rely on linguistic evidence alone, but also because effects of maturational changes can be “masked” in behavioral data (Eubank and Gregg 1999: 82) if learners resort to non-domain-specific operations.

Changes in the functional organization of the brain over time should result in different activation patterns and possibly in a different spatial organization of the brain in language processing if the onset of exposure to a language falls outside the optimal period, whereas no such differences should emerge if first exposure falls within this period. In the present context, Broca’s area, encompassing the *pars opercularis* of the left inferior frontal gyrus (BA 44) and the posterior portion of *pars triangularis* (BA 45), is particularly relevant, for it plays a crucial role in syntactic processing during sentence comprehension. The type of evidence we are looking for is provided by studies using electrophysiological or haemodynamic methods (e.g. functional magnetic resonance imaging, fMRI, or positron emission tomography, PET).

In fMRI studies, variations of cerebral activity are recorded as tomograms, i.e. slices through the brain measuring the regional cerebral

blood flow (rCBF). This, in turn, is interpreted as reflecting regional brain activation. There exists now a sizable body of research of this type, and a number of these publications found differences in the intensity of brain activation between native speakers and L2 learners in processing morphosyntactic information, and some have also detected spatial differentiation. Among the first to report findings of this type were Kim *et al.* (1997), comparing children acquiring two languages from “early infancy” with others who acquired the second language successively (11;2 average AO). They found that in early bilinguals both languages are processed in largely overlapping regions in Broca’s area, whereas processing of the two languages is spatially separated in successive bilingualism. Dehaene *et al.* (1997), too, observed that L2 processing relies on larger and spatially more diffuse networks; they concluded that L1 but not necessarily L2 relies on a dedicated left-hemispheric cerebral network. They further reported on more brain activation in the temporal lobe and in the right hemisphere and more individual variation in L2 learners when compared to native speakers. Although some of these studies have been criticized for methodological shortcomings, the most crucial aspects of their results have been corroborated by more recent brain-imaging research. Wartenburger *et al.* (2003), for example, confirmed that brain activities in morphosyntactic processing depend on AO (critical age 6 years). Interestingly, proficiency level turns out to be a crucial factor in semantic but not in syntactic processing. Whereas these studies contrasted monolingual L1 and L2 speakers, Saur *et al.* (2009) compared subjects who had acquired French and German simultaneously to French L2 learners of German and German L2 learners of French, focusing on the processing of word order variation. This analysis revealed similar patterns of activation in both L2 groups, with higher activation in L2 than in L1 sentence processing in the left inferior frontal gyrus, the basal ganglia, and the left inferior temporal gyrus. Early bilinguals, however, did not exhibit differences in activation between the two languages in these areas.

fMRI results thus confirm the finding by behavioural studies, demonstrating substantial differences between L1 and L2 speakers, and they provide further evidence suggesting that maturational changes cause such differences to occur at around age 6–7. Yet to my knowledge, research using haemodynamic methods did not inquire whether similar effects can be observed at an earlier age, possibly because, until recently, it was very difficult to perform brain scans with very young children.

ERP studies, however, have included younger children. They use electroencephalography (EEG), a method by which electrical variations induced by neural activity are recorded at the surface of the scalp. From the recorded variations event-related brain potentials (ERPs) are derived. Several ERP studies, e.g. Weber-Fox and Neville (1996, 1999), demonstrated that spatial distribution of activation patterns in the left hemisphere changes in the course of development, depending on age of acquisition,

i.e. specialization in the left hemisphere is reduced and the right hemisphere is increasingly activated. The critical age ranges seem to be at around age 4;0 years and again around 7 years, i.e. if AO happens at age 4 or later, this effect of more diffuse spatial distribution and increasing right hemispheric processing becomes increasingly stronger. Importantly, Weber-Fox and Neville (1999) and others observed not only differences in spatial distribution but also in the *quality* of ERP-responses as a result of later ages of onset. But, again, as in investigations using fMRI, such L1-L2 differences are only detected in syntactic processing, whereas semantic processing does not produce this type of effect; see Friederici (2002) for a critical summary of such findings, or Isel (2005) for a review of ERP studies investigating L1 and L2 acquisition.

In sum, neuroimaging studies support the hypothesis that age of onset of acquisition is a major cause for L1-L2 differences in processing grammatical information. They furthermore corroborate the claim that important changes happen at around age 6-7, and some ERP results confirm the assumption that crucial changes occur already at around age 4. This kind of research cannot, however, offer more detailed insights concerning the question of which domains of grammar are the developmentally earliest ones to be affected by neural maturation. Behavioral studies are better suited to shed light on this question because they can detect even subtle changes in the use of specific grammatical constructions, whereas brain imaging studies are not (yet) able to capture changes in brain activation patterns or in spatial organization reflecting such grammatical subtleties.

6.4.3 Sensitive phases: Grammatical domains

As mentioned above, the question of which domains of grammar are affected by maturational changes at an early age is still being debated. The only uncontroversial claim apparently is that some aspects of phonology count among the ones which are subject to such changes from very early on. This admittedly unsatisfactory situation is a reflection of the fact that current theorizing, in linguistics as well as in developmental psychology or the neurosciences, does not allow us to make specific predictions about developmental agendas. As should have become apparent in the preceding sections, we are able to formulate hypotheses about *which* aspects of grammar are likely to be affected, e.g. parameterized principles of UG, or about *what kind* of effects will probably occur, e.g. protracted rate of learning or uncommon brain activation patterns, but this does not tell us which of these effects will happen *at which point of development*, or which grammatical phenomena will be affected earlier or later. In view of this lack of predictive power of deductive approaches to linguistic development, we need to rely on inductive procedures, i.e. on empirical research contrasting instances of successive language acquisition at different ages of onset.

I should mention, however, that at least one study attempted to develop a theoretically based argument concerning the issue at stake here. Schwartz (2004) predicted that child L2 acquisition will be like adult L2 acquisition in the domain of syntax but not in the domain of inflectional morphology. Interpreting this in terms of possible effects of maturational changes, it amounts to saying that optimal periods for the acquisition of syntax fade out earlier than those for morphology. Yet such a claim is problematic for both theoretical and empirical reasons. Concerning the latter, even the limited knowledge about child L2 acquisition available before the recent surge in research activities, suggests that inflectional morphology is in fact more problematic for cL2 learners than most aspects of syntax. As for theoretical considerations, this claim implies a superficial and thus inadequate definition of L1–L2 differences. If, on the other hand, they are of a deeper and more abstract nature, they will necessarily manifest themselves in syntactic as well as morphological properties. As an example, think of “finiteness,” a grammatical notion relating to the placement as well as to the forms of auxiliaries and/or main verbs. Thus, if learners encounter problems with the acquisition of finiteness, this will have repercussions for word order, agreement and tense marking, and so forth, depending on how the target language encodes this grammatical notion. Consequently, it is not possible to plot syntax as a whole earlier on the developmental agenda than inflectional morphology – nor *vice versa*, for that matter. Rather, the prediction is that syntactic and morphological properties will be intertwined on the developmental axis.

Recent investigations of successive language acquisition confirm this prediction. Most importantly, it has been demonstrated that children at AO just before or after 4 frequently behave like aL2 learners in their acquisition and use of inflectional morphology. Meisel (2008), for example, studied the acquisition of finiteness in French by children with German as L1, AO ranging from ages 2;8 through 4;0. In L1, finiteness is acquired early and typically without errors in person agreement. In Colloquial French, subject clitic (SCL) pronouns enter into a close relationship with the finite verb and have been analyzed as agreement markers; cf. Kaiser (1994). Irrespective of the details of this analysis, what matters is that L1 children do not combine these elements with non-finite verb forms. Several of the cL2 learners, however, did so, and they also used constructions with two adjacent finite verbs, a pattern not attested in L1, either. The SCL + non-finite V-pattern not only makes cL2 look different from (2)L1, it also brings it closer to aL2 since Granfeldt and Schlyter (2004) have shown that this construction is used by adult L2 learners of French. According to their analysis, aL2 learners do not cliticize pronominal subjects; rather, they treat them as arguments (XPs), whereas 2L1 children analyze them as X⁰-heads from early on. This finding is confirmed by Granfeldt, Schlyter, and Kihlstedt (2007) who compared Swedish cL2 and 2L1 children with French monolinguals. The

cL2 learners (AO 3;5 – 6;7) resembled adult learners of French not only in combining SCL with non-finite verbs, but also in their use of tense forms and of gender agreement and in placing object clitics (OCL) post-verbally. Similar results have been obtained in subsequent research. Preißler (2011), for example, confirmed that subject pronouns behave like full DPs rather than SCL in the speech of cL2 French learners (L1 German, AO at around age 4). And Schlyter (2011) found that Swedish cL2 learners of French (AO at around 4 or later), behave much like aL2 learners in their use of past tense, lexical aspect, and temporal reference. She explains this primarily with reference to the children's previously acquired L1 knowledge, arguing that they rely on the system of functional categories of their L1, mapping L2 lexical items on this structure and using default forms or forms frequently encountered in the PLD. At any rate, these and similar results strongly suggest that it is precisely in the morphological domain in which child L2 resembles adult L2.

This conclusion is supported by the analysis of the acquisition of grammatical gender by German child L2 learners (AO at around age 4) of French; see Meisel (2009). Note, however, that whereas gender concord (e.g. agreement between articles and nouns) is clearly a syntactic operation, gender assignment is an operation contributing to nominal morphology. In languages like French or German where gender is neither fully motivated by semantic properties of nouns nor unambiguously determined by overt markings, L1 children have been shown to rely on phonological, morphological, and semantic cues. In instances where these provide conflicting evidence, formal cues tend to override functional ones in L1 French; cf. Karmiloff-Smith (1978). Child L2 learners of French, however, do not seem to be able to make use of these formal cues; cf. Meisel (2009). They thus differ from L1 children and resemble aL2 learners who tend to rely on functional properties (semantic, contextual, etc.) instead. Importantly, this type of gender assignment is not guided by principles of UG but by the kind of discovery mechanisms introduced in Sections 6.1 and 6.3. This suggests that not only parameterized principles of UG but discovery and learning principles too become inaccessible in the course of linguistic development.

As for word order, the German-speaking children acquiring French (Meisel 2008, 2009) do not seem to encounter problems with the acquisition of syntax. But this may be due to the fact that word order regularities of VO languages lacking verb-second (V2) effects are acquired relatively fast and without major problems by young L2 learners whose acquisition rate tends to be faster than that of adults anyway. In other words, other target languages may turn out to be more problematic for child L2 learners. In fact, in a recent study of three Polish boys learning German (AO 3;8–4;7), Sopata (2009, 2011) demonstrated that German OV order is not the preferred pattern for these children who, moreover, initially place finite verbs frequently in a target-deviant *V3 position, while at the same time

moving non-finite verbs to the V2 position, a feature unambiguously characterizing L2 acquisition. Kroffke, Rothweiler, and Babur (2007) report on similar findings in their study of two Turkish children (AOA 3 and 6) acquiring German. The acquisition of both subject-verb agreement and verb placement resembles adult L2 acquisition in the older child, whereas in the younger one both develop much like in L1 children.

In sum, the currently available evidence from child L2 acquisition strongly corroborates the claim that the nature of grammatical knowledge acquired in successive language acquisition differs from that of native L1 speakers, varying according to age of onset of acquisition. The age periods between 3–4 and 6–7 seem to be of particular importance, in this respect. If age of onset of acquisition happens after these age spans, child L2 acquisition shares crucial properties with adult L2 acquisition, and both differ from monolingual as well as bilingual first language development. The grammatical features in which early child L2 resembles adult L2 acquisition undoubtedly include examples from inflectional morphology, but at least some aspects of syntax are also concerned.

6.5 Conclusions and open questions

The goal of this contribution has been to explore the fate of the species-specific Language Making Capacity across the lifetimes of individuals and to shed some light on the problem of how it enables them to develop grammatical knowledge about their target languages. Starting from the assumption that the LMC becomes available as a result of neural maturation, the crucial question is whether it remains available indefinitely or whether it needs to be activated during specific developmental periods in order not to become inaccessible for the language learner, as predicted by the Critical Period Hypothesis.

As far as the development of a grammatical competence in a first language is concerned, there indeed exists strong evidence in support of the CPH. Although the LMC has been shown to be a rather robust device enabling learners to successfully acquire such knowledge in monolingual or multilingual settings even under unfavorable conditions, this is only true if age of onset of acquisition happens during early childhood. The more controversial question is whether critical periods also characterize successive acquisition of languages in spite of the fact that the LMC has been activated in the course of first language development. I have argued that this is indeed the case. i.e. the LMC does not remain fully accessible beyond certain age ranges. More specifically, the claim is that crucial parts of the LAD become inaccessible as a result of neural maturation, and although language acquisition continues to be possible, L2 acquisition differs in a number of crucial ways from L1 development. Three types of evidence support this conclusion: (1) research investigating the ultimate

attainment of L2 learners has shown that nativelike ultimate attainment is only possible if AO happens in childhood, probably before age 8. (2) Studies on child L2 acquisition have demonstrated that already at around AO cL2 learners differ from (2)L1 children and resemble adult L2 learners with respect to the kind of knowledge they acquire in some grammatical domains. (3) Brain imaging studies have found differences in activation patterns and in the spatial organization of the brain in language processing if the onset of exposure to a language falls outside the optimal period. Although it would be premature to conclude that a causal relationship between the changes in the functional organization of the brain and specific types of linguistic behavior has been established beyond any doubt, the developmental synchrony between neurological and linguistic changes is certainly suggestive.

The research results summarized here lead to the conclusion that critical period effects resulting from maturational changes characterize both first and second language acquisition. They do not, however, affect “language” as a whole but only certain *domains of grammar*. Moreover, it is not plausible to assume the existence of a single critical period during which all grammatical phenomena subject to maturational changes would be affected simultaneously. Rather, grammatical development exhibits a number of *sensitive phases*. Since some of them cluster during specific developmental phases, we can postulate the existence of several “critical periods,” each consisting of a cluster of sensitive phases characterized by an optimal period for the acquisition of one out of a set of grammatical phenomena.

To conclude, I would like to single out two of the remaining open questions as particularly important and urgent. The first one concerns the domains of grammar subject to critical period effects. Parameterized principles of UG have been argued to be prime candidates. Assuming this to be correct does not, however, exclude the possibility that other areas of grammar are affected as well. This possibility requires further investigation which should not be restricted to grammatical principles but would also have to explore possible critical period effects affecting mechanisms of grammatical processing. The second *desideratum* concerns the underlying maturational schedule, i.e. a principled account of which aspects of grammar are subject to critical period effects at what point of linguistic development. Insights into this currently poorly understood problem would constitute a major contribution to biolinguistics.

7

Discovering word forms and word meanings: The role of phrasal prosody and function words

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7.1 Discovering word forms

Infants in the process of acquiring their mother tongue have to learn, among other things, the words of this language. In order to build their lexicon, they firstly have to find word units in the speech stream, and then associate each word form with a meaning. In this chapter, we will discuss these two steps of lexical acquisition, and focus more precisely on the role of two cues: phrasal prosody and function words.

Processing oral speech in our mother tongue seems to us, adults, an automatic and effortless task: we identify words in sentences without difficulty and we are able to quickly compute the meaning of a sentence. This task is more difficult when we are visiting a foreign country where people use a language that we do not know: there, we do not hear individual words, but rather a continuous stream of syllables. At birth, the infant is in a similar situation: he has to discover individual words where there is only a continuous speech stream. This lexical segmentation problem is due to the fact that there are no systematic and universal cues that would allow listeners to clearly hear where words begin and end in sentences. Spoken words are not delimited by silent pauses that would play a role similar to spaces in written text. Nevertheless, it has been shown that infants are able to segment spoken sentences and identify word forms at a very young age, before they produce their first words: this capacity emerges between 6 and 7.5 months of age for English-learning infants

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(Bortfeld *et al.* 2005; Jusczyk and Aslin 1995) and between 12 and 16 months of age for French-learning infants (Gout 2001; Nazzi *et al.* 2006; Millotte *et al.* 2010); see for instance Mersad, Goyet, and Nazzi (2010), for a discussion on these cross-linguistic differences. To figure out how infants do manage to extract word forms from continuous speech, researchers have focused on several types of cues that may be available early on to infants, either because they are language-universal – common to all the languages of the world – or because they are language-specific but can be acquired on the basis of an analysis of whole sentences.

The main language-universal cue is the use of statistical or distributional information. The intuition behind the use of “transitional probabilities” between syllables or phonemes is that “sound sequences that occur frequently and in a variety of contexts are better candidates for the lexicon than those that occur rarely or in few contexts” (Brent and Cartwright 1996). The hypothesis is thus that infants might be able to compute transition probabilities between pairs of syllables, and expect word boundaries to occur whenever the transition probability is low (that is, when the next syllable is unexpected relative to what came just before). Experimental work using an artificial language showed that 8-month-old infants are able to infer that syllables that occur together frequently form a word (see, for instance, Saffran, Aslin, and Newport 1996). This capacity was demonstrated with both artificial and natural languages, by testing English (Aslin, Saffran, and Newport 1998; Pelucchi, Hay, and Saffran 2009), Dutch (Johnson and Tyler 2010), and French 8-month-old infants (Mersad and Nazzi 2010).

Still relying on a statistical analysis of the speech signal to extract information about the way speech sounds typically co-occur in their mother tongue, infants were shown to be sensitive to the phonotactic constraints of their mother tongue: two sounds that cannot occur together within words (such as the sequence /tf/ in French or /vt/ in English) are considered as belonging to two different words, and 9-month-old infants expect a word boundary between them (Friederici and Wessels 1993; Jusczyk *et al.* 1993; Jusczyk, Luce, and Charles-Luce 1994; Mattys and Jusczyk 2001). In order to acquire this knowledge, infants may have paid attention to the sound sequences that were permissible at the beginning and end of utterances (boundaries that are clearly marked by a silent pause in the speech stream). Allophonic variations, which correspond to the variations of the acoustic realization of some phonemes depending on their position in words, can also improve the lexical segmentation processes from 10.5 months of age (Hohne and Jusczyk 1994; Jusczyk, Hohne, and Bauman 1999).

Another source of information that infants may have early access to is prosody, the rhythm and intonation of speech. Prosody can help in two ways, either because the specific rhythm of the language provides candidates for potential word boundaries, or, more directly, because it delimits

multi-word prosodic units and therefore gives some word boundaries. The early rhythmic segmentation hypothesis (Nazzi *et al.* 2006) postulates that infants can identify the rhythmic segmentation procedure appropriate to the rhythmic unit of their native language (i.e. stress unit as in English, German, and Dutch, syllable as in French, Spanish, and Korean, or mora as in Japanese). In English, the rhythmic unit is the trochaic unit SW, that is, a Strong syllable followed by a Weak one (85% of the lexical words begin with a strong syllable, Cutler and Carter 1987). This rhythmic regularity allows adults, as well as infants from 7.5 months of age, to postulate the beginning of a word each time they hear a stressed syllable (Curtin, Mintz, and Christiansen 2005; Echols, Crowhurst, and Childers 1997; Houston, Santelmann, and Jusczyk 2004; Jusczyk, Houston, and Newsome 1999; Norris, McQueen, and Cutler 1995). Young infants acquiring stress-based languages thus consider Strong–Weak syllables in a fluent speech as whole units, which can also lead to mis-segmentations due to a delay in the capacity for segmenting iambic WS words (segmentation of words beginning with a weak syllable was shown around 11 months of age). In French, the rhythmic unit is the syllable and it was shown that young infants initially segment bisyllabic words as independent syllables and not as whole units (segmentation of bisyllabic words emerges between 12 and 16 months of age, Nazzi *et al.* 2006).

Regarding phrasal prosody, the hypothesis is that rhythm and intonation variations delimit multi-word prosodic units, such as phonological phrases, that typically contain one or two content words (nouns, adjectives, verbs ...) grouped together with the function words (articles, pronouns, auxiliaries ...) that are associated with them (Nespor and Vogel 1986; Shattuck-Hufnagel and Turk 1996). Even though not all word boundaries are marked in this way, all the word boundaries that coincide with phonological phrase boundaries could thus be easily identified by infants. Indeed, phonological phrases are typically marked by phrase-final lengthening and phrase-initial strengthening, and usually consist of one intonation contour with a potential pitch discontinuity at the boundary (Cho and Keating 2001; Christophe *et al.* 2003; Fougeron and Keating 1997; Keating *et al.* 2003; Wightman *et al.* 1992).

Adults were shown to infer a word boundary whenever they perceived a phonological phrase boundary (Christophe *et al.* 2004; Millotte, Frauenfelder, and Christophe 2007, in French; Salverda, Dahan, and McQueen 2003, in Dutch). For instance, Christophe *et al.* (2004) demonstrated that the potential competitor “chagrin” was not activated in “[son gros chat] [grimpait aux arbres]” (where the two syllables of “chagrin” appear consecutively but are separated by a phonological phrase boundary, as indicated by brackets). On the other hand, participants were slowed down when this temporary lexical ambiguity occurred within a phonological phrase (as in “[son chat grincheux”]). For infants, some experiments demonstrated that young English-learning infants were

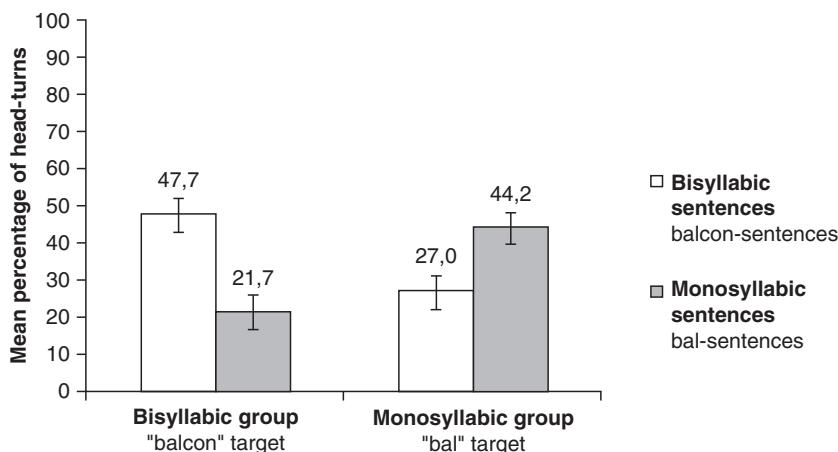


Figure 7.1 Mean percentage of head-turns for 16-month-old French infants in the bisyllabic group and in the monosyllabic group to sentences containing the bisyllabic word and sentences containing its two syllables separated by a phonological phrase boundary (figure adapted from Millotte *et al.* 2010).

able to perceive large prosodic units (i.e. intonational phrases) and to use them in order to segment speech into words from 8 months of age (Seidl and Johnson 2006).

To investigate the use of intermediate prosodic units (phonological phrases), we conducted an experiment with 16-month-old French-learning infants, using a variant of the conditioned-head-turning technique (Millotte *et al.* 2010). In a first session, infants were trained to turn their head for a bisyllabic target word (e.g. “balcon” – bisyllabic group). In a second session (one week later), infants were tested on whole experimental sentences: half of them actually contained the target word (as in “[Le grand *balcon*] [venait d’être détruit]”) whereas the other half contained the two syllables of the target word separated by a phonological phrase boundary (as in “[Le dernier *bal*] [conclura la saison]”). As represented in Figure 7.1, infants trained to turn their head for the bisyllabic word “balcon” responded more often to “balcon-sentences” than to “bal-sentences.” The reverse pattern was observed for infants trained to turn their head for the monosyllabic target “bal” (monosyllabic group), who responded more often to “bal-sentences” than to “balcon-sentences.”

At 16 months of age, French-learning infants are thus able to use the presence of a phonological phrase boundary to infer the presence of a word boundary (see also Gout, Christophe, and Morgan 2004 for similar results with 10- and 13-month-old English learning infants; and Johnson 2008).

In addition to all these cues that are available from an analysis of the speech stream, young infants can also rely on their first known words: thus, very familiar and frequent words, such as “mommy”/“maman,”

allow 6-month-olds to segment words that occur right after them (Bortfeld *et al.* 2005; Mersad and Nazzi 2010). Similarly, infants were shown to rely on their knowledge of the function words of their mother tongue (articles, auxiliaries, pronouns, etc.) to segment neighbouring content words (Christophe *et al.* 1997). Young infants were shown to know something about the function words of their native language before the end of their first year of life (Shi and Gauthier 2005; Shi, Werker, and Cutler 2006) and are able to use the presence of a function word to identify a content word that immediately follows it (Hallé, Durand, and De Boysson-Bardies 2008; Shi and Lepage 2008).

After this first step of lexical acquisition has been solved, young infants are dealt another challenging task: they have to assign a meaning to the word forms they have identified. Once again, we will show that phrasal prosody, together with function words, may be useful.

7.2 Discovering word meanings

Assigning a meaning to a word is not an easy task. One could think that infants only need to associate what they hear with what they see. Even if the lexical segmentation problem is solved, however, it is very difficult to infer what a word precisely refers to. Does “cat” refer to the animal he is looking at, or its ears, its colour, or the fact that it is mewing? This is the well-known problem of the multiplicity of possible meanings of a word (Quine 1960). Moreover, we do not necessarily talk about what is taking place here and now, we also speak about past and future, about things or persons that are not present . . . Discovering word meanings is thus a difficult problem, that could be facilitated if infants were able to analyze the syntactic structures in which words appear (Gillette *et al.* 1999; Gleitman 1990). However, for syntactic knowledge to help lexical acquisition, infants have to be able to acquire it before they learn many words.

The syntactic category of words is the simplest cue that could constrain word meaning. Indeed, nouns typically refer to objects, whereas verbs generally refer to actions and adjectives to properties. Function words, which are known around 8 months of age (Shi and Gauthier 2005; Shi, Werker, and Cutler 2006), could help young infants to categorize words: thus, articles are generally followed by nouns whereas pronouns typically appear before verbs. Some experiments have established that adults were able to use these co-occurrences in order to infer that a nonword such as “bamoule” is a noun when preceded by an article, as in “une bamoule,” whereas it was considered as a verb when preceded by a pronoun, as in “je bamoule” (Millotte *et al.* 2006). What about young infants?

Recent studies have demonstrated that young infants know something about the categories of function words. For instance, 14-month-old

French-learning infants expect a nonword heard in the context of two determiners to be preceded by another determiner, rather than by a pronoun (Shi and Melançon 2010). English-learning 18-month-olds were also found to identify a known noun faster when it was preceded by a determiner, than by an incorrect function word such as an auxiliary (they oriented faster towards the picture depicting the noun; Kedar, Casasola, and Lust 2006; Zangl and Fernald 2007). In addition, French-learning 18-month-olds seem to know that nouns should be preceded by articles (and not by pronouns), whereas verbs should be preceded by pronouns (rather than by articles). Thus, in a variant of the conditioned head-turning procedure similar to the one described in Figure 7.1, a target verb was recognized when it appeared in a correct syntactic context, that is, preceded by a personal pronoun (e.g. “je mange”/I eat) but not when it appeared in an incorrect syntactic context, preceded by a determiner (e.g. *“une mange”/* an eat) (Cauvet *et al.* 2010; Cauvet *et al.*, to appear). A similar result was also found with 2-year-old French children by using more complex syntactic contexts (Bernal *et al.* 2010).

Furthermore, it has been demonstrated that 23-month-old French children were able not only to infer the syntactic category of novel words (with the help of their preceding function words), but also to exploit this knowledge in order to assign a meaning to these novel words. Bernal *et al.* (2007) presented videos of an object that was performing an action (for instance, an apple rotating) while at the same time infants were taught a new word (for instance, “dase”) by listening to spoken sentences. The experimental group (verb group) was taught a new verb with sentences like “Regarde, elle dase !”/Look, it is dasing!, whereas infants from the control group (noun group) were taught a new noun with sentences like “Regarde, une dase !”/Look, a dase! After this familiarization phase, both groups watched two videos depicting the familiar object (e.g. the apple) performing either the familiar action (e.g. rotating) or a novel action (e.g. jumping). Infants were asked to point towards one video, hearing either: “Montre-moi celle qui dase”/Show me the one that’s dasing (for the verb group) or “Montre-moi une dase”/Show me a dase (for the noun group). As represented in Figure 7.2, infants who were taught a new verb pointed more often at the familiar action than at the novel one. This was not a simple familiarity preference since infants who were taught a new noun showed the reverse pattern, with more responses to the novel action than to the familiar one (a typical novelty preference).

Since the only difference between conditions was the syntactic structure in which the new word appeared (e.g. “une dase”/a dase versus “elle dase”/it is dasing), we can conclude that 23-month-old French infants were able to use the syntactic structures in which the new word occurred (and in particular the preceding function word) in order to infer its syntactic category (e.g. noun versus verb) and thus gather some information as to its meaning (here, object versus action).

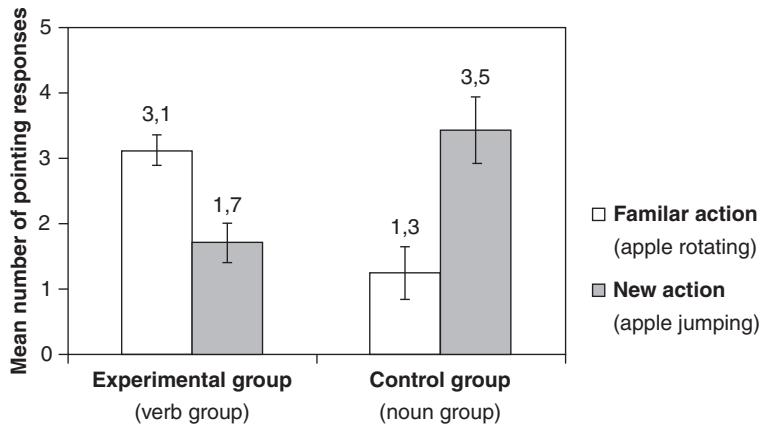


Figure 7.2 Mean percentage of pointing responses from 23-month-old French infants in the verb group and in the noun group towards videos depicting the familiar action and a new action (figure adapted from Bernal *et al.* 2007).

In many sentences heard by infants in everyday life, nouns and verbs are not always directly preceded by function words (as in the preceding experiment). For instance, nouns can be preceded by an adjective (as in “une petite pomme”/*a small apple*) whereas verbs can be preceded by a noun phrase (as in “la fille mange”/*the girl is eating*). Even in these more complex syntactic contexts, we postulate that young infants could perform the same kind of syntactic categorization. To do this, infants would again use function words and phrasal prosody simultaneously. Indeed, in addition to corresponding to word boundaries, phonological phrase boundaries strongly depend on the syntactic structure of the sentence. As a result, they generally coincide with syntactic phrase boundaries (Nespor and Vogel 1986). We know that French adults are able to exploit online these prosodic boundaries to constrain the syntactic analysis of spoken sentences and resolve a temporary syntactic ambiguity (Millotte *et al.* 2008; Millotte, Wales, and Christophe 2007; see also Kjelgaard and Speer 1999, for English). If we consider a sentence such as “le petit garçon a mangé une grosse pomme”/*the little boy has eaten a big apple*, young infants may initially construct the following syntactic representation: “[le xxxx]NP [a xx]VP [une xx]NP” where syntactic boundaries would be given by prosodic boundaries (as indicated by brackets) and where the labelling of the syntactic constituents (e.g. noun phrase, verb phrase ...) would be given by function words (i.e. articles signal the beginning of a noun phrase whereas auxiliaries typically start a verb phrase). This *syntactic skeleton* could be constructed even without any knowledge of the content words that composed the sentence (in this example, nouns, verbs, and adjectives are represented by the number of their constitutive syllables, noted x) and thus could guide the first steps of syntactic acquisition (Christophe *et al.* 2008).

Even if this working hypothesis needs to be directly tested on young infants, it is supported by some results obtained with French adults processing jabberwocky sentences (where content words were replaced by nonwords and where only phrasal prosody and function words were preserved). For instance, they were able to infer that a nonword, such as “pirdale” was a verb in “[un gouminet] [pirdale tigou]” (Millotte *et al.* 2006). To do this, they might have (1) used phonological phrase boundaries to delimit syntactic constituents (“[un gouminet] [pirdale...]”), (2) inferred that the first syntactic constituent was a noun phrase after recognizing the article “un” (“[un gouminet]_{NP} [pirdale...]”), and (3) hypothesized that the following constituent was most probably a verb phrase. As a result, they might have elaborated the following syntactic skeleton “[un gouminet]_{NP} [pirdale tigou]_{VP}” and correctly inferred that the nonword “pirdale” was a verb.

The syntactic skeleton hypothesis appears realistic: during their second year of life, infants do not have access yet to a full-fledged lexicon; on the other hand, it is well established that, at this age, they already know much about the function words (Shi and Gauthier 2005; Shi, Werker, and Cutler 2006) and the prosody of their mother tongue (Millotte *et al.* 2010; Nazzi *et al.* 2000; Soderstrom *et al.* 2003). With these two sources of information, infants may thus be able to construct a first syntactic representation of the sentences they hear, which would help them to continue developing their lexicon, find word meanings, and finally comprehend sentence meanings.

8

Luria's biolinguistic suggestion and the growth of language

Ken Wexler

8.1 Luria's biolinguistic suggestion

The human language faculty is a species-specific ability rooted in the human genome.¹ At the current stage of scientific inquiry, such a statement is unexceptionable. Language grows in the brain; this is also unexceptionable. In these respects, language is like any other species-specific faculty, e.g. the bat's capacity for sonar. What is so intriguing about human language is that it is the “knowledge” level of language, the nature of the computations and representations, that is inherited and grows in the individual's lifespan.

Language is extremely complex, though simple in its underlying principles. One might despair at the possibility of understanding the maturation of language in scientific terms, given this complexity. Certainly the difficulties and general lack of progress in understanding the maturation of other cognitive abilities would not at first glance bode well for the study of the maturation of language. Consider for example the most famous example for the last quarter of a century of immature knowledge of a piece of cognition in children – difficulties in so-called *Theory of Mind* (Wimmer and Perner 1983 and a large subsequent literature), the evidence coming from poor performance in certain tasks involving false belief. Despite the large number of experimental confirmations of the difficulties,² there is to date no reasonable account of how these abilities mature and no reasonably wide-ranging account of what is delayed in children.

Yet in the area of the knowledge level of language, the “computational system (CS)” in Chomsky's terms, we are at a far more advanced stage in understanding the maturation of language. We have theories that tie together large numbers of phenomena in an intriguing way, with unexceptionable and extremely clear and precise data, and progress made continuously. We will discuss several of these pieces of maturation in

this chapter. But first we might ask why the differences between language and cognition are so large in this regard. Why is it possible for studies in the development of language to make such progress, to be clear in their outlines, and account for much data, whereas theories of the development of cognition seem almost as primitive as they were 50 or more years ago?

The answer is clear – it lies in the quote from the distinguished biologist Salvador Luria that the editors used to help to justify the creation of a handbook of Biolinguistics. Luria (1976) argued for the possibility of the creation of a “Biolinguistics” and gave two reasons for “singling out this field.”

First, human language is the special faculty through which all conscious human activity is filtered; a faculty whose development may well have played the driving role in the almost catastrophic evolution from ape-kind to humankind. And second, language alone, among the uniquely human faculties of the human brain, offers to the scientist a systematic theory, the structural linguistics developed by Chomsky and his colleagues, which may be amenable to confrontation with biological data. What I mean is that formal language structure, plus some facts already known about language and brain structure, plus the advancing knowledge of brain organization provided by physiologists, hold some promise that between linguistics and neurobiology a kind of convergence may soon be possible that may provide a wedge into the biological exploration of the human mind.

The second reason is probably enough to show why studies in the maturation of language have made progress, whereas the maturation of other aspects of cognition has had a more difficult time. If Luria could realize in 1976 that language has “a systematic theory,” consider how much more true that claim is now. The scope and precision of linguistic theory has grown remarkably in those 35 years. It is indeed the precise and elegant theory, together with its wide-reaching empirical import, that has made possible the investigations into linguistic maturation by a serious group of experimental scientists. The coming together of the theory of language with developmental psycholinguistics is to my mind the most exciting development in cognitive science in the last quarter century.

What does the general conceptual basis of language maturation look like? Very traditional, like the maturation of other biological properties, at least those properties that have systematic, clean descriptions, what Marr (1977) called results in “Type 1” theories,³ giving linguistic theory as a prototype for that type of theory in cognitive science.⁴ Type 1 theories “decompose” in elegant ways. Marr says that Type 1 results behave “like a result in mathematics or any of the hard natural sciences.” In this respect, Luria and Marr saw the nature of linguistics as being what was hoped for in a hard natural science.

For example, there is an elegant, clean physiological description of the heart. One can imagine a theory of cardiac development, perhaps taking place in the womb, perhaps partially after birth, that understands the maturation of various pieces, and perhaps this theory of maturation extends beyond the heart, to other organs. Since there is a theory of the component

parts, and how these function together, there can be a developmental theory that relates to these parts. Just such a theory is possible in the case of language.

Lenneberg (1967) famously argued for the existence of a biologically driven development of language, on analogy with other instances of biological development. He mustered facts from the small amount that was known in detail about linguistic development at that time, for example the existence of the one-word stage and the two-word stage to draw the developmental analogies between language and other parts of biology. The fact that his arguments were often conceptual and that he had not many detailed results to go on from studies of linguistic development led Wexler (2003) to (admiringly) call his proposals “Lenneberg’s Dream.” A dream that correctly speculated on a deep reality.

At an even more conceptual level, we have Luria’s proposal, partially quoted above. It’s hard to think of an aspect of language that should more readily fit into this proposal than the study of the development of language, development being at the heart of how biology works. In this chapter I’d like to discuss the current state of the study of linguistic maturation, in particular asking how well it exemplifies what I’ll call “Luria’s Biolinguistic Suggestion.” The suggestion has many parts. One central aspect of Luria’s proposal seems to be that the study of language “may be amenable to confrontation with biological data.” Therefore we will ask, to what extent is the study of linguistic maturation so amenable?

Luria also suggested that “between linguistics and neurobiology a kind of convergence may soon be possible that may provide a wedge into the biological exploration of the human mind.” We should therefore look at the language aspects of the human mind, of course, in our case from a developmental perspective. When the ordinary person thinks of the human mind, he or she often thinks of “concepts” under some meaning. So we should also ask, to what extent is there a convergence between linguistics, neurobiology, and development that provides a wedge into the biological study of cognition.

First, we’ll review a small, selective number of results in the study of the maturation of language, to illustrate its present state. We’ll ask the question about “confrontation with biological data,” illustrating with some results. We’ll then try to look at the possibility of a wedge into the biological study of the human mind, looking at what is possible in the study of the biology of cognition.

8.2 Grammatical computation in the optional infinitive stage

From the standpoint of confrontation of a real linguistic structure with one very important aspect of “biological data,” namely genetic data,

probably the most developed instance is early (until about age 3) syntactic computation resulting in phenomena that go under the name of the “Optional Infinitive (OI) Stage.” The related set of phenomena may also be the most widely and intensely studied in generative acquisition research, with many responses from outside the generative field. Wexler (2011) has recently reviewed much of the current state of this field, so here I want to prove only a very brief reminder, illustrating the wide-ranging empirical success of the maturational account, and discussing the “confrontation with biological data” in this field. I will provide an informal account throughout.

Children in many languages go through an “Optional Infinitive” stage (Wexler 1990, 1992, 1994). Among a large array of related phenomena characteristic of this stage, the most famous is the existence of large numbers of productions of non-finite forms, what I have called “infinitival” forms. At the same time, the child will produce finite forms as well. Thus the “Optional” Infinitive stage. The most important aspect of the existence of these optional infinitives (OI’s) is that the child is not confused about the morphology or the syntax; the finite forms produced are truly finite and the non-finite forms produced are truly non-finite. Given the child’s knowledge of syntax, the finite forms show the appropriate syntax for finite verbs, and the non-finite forms show the appropriate syntax for non-finite verbs.

To illustrate, consider Dutch, a Verb-second (V2) language, in which the verb is generated to the right of the object (VP is head final). When the OI-stage child in Dutch produces a finite verb, it is essentially always in 2nd position and when she produces a non-finite verb it is essentially always in final position. (1) shows some examples (from the literature) and the table in (2) shows data for 47 children from Wexler, Schaeffer, and Boll (2004),⁵ ages 1;7 to 3;7.⁶

- (1) a. pappa schoenen wassen
 daddy shoes wash-INF
 “Daddy wash (non-finite) shoes”
 b. ik pak ‘t op
 I pick it up
 “I pick (fin) it up”

In (1a) the verb is an infinitival and it appears in final position. In (1b) the verb is finite and appears in 2nd position. This turns out to be quite a general pattern.

(2)

	V1/V2	Vfinal
Finite	1953	11
Non-finite	20	606

Results like this are typical in this field, although this table represents one of the larger studies. The generalization is not a “tendency” – it is essentially categorical, a law of nature.

There is an extremely large set of phenomena that are characteristic of this early grammatical stage and that must be incorporated into a model of the stage. The phenomena include not only word order phenomena of the kind illustrated above, but also particular properties of syntactic case, including errors, properties of interpretation, of auxiliaries as opposed to main verbs and questions of where in the construction the properties of non-finiteness may occur. For example, it is not true, as has sometimes been claimed, that only “root” phenomena are implicated. One illustration is the omission of *to* in embedded infinitivals at this age (Roberts 1997; Norris 2000). A review of this and a good deal of other evidence is in Wexler (2011).

At the same time, INFL-licensed null-subject languages, e.g. Italian, Spanish, don’t go through the OI stage, the *Null-Subject Optional Infinitive (NSOI) Generalization* (Wexler 1998a).⁷ To illustrate with another large-scale study, Caprin and Guasti (2009) studied 59 Italian children from 22 to 35 months of age. Only about 1%, perhaps less, of the 2,145 main verbs were infinitival. Wexler (2011) compared a young group from the Dutch data of Wexler, Schaeffer, and Bol (2004) to a group of comparable age from Caprin and Guasti’s study and showed that the Dutch children had an OI rate of 64% whereas the same-aged Italian children had an OI rate of <3%. There is nothing about the surface morphology of the two languages that would lead to such a result.

Thus the setting of the null-subject parameter for a language determines whether some typical OI behaviors occur in the language or not. Whatever causes the OI stage must be present in children who speak null-subject languages. We know this because we know that a child’s grammatical capacities will illustrate what’s expected in the language she learns. A child of Italian heritage growing up in a Dutch-speaking environment will develop Dutch, and will go through the OI stage. If such a child grew up in an Italian-speaking environment, she would not go through the OI stage. The model of the stage must explain all this.

The model that best explains all the phenomena is the *Unique Checking Constraint* model (Wexler 1998a). Both agreement (AGR) and tense (TNS) must be checked by DP in many languages, for example, Dutch. This double checking is what is forbidden by the UCC. The child finds this double-checking ungrammatical.

- (3) *Unique Checking Constraint* (on OI-age children, i.e. until about 3 years):
 - A DP can check only one D-feature.

A Dutch-speaking child whose grammar must follow the UCC can’t grammatically derive a simple finite sentence. The child adopts a strategy to allow sentences to be generated. Otherwise all sentences would be

ungrammatical, and the child couldn't speak grammatically (according to her own grammar) at all. Although we might assume that the child knows that AGR and TNS must exist in a sentence, she omits one of them from the grammatical representation so that the UCC isn't violated, resulting in the possible generation of a grammatical sentence. This AGR or TNS omission results in the ATOM (AGR/TNS Omission Model – see Schütze and Wexler 1996) holding for the young child. Many predictions about, for example, subject case follow.

The UCC immediately explains NS/OI, for example, why Italian doesn't exhibit the OI stage on its verbs, rendering the option of non-finite rather than finite verbs possible in simple clauses for the Italian-speaking child. Since the existence of INFL-licensed null-subjects means that agreement (which we can think of as a functional category, although this might not be necessary) is interpretable, the conclusion is that Italian does not require that Agreement be checked by the subject. That is, the EPP does not hold for AGR in Italian and other INFL-licensed null-subject languages. Such a conclusion immediately fits the conclusions reached by Romance syndications (e.g. Barbosa 1995; Alexiadou and Anagnostopoulou 1998) who argue that in the Romance null-subject languages, the subject does not raise high except in special semantic conditions; syntax itself (the EPP) does not require high raising.

Since AGR isn't checked by the subject DP, only TNS must have its subject checked. The necessity of only one checking implies that the UCC isn't violated, and no functional category (AGR, TNS) should be omitted by the child. There is no motivation for this strategy. The Italian-speaking child, therefore, produces finite sentences, as does an adult. There is no OI stage as determined by infinitival main verbs in the Italian child. This is NS/OI.

But the basic biology of UCC must exist in the Italian child. If she were developing Dutch, she would go through the OI stage. We therefore should expect to find *other* phenomena that reflect the UCC in Italian children. And indeed we do. Auxiliary omission is one of them.⁸ The most striking UCC phenomenon in Italian, though, is probably object clitic omission, which has been known for a long time to occur a great deal in Italian, but which does not occur in Spanish (Wexler, Gavarró, and Torrens 2004; Gavarró, Torrens, and Wexler 2010), or Greek (Tsakali and Wexler 2004). As these papers together with the model in Wexler (1998a) explain, the UCC derives both clitic omission and the difference between languages that have different parameter settings (we have not discussed here what the parameter is that relates to clitics; see the papers).

The UCC is a computational constraint; a constraint on how the child's computational system of grammar works, a grammatical constraint, computational in the sense of Chomsky's use of the notion "CS," "computational system [of grammar]" to indicate the underlying grammatical capacity of a human (at the computational level of analysis). It interacts

with parameter-settings derived by a child from experience to produce strikingly different linguistic behaviors in different languages. The UCC, as a computational constraint, is also a biological constraint, a biological entity, described at the computational level of analysis.

The UCC disappears slowly as the child ages, in the same manner as a first set of teeth are lost by a child, over time, some teeth going before others, according to a genetic predisposition whose time course varies somewhat over children, but, except for pathological cases, holds. The genetic system of the child has a good deal of influence on causing the disappearance of first teeth and the UCC, but the causes may possibly apply through intermediary factors. For example, possibly it isn't genes directly acting on teeth telling them to be lost; rather it may be that genes only provide a certain amount of strength for teeth, and they'll be lost as a matter of physical necessity once their properties have been set by genes. Or it may be that the interaction of the first set of teeth with other physical structures in the child has an influence on the loss of teeth. Similarly for the UCC. The causes of its loss may be directly genetics (genes determine a time course) or there may be intermediary explanatory models that cause the loss. Whatever it is, there are ultimately genes at play, just as there are for teeth. We are in the realm of biology.

8.3 The UCC: The biology of the matter

Can we use the results generated in this field of developmental psycholinguistics to help cash out Luria's Biolinguistic Suggestion? What about the "confrontation with biological data"? Naturally, it depends on what we mean by "biological data." The data we've briefly illustrated are developmental data, strongly connected to linguistic theory, but nevertheless development data. Since development is at the heart of biology, surely these data should count as biological data. Biology has huge reams of similar data over many fields.

But one might imagine that Luria had a stronger hope in mind. Perhaps he hoped that the study of language might interact with the *type* of data that biologists have gathered already or in different fields, data that they recognize as the type that they study. To meet this stronger interpretation of Luria's Suggestion, the biological data in question should relate directly to familiar biological concepts, concepts worked out through years of research. Development is perhaps too generic – what view of biology wouldn't allow for development, a very familiar everyday concept? We need instead, a more highly refined concept from biology to satisfy Luria's suggestion, a concept that biology struggled to attain and find evidence for. Luria suggests that perhaps "physiology" could be such a concept, or rather, the details of physiology. We'll return to this suggestion, but I would like to propose an even more natural suggestion, that the study of genetics might provide an

appropriate model for the integration of the two fields, creating a Biolinguistics. This is natural because the conceptual foundations of language actually speak of the inheritance of language ability as a species-specific ability. The foundations of linguistics are genetic.

The model proposed in many papers since Wexler (1996, 2003) is that the OI stage is a genetically based developmental stage. That is, the genetic system helps to determine not only the nature of language but also its developmental course. There is a great deal of evidence deduced for this hypothesis in the literature. It includes the following:

- (4) (1) Since such a huge amount of data presented to the child is finite, i.e. tensed, why does the child go out of the way to make her sentences mostly non-finite at first, especially when in many languages this makes a more complicated surface morphology?
- (2) Moreover, the child shows brilliant plasticity in learning the correct parameters of clause structure (VO/OV order, V to TNS, V2, null-subject parameter, wh *in situ/moved* parameter) that it is hard to see how a learning problem could cause the child to think that sentences should be non-finite. In the light of brilliant mapping of experience, why should the child fail to learn what should be so easy? (See Wexler 2011 for a detailed critique of a recent attempt to account for the OI stage as a product of "learning." Such a proposal mis-predicts central phenomena of the OI stage.)
- (3) There are known variables that influence "learning" e.g. richness of input such as "Mother's Degree of Education." Vocabulary size, for example, is heavily influenced by such variables. Nevertheless, these variables do *not* influence the development of finiteness (Rice, Wexler, and Hershberger 1998).
- (4) Rates of finiteness development are continuous; it's not a one-step process of "learning" a particular piece of knowledge. This type of continuity is characteristic of maturational processes, e.g. a set of teeth don't come in all at once, but grow.
- (5) The child in the OI stage produces many structures that she hasn't heard, that are not part of adult English, but that follow from her genetically determined grammar.
- (6) Specific Language Impairment in English and many other languages is characterized by an Extended Optional Infinitive (EOI) stage. It looks as if SLI is a delayed maturation of the system (Rice, Wexler and Cleave 1995; Wexler 1996, 2003 among many others. See Borer and Wexler 1987 for the first hypothesis of maturation in modern grammatical development studies.).
- (7) The OI (UCC) stage could not be a "learning a language" phenomenon, since there is no conceptual basis for that. Moreover, and strikingly, L2 learners past the youngest ages do *not* go through an OI stage.

8.4 Is the OI stage simply an effect of learning a language? Evidence from second language acquisition

I can't review the rich evidence summarized in (4). Let's just consider a bit of the evidence for point (7) of (4) – the non-existence of the OI stage in 2nd language acquisition, since I believe that these phenomena may not be as well known as some of the others. Could it be that the properties of the OI (UCC) stage flow from the fact that the language has to be learned? Is this a "learning" phenomenon? It's very hard to see how that could be, given that a central property (the existence of large numbers of non-finite root sentences, the omission of INFL categories) are contradicted by the input data. One cannot think of an even barely adequate model that would give the existence of the OI stage as a "learning" phenomenon.⁹ Nevertheless, there is a clear prediction from the biological/growth/maturation model that past the youngest (OI) age levels, there should be no OI stage when a 2nd language is learned. For example, adults shouldn't go through the OI stage.

This is such an obvious prediction, yet just the kind of prediction that is rarely tested from either point of view in the study of language acquisition or even of development in general, in any precise sense. I recall Gary Marcus asking just that question at a plenary talk I gave at the Boston University Child Language Development conference several years ago, and the impression that I had that he was startled by the answer I gave – that the experiments had been done and came out on the side of the biology/growth model – L2 learning does not show the OI stage. And the opposite point (the expectation that the phenomena of L1 and L2 acquisition should be identical) is so ingrained in scholars (I believe because of the lack of understanding of biology and development and even of the growth basis of the conceptual foundations of linguistics theory) that the unmarked assumption always seems to be the wrong one – that L1 and L2 acquisition are identical. Rather, we should expect L1 and L2 acquisition to differ – the species undergoing the learning is at a different developmental stage in the two processes.

Thus, White (1996) argued that the existence of optional infinitives in L2 showed in fact that – contra to (my) claims – the existence of the OI stage could not be a growth/maturation phenomenon. This logic was right, the analysis was wrong. The standard assumption of so many scholars, that there is nothing interesting about development, that there is no biological fact of the matter, made it difficult to see that the OI stage demanded much more in the way of evidence than a few morphologically wrong verbs. So much for Luria's Suggestion. The far too ubiquitous assumption of the lack of biology underpinning language (totally at odds with the conceptual foundations as originally argued by Chomsky) not only misses perhaps the most important point of linguistic theory, and the underpinning of Biolinguistics, but gets the analysis wrong.

To see why, we have to look at some data. It's true that L2 learners use non-finite main verbs. But they often use these verbs in the position that a finite verb should occupy. This is exactly what a native L1 learner at a young (OI) age does *not* do. We have seen, for example, the strikingly clear relation in Dutch OI stage grammars between finiteness and 2nd position, on the one hand and non-finiteness and final position on the other (2). Such behavior, such data tables, are *not* characteristic of L2 acquisition. Prevost and White (1999) for example, found that adults learning German (another V2 language, similar to Dutch in essential respects) often put infinitival verbs in 2nd position. Let's look at some data (5) from Prevost and White. The two learners of French were native speakers of Moroccan Arabic and the two learners of German were native speakers of Spanish (Ana) and Portuguese (Zita).

In order to make very clear comparisons between L1 and L2, in (5, 6) I've taken tables from several papers and put them in equivalent tabular form. We'll use the L1 Dutch data from (2), repeated here in (5a) as a comparison for the German L2, as the relevant facts of development in these 2 V2/SOV languages are the same and we'll use the data from Pierce (1992), as summarized in Wexler (1998b), as the L1 comparison (6a) for the learning of L2 French. (5b,c) and (6b,c) are adapted from Prevost and White's Table 3.

(5)

	Finite context	Non-finite context	%Finite context
Finite verb	1953	11	99.4
Non-finite verb	20	606	3.0
%Non-finite verb	1.1	98.2	

Dutch children (OI stage)

	Finite context	Non-finite context	%Finite context
Finite verb	389	7	98.2
Non-finite verb	45	76	37.2
%Non-finite verb	10.4	91.6	

Ana (L2 German)

c.

	Finite context	Non-finite context	%Finite context
Finite verb	434	6	98.6
Non-finite verb	85	98	46.4
%Non-finite verb	16.4	94.2	

Zita (L2 German)

(6)

a.

	Finite context	Non-finite context	%Finite context
Finite verb	185	11	94.4
Non-finite verb	2	77	2.5
%Non-Finite verb	1.1	87.5	

French Children (OI Stage)

b.

	Finite context	Non-finite context	%Finite context
Finite verb	767	23	97.0
Non-finite verb	243	278	46.6
%Non-finite verb	24.0	92.4	

Abdelmalek (L2 French)

c.

	Finite context	Non-finite context	%Finite context
Finite verb	755	2	99.7
Non-finite verb	224	156	58.9
%Non-Finite verb	22.9	98.7	

Zahra (L2 French)

For Dutch/German (5), we only have V1/V2 as “finite” contexts, so there might be small differences from Prevost and White’s L2 data set, which might have a few other productions (e.g. embedded sentences), plus the infinitives will allow expected infinitives, as we discussed.

The largest difference between the L2 and L1 tables is how often the L2 learners use non-finite sentences in finite contexts. Ana (5b) in some ways is the most advanced L2 learner, as she only uses infinitives in 10.4% of her finite contexts. Still this is far larger than the 1.1% of infinitival usage in finite contexts in the large group of OI-age Dutch children (5a). Ana does better in non-finite contexts, 91.6% of those contexts see a non-finite verb used. Nevertheless, even this is worse than L1 monolingual children, who have a 98.2% correct usage.

Zita, the second L2 learner of German (5c), uses 16.4% infinitivals in finite contexts, even larger than Ana’s and much larger than the OI children’s 1.1%. Her use of infinitivals in non-finite contexts is 94.2%, so she is quite a bit better there than on finite contexts.

So the two L2 German learners very much overuse infinitives in finite positions, that is their big error, and that is something that the OI children almost never do.

Turning to French, the Pierce data in (6a) is based upon finite contexts being ones in which the verb precedes negation *pas/not*. Finite verbs in French raise to Tense, and non-finites don’t raise, so only finite verbs must precede *pas* and non-finite verbs must follow *pas*. The L2 data might have a few productions in which finite/non-finite contexts are a bit different, e.g. for the infinitivals, when they occur in some grammatical positions. These were not included in Pierce’s analysis. Nevertheless, the crucial points are not affected by this difference.

The French OI children only produce 1.1% infinitivals in finite contexts (i.e. preceding *pas*). This vanishingly small number accidentally turns out to be equivalent to the Dutch children’s use of infinitives in finite contexts. This number is perhaps best thought of as the “error rate.”

Turning to L2 French, Abdelmalek (6b) uses 24.0% of infinitives in finite positions. This is huge. Not only is it huge compared to 1.1% for the OI children, but it is unheard of in the very large OI literature, where infinitives are overused so infrequently. It’s a signature of L2. Notice this large number isn’t an accidental statistical result of small numbers. Abdelmalek has about 1,000 utterances that are measured in the table.

Zahra, the second L2 French learner, confirms the result. She has 22.9% use of infinitives in finite contexts, again out of almost 1,000 utterances. Both French and German learners are using large numbers of infinitives in finite contexts.

Abdelmalek and Zahra have 92.4% and 98.7% use of infinitivals in infinitival contexts, respectively, so like the German L2 learners, they are much better at using infinitives in non-finite contexts than at using finite verbs in finite contexts.

We should be careful, however, before strongly drawing the conclusion from this data that L2 learners will mostly correctly use infinitives after *pas* in French and in final clause position in root sentences in German/Dutch. This is because the L2 tables we've looked at include infinitives in other contexts, acceptable infinitives.

The L2 use of infinitives is *not* the same as the L1 use of infinitives, as all authors on the topic after the White paper I mentioned above have agreed. The idea has been presented in Haznedar and Schwartz (1997), Prevost and White (1999) (the *Missing Surface Inflection Hypothesis*) and other places. Here's a simple model of the L2 behavior, to a first approximation. L2 learners have UG, the computational system of language. This is the most natural assumption. After all, they use this system to produce and understand sentences all the time. Many of these sentences are new to them, used in unusual situations, etc. They need to have an active set of principles to use their language.

What, then, is different about L2 learners? The most obvious, simple assumption is that they have reduced plasticity, the ability for their brain to change in systematic ways given experience. Experimental studies (e.g. Johnson and Newport 1989) make it clear if a 2nd language learner starts to learn the language after a certain age (about puberty, perhaps a bit longer) then she is very likely to not get the grammar right. It's not the computational system that she's missing, according to this model. Rather it's difficult to learn the correct language-particular facts about the language.

There are large numbers of examples. One simple one is plurality. If the learner's native language doesn't mark plurality on nouns, then it is very difficult for an adult L2 learner to get this right in an L2 that *does* mark plurality. It's not that the L2 learner can't count. Rather, the plasticity of the linguistic ability with respect to what must come from experience (does it mark plurality or not) is lacking or greatly reduced.

So the L2 learner in the finiteness case has difficulty learning morphology of tense and agreement on a verb, the spelling out of these values. Perhaps effort goes into the learning of the "citation" form, which is usually the infinitival form, the form one sees in standard dictionaries. If the learner has learned this citation form, but has not learned, or has forgotten, the finite form she is trying to recall, it is natural to use the infinitive as a "default." The idea is that it is what the learner recalls, and doesn't put in extra features that are perhaps wrong, etc. Intuitively, imagine learning the verbs of a language on a flash card, that is, one learns, for example *parler* = speak. If you don't recall the agreement paradigm, which also has to be learned, you might say *Jean parler/John speak [non-fin]* instead of *Jean parle/John speaks [fin]*. It'll get the communication job done in many contexts, although it will grate on a native speaker.

Assuming that L2 learners know the principles of grammar, they'll know that a finite verb belongs in certain positions, e.g. as the main verb of a simple sentence. But they'll use an infinitive because of lack of

knowledge or recall ability of the finite inflection, as a kind of strategy. It would have been natural 25 years ago to think that an L1 child had the same strategy. But she doesn't. We know this because, as we have seen, adult L2 learners use the infinitive in finite contexts, quite often, whereas the young child doesn't.

In infinitival contexts L2 learners are more accurate than in finite contexts. According to the simple model I have outlined, this is simply due to their knowing the morphology, the spell-out of the infinitive better than that of the finite form. In the simplest analysis, it's because they've learned the infinitive in different learning contexts, perhaps as a citation form. The effect isn't monolithic however. One could expect certain L2 learners to have learned another form of a verb, and overuse that.

There are several other papers that have investigated the issue (Ionin and Prevost 1997; Wexler 2003; Tran 2005) and they all agree on the general outlines of these results. I know of no results that would lead to an opposite conclusion. It should be acknowledged that the number of L2 subjects analyzed in this way is much smaller than the larger studies of L1, and much is yet to be learned about the effect of L1 on the learning process, etc. Nevertheless, even with the small number of studies and subjects, the fact that the direction of the results is clear is quite encouraging.

The results are clear. The amazing difference between L1 and L2 learners is fairly stunning evidence for the maturational explanation of OI stage. This is the type of study that rarely gets done when one proposes, for example, that an error in language learning is due to "learning" and not to biology. If one takes the scientific approach seriously, the result has to be acknowledged; the child error is distinctly different in kind than the adult "learning" error, and to be explained in a different way. We have an effect of biology, and an entry into Luria's Biolinguistic Suggestion.

Why should it take so long and present such difficulty for an adult L2 learner to pick up the inflectional system, the spell-out of the features? It's a lack of plasticity. Can we say anything in detail? Earlier I mentioned the attempt of Legate and Yang (2007) to try to explain the existence of the OI stage in children via an application to a "Tense Parameter" of Yang's (2002) model of parameter-setting. As Wexler (2011) shows, the model mispredicts the central facts of the OI stage. But one might consider whether this model could be fitted appropriately to adult L2 learning.

Why might this work? The model is what I call a "stamping in" model, akin to habit-formation learning models of the mid twentieth century. If the learner hears a sentence she very slowly, very laboriously increases and decreases the weights of parameters. There is no kind of use of data to infer the correct grammar. And it is extremely slow. It predicts massive errors during learning.

Strikingly, as Wexler (2011) shows, and contra the claims of Legate and Yang, the learning model predicts that the learner will produce errors in word order, using, for example, infinitives in finite positions. This is exactly

what L2 learners do. So perhaps the model can be applied to L2 learning, where it is anyway more natural, as it is a model that appeals to what appears to me to be defective plasticity, really, just very slow, laborious learning, not what's characteristic of children in their language forming years.

Looking into this in a somewhat more detailed fashion, the Legate and Yang predictions depend on differential rates across languages of “ambiguity” in whether the language has Tense or not, as a question of a parameter. As Wexler (2011) argues, this doesn't make much sense. But perhaps the ambiguity idea can be reframed as a question of how transparent it is in the input data what the agreement forms are. So that the slow, laborious learning process is made easier when the input is clearer and more abundant. Since a language like Spanish has a good deal of clear input evidence about a rich set of inflectional forms, these spell-outs of the inflectional features will have abundant, clear evidence, and the L2 learner of Spanish would pick them up relatively quickly. In English on the other hand, the evidence for 3rd person singular *s* might appear relatively infrequently and the existence of no audible element in the spell-out of other members of the paradigm might interfere with slow learning also, so that the learning of *s* is delayed. The details of the model would matter. But we'd predict more difficulty for English inflections as an L2 than for Spanish inflection as an L2.

One would have to test the use of infinitives in Spanish L2 against English L2, for example, to test this prediction. But it's a possibility. Perhaps the L2 parameter-setting process has this stamping-in characteristic of the Yang model, that doesn't exist for L1 learning. If in fact the rich agreement languages (Spanish) show faster L2 learning (fewer uses of the infinitive in finite contexts) than the poor agreement languages (English, Swedish) then one further prediction is clear. Icelandic as an L2 (learned by an adult) should show few infinitives in finite positions, just like Spanish. That is, not much of an overuse of infinitives. Whereas for young children we know that Icelandic doesn't behave like Spanish, because Icelandic is not null-subject.

There is other research concerning L2 and OI's, all of which to my knowledge agrees that adult and older child L2 learning does not show the properties of the OI stage, but rather uses infinitives as a kind of default due to insufficient learning or recall of the inflectional forms. Tense is not omitted from the representations at these ages, unlike in early L1 learning. Tran (2005) showed that English-speaking students learning German in Hawaii had a strong use of infinitivals in 2nd position. Haznedar and Schwartz (1997) showed that a very young Turkish child who moved to London used non-finites as a default in English L2; the child did not use null-subjects the way an OI-stage child does and did not use non-nominative subject case for non-finites, which is another sign of the OI stage in English. Ionin and Wexler (2002) showed that 5- to 10-year-old Russian-speaking children who learned English as an L2 in the United

States used infinitival instead of finite verbs in many instances but were not in the OI stage. They made types of errors that are not typical of L1 learners in the OI stage.

Much remains to be investigated about L2 and what learning models are appropriate for parameter-setting there. But we can safely conclude that there are biological growth processes at work in L1 that don't exist in L2, and that these are responsible for the OI stage. Luria's Biolinguistic Suggestion works out in this context.

8.5 Toward a biolinguistics in the growth of language: Behavioral Genetics and Specific Language Impairment

There is a large complex of properties that flows from the analysis of the finiteness system, or more generally the UCC-delayed system, as a piece of biological development. These properties don't follow from the small number of alternatives that might be offered, e.g. the OI stage is a result of "learning" or it is due to some kind of memory error (why, then, doesn't it occur at all in many languages, e.g. Italian? why does NS/OI hold?). In short, there is a great deal of evidence for Chomsky's (2004, 2005) observation that one of the "three factors that enter into the growth of language in the individual" is:¹⁰

Genetic endowment, apparently near uniform for the species, which interprets part of the environment as linguistic experience, a non-trivial task that the infant carries out reflexively, and determines the general course of the development of the language faculty. Among the genetic elements, some may impose computational limitations but that disappear in a regular way through genetically-timed maturation. Ken Wexler and his associates have provided compelling evidence of their existence in the growth of language, thus providing empirical evidence for what Wexler calls "Lenneberg's dream."

This complex of integration between linguistic theory and psycholinguistic growth is already a significant step toward Luria's suggestion. It could have been accomplished only with the kinds of formal language study that Luria mentions together with experimental investigation of the detailed properties of language growth in the individual and its cause. So we are already on the road to showing how the suggestion might be realized.

But there is a further goal mentioned in Luria's suggestion, the goal of integrating biological data of the sort that biologists are familiar with and linguistic theory and growth studies. Is anything known about that possibility in the domain that we are looking at? Amazingly enough, the answer is yes. Even compared to as recently as the possibilities for such integration mentioned in Wexler (2003) there is real development. Let me review what is known.

As we've previously argued, the most natural place to look for a Biolinguistic connection between language and biology is in the domain of inheritance, of the genetic system construed broadly. How is the biological capacity for language passed down from generation to generation? What does get passed down? What does this passing down accomplish? These are the kinds of questions (not about language, but in general) that revolutionized biology in the mid twentieth century. We have the outlines of an answer to the question of inheritance.

The first task to be accomplished in attempting to study the genetic underpinnings for any system is to prove that it's inherited. Chomsky's arguments for the inheritance of the capacity for language are famous. In addition, the study of language development adds quite a few reasons to believe in the genetic nature of the capacity. But what about "biological" evidence that is used in most genetic studies.

It may not be apparent to the linguistic reader, but the kinds of arguments we have already given are the kinds of arguments that are used to study the question of genetic capacity in biology in general (other than when the general expectation in biology is reason enough to have a bias to genetic inheritance). It is a question of simple logic and evidence. So the fact that environmental variables that can have an effect don't have one in the case of finiteness is clearly an illustration of the kinds of evidence that biologists would use.

But how about evidence that uses "biological" variables relating to genetics? Evidence that uses what we know about biology more specifically? The first place one would look in biology is to test whether individuals that share more of their genetic systems develop/grow in a closer way with regard to the property under consideration. One studies the "heritability" of a trait in biology.

Can we do this in language? Yes. More particularly, can we study the development of the phenomena that follow from the UCC, the phenomena that we have evidence are under genetic timing? For example, and centrally, can we study the development of finiteness in this way? Do two individuals who share more genes develop finiteness in more similar ways than two individuals who share fewer genes?

This area of research is called "Behavioral Genetics." The first paper I know that asked this question was Ganger, Wexler, and Soderstrom (1997); see also Ganger (1998). Their study was carried out via the standard method of analysis of twins, which I present here in quite simplified form, not taking account of possibilities that can complicate the analysis, e.g. environment in the womb. Identical (monozygotic, MZ) twins share 100% of their genes and fraternal (dizygotic, DZ) twins share about 50% of their genes. We wish to determine how much of a trait X is due to the genome and how much is due to the environment. The assumption is that for the trait X under investigation, if the twins are brought up together, with respect to the relevant experience for X, the similarity of

environment between two identical twins is quite close to the similarity of environment of fraternal twins. Thus to the extent that identical twins resemble each other more on trait X than do fraternal twins, we must attribute this difference to the genome, that is, to inheritance of trait X.

An example is height. On the average, identical twins are more similar to each other in height than are fraternal twins. On the assumption that the identical twins, growing up in the same household, same parents or caregivers, had about the same environment, and similarly for fraternal twins, the greater similarity in height for identical twins is attributed to the genome. All this is the case in explicit terms of amounts of heritability, but we won't provide that kind of formal discussion here.

Clearly there are instances in which the crucial assumption of similarity of environments fails. Suppose the trait X is something like how one chooses to dress. For example, one might suppose that the parents of identical twins tend to dress them more alike than the parents of fraternal twins. If early style of clothes influences adult choice of clothes, it might turn out that identical twins grow up to dress more alike than do fraternal twins. We couldn't attribute this difference in dressing style to genes because there was an environmental difference that could have caused the difference in the trait to emerge.

Finiteness is not such a trait, however. Children receive thousands of utterances, and there is no reason to believe that adults choose to speak more similarly to their identical twins than they do to fraternal twins. And we have already seen that the standard variables for learning (e.g. mother's degree of education) do not influence the development of the rate of finiteness. Parents also don't choose their sentences on the basis of language lessons (Newport, Gleitman, and Gleitman 1977). If there were anything in the realm of cognitive science that is amenable to standard behavioral genetics, I would argue that the rate of finiteness production is.

So Ganger *et al.* took rates of finiteness in obligatory contexts in English as the measure in their twin study. They showed that MZ twins were more similar in attaining a particular criterion than were DZ twins; in particular, on average the MZ twins were 3 weeks different in reaching the criterion, whereas the DZ twins were 13 weeks different. The greater similarity of the MZ twins is attributed to the developmental rates being affected by genetic inheritance, which is more similar for MZ twins than for DZ twins. For further evidence and discussion, with more twin pairs, see Ganger (1998) and for discussion see Wexler (2003).

Since the publication of Wexler (2003), which made the case for the hypothesis that finiteness (really the UCC) was a genetically determined piece of Biolinguistic growth, bringing together much data integrating biological type thinking and linguistic data that bore on the question, the biological support for the hypothesis has grown even stronger and more detailed. One example has to do with behavioral genetics, what we've just briefly looked at. Ganger *et al.* and Ganger's dissertation had

the right logic and evidence, but a limited number of twins under investigation. There is now a much larger study, more typical of the size range of behavioral genetic studies in other fields, that can be used for evidence concerning what is inherited, over a larger array of capacities. This is the kind of evidence that isn't typically brought into discussion of studies of language acquisition when such questions are debated. But that omission makes no sense. This type of evidence speaks directly to the question of genetically based maturation versus experience as being the determinants of aspects of language acquisition. And of course, Luria's Biolinguistic Suggestion tells us to move in that direction. Let's see what happens when such studies are carried out.

First, we'll have to take a very brief (unfortunately) detour into the study of language impairment. *Specific Language Impairment (SLI)* is a deficit in linguistic abilities that is not accompanied by an obvious cognitive or behavioral deficit. It is a huge topic, the literature of which we can't even begin to summarize here. I'll just mention a few central and relevant points.

There is good reason to believe that children with SLI aren't just the tail of a normal distribution of development. There is too much of a distinguishing gap between such children and typically developing children. Furthermore, on particular grammatical properties that the deficit targets, SLI children perform significantly worse than (younger) control children who are matched to the SLI children on measures of general language development (Rice, Wexler, and Cleave 1995 and many papers since). In addition, there is now good evidence that SLI continues into the late teen years, and most likely, never goes away (Rice, Hoffman, and Wexler 2009). Children just learn to cover it up, to behave appropriately in common grammatical situations. They are intelligent, receive therapy, and have every motivation to do so. They are probably using their general cognitive abilities, but are still missing something grammatical in their linguistic systems. See Wexler (2003) for a brief review.

As Rice, Wexler, and Cleave first showed, children with SLI are greatly delayed in their development of finiteness, significantly worse than TD children matched to them on general language development. They argued for the existence of OI's as being a "clinical marker" for SLI in English. Rice and Wexler (2001) is a standardized test based on rates of finiteness in SLI that selects English-speaking children as having SLI or not. It is highly sensitive and specific.

At the same time, children with SLI do not show serious problems with subject-verb agreement, nor make certain other kinds of grammatical errors concerning inflection that TD children don't make (Rice, Wexler, and Cleave 1995). Thus these children are in the OI stage. We call it the *Extended Optional Infinitive (EOI)* stage just to remind us that it is the OI stage continuing to a later age. Although children with SLI increase their rates of finiteness in obligatory contexts as they get older, reaching 90% or so at age 8 (as a group), this remains a bit below TD children. More significantly,

Rice, Hoffman, and Wexler (2009) showed that in slightly more complicated contexts, where direct therapy probably didn't help them cover up the error, even many (late teens) children with SLI behave poorly.

SLI, of course, provides a greatly expanded range of data appropriate for the Biolinguistic view. SLI is inherited (Rice, Haney, and Wexler 1998; Bishop 2002 for a review, and see ensuing discussion here). Thus we have a genetically based impairment that seems to target grammatical properties, in particular finiteness. Of course, since OI is a special case of the Unique Checking Constraint, the natural hypothesis is that SLI is governed by the *Extended Unique Checking Constraint* (EUCC). The first prediction of EUCC is that non-finite utterances will be characteristic of only certain languages, i.e. NS/OI will hold of SLI children. In particular, there should not be large rates of non-finiteness in the productions of children with SLI speaking null-subject languages, e.g. Italian. This prediction turns out to be true (Botarri, Cipriani, and Chilosì 1996; see Wexler 2003 for discussion).

On the EUCC hypothesis, we would expect slowness in development of other grammatical properties in children with SLI. An example with a good deal of empirical study is the case of object clitic pronouns, which are often omitted by children in the OI stage in certain languages (e.g. French, Italian, Catalan) that have such clitics. The UCC is the best explanation for this omission (Wexler 1998), explaining not only the omission of clitics, but also why omission doesn't occur in certain languages (Spanish, Greek), (Wexler, Gavarró, and Torrens 2004; Tsakali and Wexler 1998; Gavarró, Torrens, and Wexler 2010).

The next prediction from EUCC is clear: SLI children developing languages in which TD children don't omit clitics also shouldn't omit clitics. This turns out to be true. Manika, Varlokosta, and Wexler (2011) showed that the rate of clitic omission in Greek-speaking children with SLI was very small and that there was a very large difference between the rates in the literature from Italian-speaking children with SLI.

Their [Arosio *et al.* 2010] method was an elicitation method very much like our method. The SLI children ranged from 6;2–8; 4 years with mean age 6;10. The SLI children in our study had a mean age of 6;2. Thus, we have a very similar study in Italian with somewhat older children. Arosio *et al.* (2010) found that their SLI group omitted clitics 40% of the time, compared to the 5% that we found for Greek SLI children in our study. This is really a huge difference.

This tight constellation of a rich set of developmental facts is exactly what we'd expect of a biolinguistic system, and how it might go awry. Since SLI is inherited, the biological fact of its inheritance adds to the developmental facts to support the conclusion. Luria's Biolinguistic Suggestion applies.

Now we're ready to turn back to the behavioral genetic data. As we have shown, Ganger *et al.*'s study of TD twins showed that there was some genetic inheritance of finiteness rates but was a small study. There is

now a much larger, very detailed study (Bishop, Adams, and Norbury 2006) that also compares the inheritance of finiteness to the inheritance of other capacities and considers the genetic relations of the inheritance. This paper studies language impaired children, so that we can look for differences in development of finiteness rates at later ages than in Ganger *et al.* The understanding of SLI as EOI and, more generally, EUCC that has been documented in so many previous studies is crucial for our ability to use the data from the genetic basis for SLI as indicating support for that particular genetic basis of development in TD children. In other words, the simplest hypothesis we have is:

- (7) The Genetic Uniformity Hypothesis (GUH): The genetic cause of SLI is an aberration of the genetic system that causes the EOU (more generally the EUCC).

Genetic Uniformity does not have to hold; if true it is a discovery (though it is the simplest hypothesis). We have excellent reason to believe in Genetic Uniformity. Namely, many striking effects (including OI's, omitted clitics and the particular correlations with their existence in particular languages) behave exactly the same way in TD and SLI. This did not have to happen. It is easy to create possible scenarios that would lead us to not support Genetic Uniformity. E.g. suppose SLI children had EOI, but didn't omit clitics. Then we'd have either to think of the OI stage in TD children and in SLI children as having different characterizations (the UCC in the case of TD children, something else in the case of SLI) or we would have to give up the UCC characterization of the OI stage, leading to a lack of understanding of and synthesis with the OI stage, and difficulty in explaining why clitics weren't omitted in particular languages. Or, suppose SLI children showed a great number of subject/verb agreement errors, and, perhaps, omitted *ing* from participles. This is so at odds with the OI stage, that we couldn't support Genetic Uniformity. But the many tests that have been conducted so far support Genetic Uniformity. Thus we'll assume this and be able to conclude (as a hypothesis, with empirical support) that whatever the genetic basis of the OI (UCC) stage in TD children, SLI has the same genetic basis, but with aberration. This is a well-known genetic effect of heterochronology. Some genetic development takes place out of time, or not at all. In this case, we think of the genetic system causing a change in physiology (in the brain) that allows the UCC to disappear over time. In children with SLI, the genetic system has lost this capacity, thus it cannot cause the brain to undergo the development that allows the UCC to disappear, and it remains in place. For an extreme example, tadpoles develop into frogs and there is a genetic basis for this development. Imagine a genetic defect that doesn't allow this development to take place, so that the animal remains a tadpole all through life. Genetic Uniformity applies the same logic, though SLI is subtler than the tadpole/frog case, only affecting a much smaller part of the organism.

Bishop *et al.* started with a large set of twins, each member of which they labeled as *Language Impairment Risk (LI risk)* or *Low Risk* on the basis of parental report at age 4. They selected 87 MZ twin pairs and 86 DZ twin pairs to study. The pairs were selected so that the numbers of pairs in three groups were about equal, the three groups being ones in which neither child was at LI risk, one child was at LI risk, or both children were at LI risk. At age 6, the investigators performed a battery of tests on the children. Unsurprisingly the LI risk children did more poorly, but that is not the point of the study. For that, we have to look at the battery of tests.

The battery included IQ tests, including Verbal and Performance IQ, and various sub-tests of a standardized test for language, "Listening to paragraphs," "Sentence Structure," and "Recalling sentences." There were two measures used that were of interest because they had been proposed in the literature as being a good means to identify children with SLI. These included what the authors called "Phonological Short Term Memory" (Phonological STM) and "Verb Inflections." The measure of Phonological Short Term Memory is Non-Word Repetition (NWR), how well the child repeats a nonword that has the phonological structure of English. Since there is no interpretive support, the memory is taken to be completely "phonological." The nonwords varied in number of syllables. It is well known that the longer the word, the more difficult it is to repeat back correctly. The particular test used was the Children's Nonword Repetition Test (Gathercole, Willis, Baddeley, and Emslie 1994).

The idea of Phonological STM (Baddeley, Gathercole, and Papagno 1998) is that when children are developing language, they have to learn from input strings that they cannot yet analyze. In order to take these strings in, analyze them and perform learning using them as data, the child has to be able, among other things, to perceive the string and remember it long enough to work on. If the child couldn't do this, how could she learn?

It seems obvious that such a capacity must be necessary. Suppose a child has to learn that a lexical item is a verb and that verbs in that language follow their objects (as in Japanese). Even suppose the child understands and can interpret the noun phrase that's spoken, if she doesn't know the verb, she must hold it in memory together with the noun phrase object, e.g. . . . *the candy ate*. The learner has to hold *ate* together with the object in memory even when she can't interpret *ate*. So the capacity to analyze so-far unanalyzed strings must be a minimal part of a language-learning device. Of course, we know that it is only a very small part of the device needed to learn language; the arguments are legend. But that is not to deny that some such capacity is also necessary.

Whether Phonological STM defines SLI, of course, is another question. We can understand why many authors (e.g. Bishop) would like it to be such. Phonological STM is a kind of "general purpose" ability, not particularly characteristic of linguistic systems, not representing the "knowledge" level of the system, and quite acceptable to a psychologist who

doesn't want to assume much is special in inheritance or the brain about human language. So it is a very appealing proposal to psychologists of such beliefs.

On the other hand, Phonological STM (PSTM) has a very difficult time explaining the facts of SLI, as I have described them. Why should there be an OI stage, in particular? Well, perhaps if PSTM is very weak, perhaps a child can't remember the verb and its inflection, so the child can't learn the appropriate inflection in the right semantic "context" and thus omits the inflection. Such a characterization might at least at first sight work for English, where the OI in main verbs in present tense is mostly the use of the stem instead of 3rd-person singular s, or for OI's in past tense, where the stem replaces ed or the irregular vowel change or other irregular change. It looks like the stem is simply omitted. As we know since Wexler (1990, 1992, 1994), however, this isn't the general, or even most frequent case; it applies to English but not other languages. And that's the reason the OI stage wasn't first discovered in English, despite the large number of studies of early development in English. In other languages, the verb that children mistakenly use often adds to the stem, and is the infinitive. This is true for TD and SLI children. Why should the child make that mistake on the PSTM account? There seems to be no explanation. Rather, we'd expect the omission of the inflection, just as in English.

Even more strikingly, why should NS/OI hold? If it's a question of PSTM, surely Italian-speaking children with SLI should have the same difficulties. Shouldn't they produce infinitives instead of finite verbs?

One could go on to the fact that children with SLI don't make many subject/verb agreement mistakes, showing that the verb + inflection span is not so difficult for them, to the use of default accusative case as subjects of non-finite verbs in English (how can PSTM explain this?), to particular patterns of auxiliary omission, to clitic omission in only some languages, to the child's early mastery of wh-questions (OI-age children produce almost no *in situ* wh forms in English, unless they're echo questions, de Villiers 1995), which seems to demand a much longer span of sentence analysis than a verb plus inflection, among much else. The facts of EOI and EUCC don't follow from difficulties in Phonological STM.

Still, it's possible that there are other types of SLI, one defined by PSTM, and one defined by something more directly grammatical. Most studies haven't tested both PSTM and finiteness rates in the same children. It is a virtue of Bishop *et al.* that they did, as we will now describe.

The second interesting variable that Bishop *et al.* study is finiteness rate in obligatory contexts, as we have been considering in this chapter. They call the measure "verb inflection," although they sometimes refer to it as "% verbs inflected," which pretty well describes what I've got as finiteness rates in obligatory contexts. It is the same measure as we've discussed. The authors write, "Use of verb inflections was tested using pre-publication

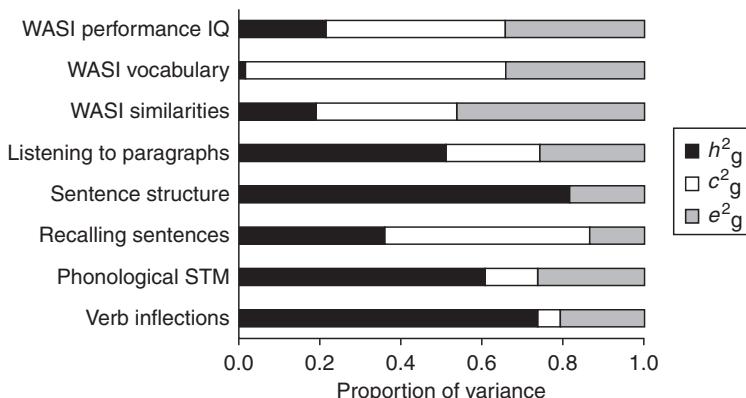


Figure 8.1 Proportions of variance in language-deficit status attributable to genes (h^2_g), environmental influences shared by both twins (c^2_g), or other influences (e^2_g). The “other” term incorporates influences traditionally referred to as nonshared environment and includes measurement error. WASI, Wechsler Abbreviated Scale of Intelligence; STM, short-term memory.

versions of two subtests from the Rice–Wexler Test of Early Grammatical Impairment (Rice and Wexler 2001).¹⁰ The first sub-test was an elicitation experiment for past tense (the error that TD and SLI make is the OI error, omitting tense; they use the stem, e.g. *push* instead of *pushed*). The second sub-test was an elicitation experiment for 3rd-person singular present tense (again the TD and SLI child error is to omit s, *push* instead of *pushes*). The measure then is % use of *ed* or *s* (the experimental contexts make these obligatory).

Each of these measures was then subject to a heritability analysis. The results may be seen in Figure 8.1 from Bishop *et al.* (2006). The black regions indicate the proportion of variance due to genes. The white regions indicate the determination of environmental influences shared by both twins, for example, effects of their particular parents. The gray regions indicate other influences. As Bishop *et al.* say, these other influences are often thought of as the environment, but not shared.

What’s inherited? In fact, Phonological STM shows a great deal of inheritance; inheritance explains about 60% of the variance. Finiteness (“Verb Inflections”) shows even more, about 77% (I’m reading these numbers off the graph). That is, about 77% of the variation in rates of finiteness is explained by genetic factors. As heritability rates go, this is quite large. Effects of shared environment on both Phonological STM and finiteness are small; on finiteness it looks to be only about 7%. That is, having a particular set of parents, with what they do, e.g. language input they provide, as opposed to another set of parents, only explains about 7% of the variation in finiteness rates. This result agrees, of course, with what we’ve already discussed concerning vanishingly small effects of maternal

education (as a measure of richness of environment) on the rate of finiteness development in TD and SLI children.

Other factors deserve at least brief mention. Note that the heritability of vocabulary size is almost zero. This too agrees with what we know – that there are large environmental effects on how large and rich a vocabulary a person develops. This is perfectly comprehensible. If a child has an environment (perhaps a parent) with a richer vocabulary, that is apt to be used around the child, she will develop a larger vocabulary. One parent might speak to the child more than another, delivering more input for vocabulary development. One parent might read to a child more, and this might help vocabulary development. A parent with more education might be able to send the child to a school that provides a richer environment, and this too will lead to a larger vocabulary. Vocabulary is learned item by item, and environment matters. We see the behavioral genetic effects of that set of considerations in this graph.

Moreover, the fact that certain behaviors that are part of language (vocabulary size) show such a different inheritance pattern than language gives us reason to have more faith in the results. It matters *what* we are studying – it isn't that everything is strongly heritable.

The only measure that is more heritable than language is the sub-test on “sentence structure” at around 83%. The authors don’t tell us exactly what is in that test,¹¹ but they do tell us it’s a test of understanding (comprehension, I presume) of syntactic structure and that passive structures and embedded clauses are in it in some form. Exactly what is driving its heritability?

8.5.1 Is UCC the only property that matures or matures away in SLI? A brief detour from finiteness

The issue is important because we’d like to know exactly what grammatical properties are delayed in SLI. We know that finiteness in some languages is, that clitics are, that UCC holds for SLI for a long time, or indefinitely. But how about other maturing aspects of grammar? Let’s consider two alternative hypotheses:

- (9) Two Alternative Hypotheses. In SLI,
 - a. UCC holds for an extended or, perhaps, indefinite period, or alternatively,
 - b. All aspects of grammar that mature in typically developing children take an extended period, or indefinitely, to mature.

In between, of course, there might be other hypotheses. Perhaps the UCC and some particular other properties are delayed in SLI, but not all maturing properties of grammar. We know that some properties of the inflectional system of grammar are well known to children with SLI (subject/verb agreement, plural marking, the necessity for *ing* on a present

participle in English), but these are properties that develop very quickly in young TD children, without an extended maturational period. We don't really know about verbal passive. One unpublished study shows that verbal passive in English-speaking children with SLI is delayed compared to age-matched controls, but isn't delayed compared to language-matched controls. That result makes it look like verbal passive delayed in children with SLI only has some kind of general delay, but not specifically as a trait, that is, it shows a much weaker pattern of delay than finiteness. We might then expect that children with SLI will grow out of the delay in verbal passive, unlike what seems to happen with finiteness.

On the other hand, the essential studies really haven't been done with verbal passives in children with SLI. It's well known that young children before about age 8 do better than their syntactic capacities allow on verbal passive, because they substitute the adjectival passive, that they do know, as a strategy that allows them to interpret the sentences in the experiment well enough (Borer and Wexler 1987 and many subsequent publications). In languages in which the verbal passive is not homophonous with the adjectival passive, children do much worse on verbal passive (e.g. Greek; Terzi, and Wexler 2002). Even as theories have been changed as to what exactly is the grammatical capacity that is missing in children that makes verbal passive so difficult, this use of the adjectival passive as a strategy has proven crucial. The crucial test in English that shows how slowly passives develop is a test that uses verbs that form syntactic passives but do not form adjectival passives. The standard case is verbs with subject experiencer, e.g. *like*, *see*, etc. Children develop most slowly on these, and even when about age 5 they do pretty well on passives of "actional" verbs (verbs with agents and patients), they do poorly on verbs with subject experiencer verbs until about age 8.

Yet, to my knowledge there are no experiments on children with SLI on passive constructions with subject experiencer verbs. Until those studies are done, we will have to wait before we can claim that verbal passive is not delayed in children with SLI to the same extent that finiteness and other UCC effects are. It may turn out that on passives of subject experiencer verbs, children with SLI perform more poorly than do language-matched younger children. In which case, we'd have to conclude that what is taken to mature in syntactic passives (the notion of "defective" phase, according to Wexler (2004)'s analysis) matures even later in children with SLI. The experimental test is open. My own view is that far too little is known about other grammatical constructions and processes in SLI to choose strongly between (8a,b).

8.5.2 Back to the heritability of the loss of UCC

So the Bishop *et al.* data show that finiteness is greatly heritable. This is good evidence for the maturational conclusions that we've drawn, and

gets us a long way into Luria's Biolinguistic Suggestion. But we've also seen that Phonological STM is also quite heritable. What does this tell us about SLI? Those with a bias toward "general purpose" theories, theories in which there are only a few simple psychological processes and all else follows, basically an assumption that there is nothing special about language, might propose that Phonological STM difficulties were the underlying cause of UCC difficulties, of the OI stage. I have already argued how implausible this is, how the properties of the OI stage in no way can be seen to follow from difficulties in Phonological STM; the calculations of the theory of STM, if there were such, would make the wrong predictions. So far as I know, there is no attempt to cash out the claim, no attempt to show computationally how the properties of SLI follow from PSTM difficulties. And one can see why; the properties don't follow on any common-sense analysis of what PSTM might mean.

Nevertheless, it would be helpful to see if the genetic data could be brought to bear on the question. Bishop *et al.* undertake exactly this task. First, let's point out that Bishop *et al.* do a Defries-Fulker analysis (for details see the paper) that shows, of all their measures, only PSTM, finiteness, and Sentence Structure were significant predictors of language impairment. They also write, "These three measures were striking insofar as they showed no significant effect of environmental influences that are common to both members of a twin pair." So putting aside Sentence Structure, the details of which we don't know right now and which we've discussed, let's see how Bishop *et al.* bring behavioral genetic data to bear on the question of one heritable property being the cause of another property.

They gain purchase on this question by asking, do the same genes cause different language deficits? If the answer to this question is yes, it is plausible (if unproven) that the underlying condition of one deficit causes the other deficit. If the answer to this question is no, it seems extremely implausible that one condition is the cause of the other. How do they study this question?

First, the Pearson correlation between the measures of PSTM and finiteness for the whole sample of 346 children was only .299, as they say "statistically significant but weak."¹² Standard analysis tells us that one of the measures only explains about 9% of the variance in the other measure. This doesn't look like one measure is the cause of the other. The authors write that, "This suggests that they are not different manifestations of the same underlying ability."

They go on to a stronger test, a test that can be used to determine whether two measures are caused by the same genes. If finiteness and PSTM are due to the same genetic causes, then it is plausible (not proven) that one measure is somehow the cause of the other. If they have distinct genetic origins, it would be quite unlikely that one measure was the cause of the other. For suppose PSTM "caused" the OI stage. But the OI stage

arises from different genes? How could these facts be consistently put together?

Here's how the test goes, in the informal description the authors give (we'll skip the more technical description):

A stronger test of common origins of the two deficits is given using a bivariate extension of DF analysis (see Purcell et al. 2001). In this method, one identifies probands on the basis of low scores on one measure (X) and then considers whether one can predict scores of their co-twins on another measure (Y). If the prediction is stronger for MZ than for DZ twins, this points to shared genetic origins for X and Y. . . . Another statistic that can be estimated from bivariate DF analysis is the phenotypic association between X and Y, . . . If X and Y are unrelated, this should be zero; if they are equivalent, it will be 1. Bishop, Adams, and Norbury (2005) p. 164

The results are that "bivariate DF analysis gave estimates close to zero for bivariate heritability . . . of phonological STM, and verb morphology." By "verb morphology" the authors mean finiteness rates, as described. In other words the genetic basis of phonological STM and finiteness are distinct, and the results are quite strong, not ambiguous. The authors conclude that "The current study expands the list of factors that seem inadequate to account for grammatical impairments: the heritable deficits in verb inflections and syntactic comprehension seen in our sample cannot be explained in terms of weak phonological STM, low IQ, poor articulation or vocabulary limitations. Thus, most of the domain-general candidate explanations that have been put forward to explain grammatical deficits in SLI are inadequate to account for this pattern of results."

The authors point out that a general-purpose measure may yet be found to explain language impairment, but there does not appear to be a proposal that they think bears any plausibility at the current time. Of course, one never knows what will be found. It is particularly difficult to make predictions of course, because the "general-purpose" idea has no detailed theory behind it, so it's difficult to see from where a proposal could be made.

At the present time one must conclude, to the extent that we have evidence, that there is heritability of grammatical development; the rates of development depend on the genome, and to the extent that we have any evidence, these rates are not intermediated by any known general-purpose structure.

How can we account for the fact that both SLI and phonological STM are strong, genetically based predictors of SLI? Why do different studies find different results? It's quite possible that different studies pick different populations; as I already pointed out, extremely few studies measure both NWR and OI rates. A simple hypothesis is that there are two kinds of SLI; one based on the EUCC, a grammatical computation deficit, and one based on phonological STM, a memory/processing deficit. There is no reason in

principle why both couldn't be possible and co-exist. Bishop *et al.* found a small amount of greater overlap between the two deficits than expected (see the paper for an interesting discussion of possibilities to explain this result) but mostly cases of one deficit and not the other. So, perhaps we should consider that there are two kinds of SLI, as described, and plan our studies accordingly. One excellent innovation would be to study both OI rates and NWR in the same studies. Possibly we'll find that populations that are selected on the usual general grounds (lagging on standardized tests, clinical referrals, etc.) actually comprise two genetically distinct subtypes. Separating the groups might provide even cleaner results than attained by data.

These results look like Luria's Biolinguistic Suggestion worked out in some detail in a particular case. Of course, as I have already suggested, the case may be more general; the research is only starting. Perhaps there is a more general syntactic property subject to variation. But all properties aren't the same. For example, the development of finiteness and other UCC properties in TD children is pretty much over in English by about age 4; the development of verbal passives in TD children takes until age 8 or 9. Even if both developments turn out to be typical of SLI, say, we still want to know why one takes longer than the other, how the genes direct the physiology, why it takes particular lengths of time, etc.¹³

8.6 Genetics

Biology takes behavioral genetic data seriously, as part of its enterprise, so the integration of which Luria wrote has in fact occurred. But biology also goes deeper by looking at the physical structure of the inheritance, by finding genes that are responsible for a particular development. Is this science fiction for language, or is it real? Amazingly enough it is real. Here I can only give the briefest sketch of what has been accomplished in the area relevant to finiteness, which is, in fact, the major particular grammatical area that has received genetic attention.

Most of the work has been accomplished via the method of studying SLI, where one sees larger and longer lasting variation in rates of development of finiteness, as we have seen. Geneticists and SLI researchers, working together (mostly under the banner of the SLI Consortium in the UK), have identified two regions that seem to relate to particular proposals for the underlying deficit in SLI: phonological working memory and something more related to grammar, in fact tense marking, the OI stage phenomena.

I will only review (and briefly) one paper, by Falcaro *et al.* (2008). Previous to that, the SLI Consortium had identified regions of the genome that correlated with measures related to SLI. *SLI1*, a region on chromosome 16, has been linked to Nonword Repetition, the measure of PSTM that we have already discussed. *SLI1*, a region on chromosome 19 also linked to SLI,

but more ambiguously for various reasons as discussed by Falcaro *et al.* No measure of OI rate had previously been used in these studies. Falcaro *et al.*, taking off from Bishop *et al.* (2006) that we have just discussed, undertook a genetic analysis by using both NWR and OI measures, asking how these measures related to genetic regions.

The genetic method studied 93 children with SLI (the “probands”) and their first degree relatives, 300 of them. A crucial point of the interpretation of the data is that the children with SLI had a mean age of 14;5 (range 13;1–16;2). The relatives, of course, had a great span of ages. Even the youngest group, the “younger siblings” had a mean age of 12;4. The ages are important because to the extent that some of the results on finiteness rates are weaker than we might like, we should remember that simple finiteness rates at these ages do not show strong effects.

The OI (finiteness) task used was the Past Tense (PT) task of Marchman, Wulfeck, and Weismer (1999). The authors write, “Participants are shown a drawing of everyday activities and asked to verbally fill in the missing word that the assessor leaves out while reading a sentence to them. The task comprises both regular and irregular verbs and is of the following type: ‘The boy is walking. He walks everyday. Yesterday, he …’. Each answer is classified as correct or incorrect and the test scored as the total number of correct PT inflections …” The results were then standardized (using other studies) and the standardized measures used in the genetic analysis. Past Tense in this sense is one measure of OI rate.

So far as I can tell, overregularizations (*throwed* instead of *threw*) are counted as an error in this paper. We know, however, that overregularizations are not particularly related to the OI stage. The crucial measure for OI rate (finiteness rate) would include *throwed* in this example as finite. OI children can choose *throw* (the OI), *threw* (correct), *throwed* (incorrect but still finite). The OI stage predictions only relate to proportion of finiteness, so to get the correct measure, *throwed* has to be taken as correct. (Alternatively, one might only use regular verbs.) If, in fact, Falcaro *et al.* counted overregularizations as incorrect, this could have introduced a good deal of noise into their data. That they still found a genetic basis for finiteness indicates that it is probably a strong relation between genes and finiteness rates.

DNA was taken from the participants, and a linkage analysis performed on markers in the candidate regions, on chromosome 16q¹⁴ (16q23.1–16q24.2) and chromosome 19 (19q12–19q13.42). I will skip the complicated details of the different kinds of analyses. In particular, a linkage analysis was performed on siblings, comparing the genetic markers in these regions and the PT score of one sib with the same for the other sib.

The study linked NWR to the candidate region on chromosome 16, further supporting earlier findings. How about the finiteness task, PT in this case? … we report new findings for the PT measure, for which to our knowledge there has been no previous molecular genetic study. When PT

was measured on the continuous scale, the DF models suggested linkage on both chromosomes but evidence for linkage was stronger on chromosome 19. When a binary measure for PT (cut-off of 1.5 SD) was considered, linkage was only observed on chromosome 19, not on chromosome 16. This is consistent with there being distinctive genetic bases to PT and NWR and similar to the Bishop *et al.* (2006) twin analysis finding of limited sharing of additive genetic variance for grammar and phonological short-term memory. However, in the context of the heterogeneity of findings that we often see across samples and measures, we again do not consider this strong evidence. Nonetheless, these results suggest that the SLI2 region on chromosome 19 is worthy of further investigation using larger samples and a refined range of measures.

So there was evidence for PT being specifically related to the identified region on chromosome 19, but the evidence that it was related only to chromosome 19 and not 16 only appeared when a cut-off for impairment was assumed. When the measure of PT was continuous, PT appeared to be related to both regions, though it was more strongly related to chromosome 19.

Why this somewhat differential result was obtained is not clear, but we should first note (as the authors do) that the children with SLI were quite old (range 13;1–16;2 as we said before). This means, given previous results on EOI, that their performance on the PT task would have been quite high, with not much variation. In fact, the mean score for the SLI probands was 43.09 (out of 52 perfect). This would not have provided much power to the analysis. Moreover, if overregularizations were included as errors, many of the errors could have been overregularizations (the exact facts of the analysis are not reported, so far as I can find). So the fact the PT nevertheless related to the SLI2 region on chromosome 19 under any analysis is quite suggestive.

In an attempt to understand the difference in the strength of the results due to the continuous or cut-off methods, Falcaro *et al.* write:

We want to take this argument further and suggest that tense marking may not be a phenotypic trait that is measurable as a continuous dimension across development but instead may be a skill in which competence is either acquired or not acquired by early school age, comparable to a Piagetian stage in learning. In this sense, qualitative distinctions in the trait is what appears to be familial, while quantitative variability is likely to be more a consequence of non-familial factors, notably age and others, which may well be related to age, e.g. motivation/attention to task. p. 399

Clearly in TD, by “early school age,” tense marking is acquired, and any deviation from it is some kind of performance error. But quantitative variability must have genetic causes, whether in TD or in SLI. We know from Bishop *et al.* (2006) that degree of tense marking is strongly inherited – of course this is at an age when it hasn’t fully developed. Of course

quantitative variability in tense marking is a function of age – we know this from large numbers of studies of the OI stage. But the suggestion that factors like “motivation/attention” are of major importance is at odds with the facts, if it means that the quantitative measure is a measure of motivation or attention. Consider Italian, which doesn’t show an OI stage. Are we to infer that all Italian children are more “motivated” or pay more “attention” than all English-speaking children? Or within English, if it’s lack of attention or motivation, why don’t English-speaking children in the OI stage produce sentences with subjects and verbs not agreeing as often as they produce OI’s. There are many other facts that don’t seem to agree with the suggestion.

Similarly, following Ullman (2001a), the authors suggest that “procedural memory” (defined as *the acquisition of new skills, both motor and cognitive, over multiple trials without the need of conscious awareness ...*) might be the cause of the lack of correct use of past tense. Doubtless skill learning would have such a component. But to apply it to the acquisition of finiteness makes no sense, given the facts. The authors say it can explain difficulties in accessing “appropriate endings to inflect verbs” but why then aren’t there agreement errors? Even more strikingly, why do finite verbs end up in finite positions? A brief glance at the overpoweringly strong Dutch data from Wexler, Schaeffer, and Bol (2004) shows us why the OI stage (and Falcaro *et al.*’s PT experiment) doesn’t show a problem with accessing endings. Only finite verbs end up in V2 (we haven’t shown it, but the same is true for children with SLI – the data is in the same paper as the TD paper).

We have come far in the study of the OI stage. From typical development, integrating a large number of different looking constructions, with explanatory models, serious evidence across a number of different fronts on psychological/biological looking variables that the UCC grows as a result of genetic effects, strong relations to language impairment, behavioral genetic evidence that argues that there is no general purpose factor that has been proposed that could cause SLI, and now progress towards identifying genetic regions that might be part of the cause of the OI stage. (The evidence concerns impairment, but we are working on the assumption of genetic uniformity, so we would expect that the delay is caused by the lack of genetic action that causes TD development, of UCC weakening over time.)

8.7 Physiology (neural computations): Possible for OI stage?

There is one other level that is quite relevant for Luria’s Biolinguistic Suggestion, the level of physiology. Genetics is not physiology. If we uncover genes, this doesn’t tell us what they do to the brain, but it is a beginning on that task. Is there any other way of getting at physiology?

The method that looks most promising at the current time is a method that studies what goes on in the brain as linguistic analysis is done. There has been very little on this topic with respect to the OI stage. I can report however that research is now taking place between my laboratory and John Gabrieli's fMRI laboratory that attempts to see what can be learned about the brain and OI's. We started by studying grammatical judgment tasks in adults. We studied judgments of grammatical sentences, and two kinds of ungrammatical sentences, sentences in which subject/verb agreement was violated (e.g. *John am leaving*, not the actual example) and OI's (e.g. *John leaving*). The behavioral results are interesting and important, but let me just mention the imaging results. OI's show up as showing a left Broca's area effect. That is, subtracting the bad agreement sentences from the OI sentences yields a response in left IFG, a classic language area.

It will take much research to pin down the details, and I have nothing to report yet about children. Suppose we do see that Broca's area is involved in OI's. What then? One important fact is that Broca's area takes time to develop; this is well known. An obvious hypothesis springs to mind? Could the late development of Broca's area be a cause of the OI stage?

Even if such elegant explanations turn out to be true, they are radically incomplete. What is the nature of the computational limitation imposed by a lack of development of Broca's area? Why does the lack of development of Broca's area cause the UCC? How do the neural networks work that impose the UCC? Physiology of the brain with relation to language is in its infancy; mostly we have evidence that areas of the brain are involved in certain types of competence or processing. This is true more generally of anything in cognition. To my knowledge, there is no model or theory of cognitive neuroscience, in any area, for anything beyond the sensory level.

8.8 Some genetic questions to research

- (10) What are the possibilities? What should be done?
 - (a) We have to dig much deeper into the UCC stage. First, for the OI stage, there are many effects, and these should be studied. Suppose we study auxiliary omission and its genetic basis? It should be the same as Past Tense. Past Tense should be studied more precisely, as I have argued above.
 - (b) OI rates in languages that mark the infinitive and aren't null-subject (Dutch, French, etc.) should show the same genetic basis as PT in English. Are these on chromosome 19? I would expect a language like Dutch would make an even clearer and neater test than English in this regard, as the infinitive can't be the result of an omission process. We have seen how clean the OI results are with respect to verb placement.

- (c) Different languages should show different effects. There shouldn't be an effect of particular genes on development of finiteness in, say, Italian. This almost has to be true, as there will be little rate of development of finiteness in Italian, even for children with SLI.
- (d) Study Icelandic, and the genetic basis of finiteness development in that language. The OI stage is very clear for some interesting reasons in Icelandic. The infinitive is audibly marked also. We predict that SLI in Icelandic should show a clear EOI effect. Moreover, since Icelandic is a relatively homogeneous country genetically, that should help in the search for genes. There are also good records of family history.
- (e) More strikingly, other UCC properties are predicted to have the same genetic basis as finiteness. The most striking prediction at the moment would be that if we study clitic omission in French or Italian or a similar language, the genetic basis should be the same as for the development of finiteness in English.
- (f) Turn to other developmental stages of language. How about:
- (g) Passives and related structures. In a passive study, Ganger, Dunn, and Gordon (2005) showed that in TD, subject experiencer passives have a large inherited component and a very small environmental component whereas the passives of actional verbs had a large environmental component and little inherited component. This makes a great deal of syntax if the syntactic ability is maturational and the strategy that allows a child to understand the actional verb passive is affected by the environment, e.g. motivation, learning, etc. The possibilities for study are endless.
- (h) One could go on. Semantics of Determiners? Maturational? They turn out not to relate to Theory of Mind, as we all thought, but that is a story for another day.

8.9 The state of biolinguistics in the growth of language

For language, I would conclude, there is a serious possibility of Luria's Biolinguistic Suggestion providing a model for how results can actually be obtained – by seriously integrating biology and linguistics. It is obvious how biology plays a role, we have discussed that throughout the chapter. But it is also obvious how linguistics plays a role; we have also discussed that throughout the chapter. Without linguistic analysis, the whole question of the OI stage would have been impossible and the array of facts that are now on the table would never have been accumulated.

I have spent two paragraphs pointing out the obvious deficiencies in Falcaro *et al.*'s suggestions for explaining variation in tensing rates because I want to point out here that although I have been trying in this paper to

show how biological data and ideas can add to the study of language, how Luria's ideas can be instantiated, we have to realize that his idea has two components, the integration of two types of data and analyses. Collecting data on English OI's and doing genetic analysis is very helpful, but one cannot understand the implications of such data unless one understands the great array of experimental findings about the OI stage, an array of findings integrated and made possible through linguistic theory. There is no Biolinguistics without linguistics. This is not a rhetorical statement, but an extremely practical, science-oriented statement.

In fact, I would submit that Luria knew exactly what he was doing. He called for the integration of linguistic theory and biology. He didn't say there should be an intermediary from psychology. My own view is just that. Namely, we have reached the stage of development in linguistics and in biology that we should try to directly integrate the concepts, looking for genetic and physiological (neural) underpinnings of linguistic concepts. "Psychological" concepts (procedural memory, etc.) should be avoided if at all possible. They may be necessary in areas where we are confused, have nothing to say, are trying to understand a miasma. But my "suggestion" is that we attempt to integrate linguistics and biology in a direct manner. These are two sciences with relatively clear concepts and a fairly obvious intuitive reality. Inventing psychological intermediaries may be like inventing the ether. In this chapter I have proposed one direction, a direction that has attempted the route described by Luria's Biolinguistic Suggestion, with promising results. We are in the infancy of what may turn out to be a hugely exciting, and overdue, development.

9

Parameters in language acquisition

Lisa Pearl and Jeffrey Lidz

9.1 What a parameter is (or is meant to be), and what it's for

A parameter, in its simplest conception, is an abstraction that can account for multiple observations in some domain. A parameter in statistical modeling, for example, determines what the model predicts will be observed in the world in a variety of situations; a parameter in our mental model would determine what we predict will be observed in the world in different scenarios.

In statistical modeling, there are numerous parameters defined mathematically to account for the expected shape of data distributions. Take the Gaussian distribution, sometimes called the “normal distribution” or “bell curve,” which can be used as a simple statistical model to explain complex phenomena (such as “how many minutes late to class I’ll be”). In this model, the value of the measured variable (X in [Figure 9.1](#) below) tends to cluster around the mean μ . Two parameters are used to determine the shape of the curve that represents the expected distribution of data: the mean μ and the variance σ^2 . These parameters are part of the fixed function $\varphi(X)$ (note the y axis label in [Figure 9.1](#)) that produces the probability of X having a specific value:

$$(1) \quad \varphi_{\mu, \sigma^2}(X) = \frac{1}{\sqrt{2\pi\sigma^2}} e^{-\frac{(X-\mu)^2}{2\sigma^2}}$$

While the function itself is invariant, the predictions produced with this function vary based on the values of μ and σ^2 . Changing the value of these parameters demonstrably changes the expectation of how many data points with particular values we expect to see (or comparably, how often we expect to see a data point with a particular value).

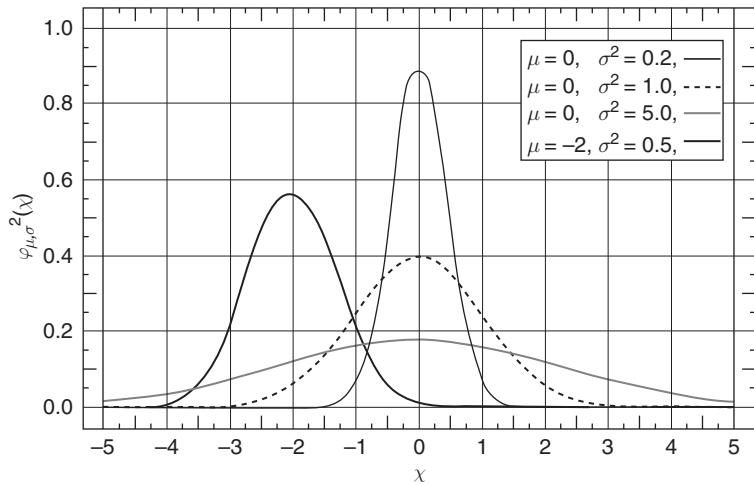


Figure 9.1 The Gaussian distribution, with different values of μ and σ^2 . Courtesy of Wikipedia Commons.

In a related fashion, imagine that we are trying to determine the values of μ and σ^2 in the function $\varphi(X)$ for some data that we observe. Observing different quantities of data with particular values can tell us which values of μ and σ^2 are most likely. For instance, if we observe 1000 data points whose values are mostly between -3 and -1 (perhaps producing a curve like the leftmost one in Figure 9.1), we would be more likely to think that μ is -2 rather than 0 , and that smaller values of σ^2 are more plausible than large values. Even if we have never seen a data point with value 5 , we can expect that this data point is not very likely to occur, given what we have inferred about the data distribution and how we can describe that data distribution with the values of μ and σ^2 . Importantly, we do not see the process that generates the data, but only the data themselves. This means that in order to form our expectations about X , we are, in effect, reverse engineering the observable data in order to figure out how these data were generated, given what we (perhaps unconsciously) know about function $\varphi(X)$ and variables μ and σ^2 . Our knowledge of the underlying function and the associated parameters that generate these data allows us to represent an infinite number of expectations about the behavior of variable X . Indeed, within the cognitive sciences, learning is generally conceived as this kind of inverse problem: Given a set of observations (e.g., sentences), learning consists of identifying the underlying system that determined those observations (e.g., the grammar) (Chomsky 1965; Gallistel 1990, *inter alia*). Notably, and perhaps particularly relevant for the nativist view of language, knowing the function allows us to generate an infinite number of expectations that are nonetheless constrained in their behavior (here, constrained to obey this underlying function). Thus, the hypothesis space consisting of the infinite expected behaviors, given

this function, is still smaller than the hypothesis space consisting of all possible behaviors. Knowing the function thus narrows the hypothesis space of expected behaviors, while still allowing an infinite number of expected behaviors.

Meanwhile, statistical parameters of a function can help us describe complex phenomena in a very compact way. By knowing one function and the values of only two parameters, we can define a Gaussian data distribution that predicts how likely we are to see a data point with a particular value (“I’ll be 15 minutes late to class,” $X=15$), without necessarily observing that particular data point a large number of times – or even at all. This, of course, is just one example of parameters used in statistical modeling. There are numerous others, some of which have been recently used in statistical modeling of language acquisition (see, for example, Tenenbaum, Griffiths, and Kemp 2006; Goldwater, Griffiths, and Johnson 2009; and Perfors, Tenenbaum, and Wonnacott 2010 for examples of hierarchical Bayesian modeling that use a small number of parameters).

In the domain of human language, both principles and parameters are often thought of as innate domain-specific abstractions that connect to many structural properties about language. Linguistic principles correspond to the properties that are invariant across all human languages. Using our statistical analogy from before, the equation’s form is invariant – it is the statistical “principle” that explains the observed data. Linguistic parameters correspond to the properties that vary across human languages. Again using our statistical analogy, the statistical parameters of μ and σ^2 determine the exact form of the curve that represents the likelihood of observing certain data. While different values for these parameters can produce many different curves, these curves share their underlying form due to the common invariant function in (1).

Since linguistic principles do not vary, they might not need to be learned by the child. Parameter values, on the other hand, must be inferred from the language data since languages do not all have the same parameter values. As with the statistical parameters above, children must reverse engineer the observable data to figure out the most likely way these data were generated. Even given knowledge of linguistic principles and linguistic parameters, children must still decide the most likely value for any parameter.

The fact that parameters connect to multiple structural properties then becomes a very good thing from the perspective of someone trying to acquire language. This is because a child can learn about that parameter’s value by observing many different kinds of examples in the language. As Hyams (1987) notes, “the richer the deductive structure associated with a particular parameter, the greater the range of potential ‘triggering’ data which will be available to the child for the ‘fixing’ of the particular parameter.”

This idea figures prominently in hierarchical Bayesian models (see Kemp, Perfors, and Tenenbaum 2007 for an accessible overview), where a learner can make generalizations about the observable data at different levels of abstraction. To take a non-linguistic example from Kemp *et al.* (2007), suppose a learner must learn something about bags containing marbles. Suppose this learner observed twenty bags, each of which is filled with 100 black marbles or 100 white marbles. This learner then sees a new bag, and is only shown one marble from it, which happens to be purple. A hierarchical Bayesian learner will likely make the following inference at this point: All the marbles in the new bag are purple. The learner can make this inference because it is able to learn something about properties of bags in general, sometimes called an “overhypothesis” (Kemp *et al.* 2007), instead of only about bags containing black or white marbles. Specifically here, bags seem to contain only one color of marble. That is, by observing individual bag examples, the hierarchical Bayesian learner has learned something about how to compactly represent the structure of those bags’ composition – and it infers this property will apply to bags in general. In this way, the more abstract knowledge of marble bag composition has been triggered by many individual examples of marble bags. More interestingly, this learner was able to have certain expectations about a marble bag containing a novel marble color by learning from bags containing only black or white marbles.

Translating this to a linguistic example, suppose the marble bags are individual sentences. Some sentences contain verbs and objects, and whenever there is an object, suppose it appears after the verb (e.g., *see the penguin*, rather than *the penguin see*). Other sentences contain modal verbs and non-finite main verbs, and whenever both occur, the modal precedes the main verb (e.g., *could see*, rather than *see could*). A learner could be aware of the shared structure of these sentences – specifically that these observable forms can be characterized as the head of a phrase appearing before its complements (specifically $[_{VP} V NP]$ and $[_{IP} Aux VP]$) – and could encode this “head-first” knowledge at the level that describes sentences in general, akin to a hierarchical Bayesian learner’s overhypothesis. This learner would then infer that sentences in the language generally have head-first structure. If this learner then saw a sentence with a preposition surrounded by NPs (e.g. *penguins on icebergs are adorable*), it would infer that the preposition should precede its object ($[_{NP} penguins [_{PP} on icebergs]]$ and not $[_{NP} [_{PP} penguins on] icebergs]$), even if it had never seen a preposition used before with an object. In this way, the notion of a head-directionality parameter (discussed further in Section 9.3) can be encoded in a hierarchical learner. More broadly, parameters are similar in spirit to the overhypotheses in a hierarchical Bayesian learner: Inferences (e.g., about prepositional phrase internal structure) can be made on the basis of examples that bear on the general

property being learned (e.g., head directionality), even if those examples are not examples of the exact inference to be made (e.g., verb phrase examples).

Parameters can be especially useful when a child is trying to learn the things about language structure that are otherwise hard to learn, perhaps because they are very complex properties themselves or because they appear very infrequently in the available data. The way it would work is this: Each hard-to-learn property is linked to a parameter that has at least one easy-to-observe property connected to it. Through the easy-to-observe property, children learn about the value of the parameter that is connected to it. Through the parameter, the hard-to-learn property follows, perhaps even without seeing any data that directly instantiate it. Thus, the parameter would allow children to observe data that fix the value of the hard-to-learn property. So, structures that may not have been directly observed can nonetheless be generated by the grammar.

If we think about the hypothesis space of a child trying to acquire language, having parameters can make acquisition a lot easier (as argued by Chomsky 1981b, 1986a). Instead of needing to identify the language properties individually, the child now has a built-in shortcut since some properties are connected to other properties, and can thus be acquired on the basis of fewer (and perhaps less varied) observations. For example, suppose we have 5 different structural properties in the language, each of which is binary. If each property has to be learned individually, there are 2^5 possible ways these 5 structural properties may be set. So, the child's hypothesis space contains 2^5 hypotheses of how the language could work. To learn each property, the child must see data pertaining to each property. (This many not seem so bad right now, but languages clearly have many more properties than this. See Section 9.3 for some suggested parameters.) Suppose now that there is one binary parameter connected to each of these 5 binary language properties. This means that if that one parameter is set to "yes," all 5 connected properties have their values set definitively one way (either to "yes" or to "no," depending on the property). If the parameter is instead set to "no," the 5 connected properties have their values set definitively the other way. So, the child's hypothesis space now contains only 2 hypotheses of how language could work, one with the parameter set to "yes" and one with the parameter set to "no." Moreover, the child can learn about properties 2, 3, 4, and 5 by observing data only for property 1 and learning what the parameter's value is. This would seem to be much easier than learning each property individually.

So, in sum, the word "parameter" is simply the name given to those abstract features of grammar (a) that govern many different observable structures and (b) that vary from language to language. Its use in both the theory of language acquisition and the theory of grammar typology is to condense the representation of the language, thereby structuring the

learning task for the child in such a way as to reduce the range of observations required to construct a grammar. In theory, this works by connecting together observations that might otherwise need to be accounted for independently from each other.

9.2 How do we tell what counts as a parameter?

Parameters seem useful in the abstract, but how do we define what a parameter is? How do we recognize when we have found one?

Chomsky (1981b) imagines parameters much as we have discussed them above: “we hope to find that complexes of properties … are reducible to a single parameter, fixed in one way or another.” That is, a parameter should connect to many different properties about a language, and not simply apply only for one property. Safir (1987), in fact, cautions against “describing any sort of language difference in terms of some *ad hoc* parameter” in order to prevent a parametric theory from “licensing mere description.” The point of parameters is to make the description of language structure more compact in some sense. Having a parameter for each point of variation in a language wouldn’t accomplish this very well. Smith and Law (2009) note that parameters that connect to a variety of linguistic properties are often referred to as “macro-parameters” while “micro-parameters” are those that connect to only a few properties. Macro-parameters seem to be more in line with the original spirit of linguistic parameters.

Given that positing an independent parameter for each point of variation would undercut the explanatory value of parameters, it is important to determine just what the right parameters are. Ideally, this will come from the theory of comparative grammar and there are many proposals which vary in their success at capturing the full range of observed variation (Newmeyer 2005).

One of the earliest parameters proposed concerned the nature of overt subjects. Rizzi (1982) argued that a cluster of properties varied predictably with whether overt subjects were obligatory. For example, languages that do not require overt subjects in simple declaratives allow postverbal subjects, do not exhibit expletive subjects, and do not exhibit *that*-trace effects (see 2a below). Languages that require a subject in all simple declaratives, however, do not allow postverbal subjects, do exhibit expletive subjects, and do exhibit *that*-trace effects (see 2b below).

- (2) Italian vs. English

(a) Italian

(i) –overt subject: verrá
 3rd-sg-fut-come
 “He will come”

- (ii) +postverbal subject: verrá Gianni
3rd-sg-fut-come Gianni
“Gianni will come”
 - (iii) –expletive subjects: piove
3rd-sg-rain
“It’s raining”
 - (iv) –that-trace effect Che credi che verrá?
who 2nd-sg-think that 3rd-sg-fut-come
“Who do you think that will come”¹
- (b) English
- (i) +overt subject: He will come.
* Will come.
 - (ii) –postverbal subject: Jack will come.
* Will come Jack.
 - (iii) +expletive subjects: It’s raining.
* Is raining.
 - (iv) +that-trace effect Who do you think will come?
* Who do you think that will come?

This proposal illustrated the potential benefits of parameters because it solved what was believed at the time to be a poverty of the stimulus problem for *that*-trace effects. Specifically, the problem was that English-learning children could observe the optionality of complementizers in declaratives and even in object wh-questions (3). They might then reasonably extend this optionality to subject wh-questions (2b iv).

(3) Optionality of complementizers

- (a) declaratives: Jack thought (that) Lily would eat lunch.
- (b) object wh-questions: What did Jack think (that) Lily would eat?

Such an extension would be problematic because complementizers are disallowed in subject wh-questions in the adult English grammar. However, a child with an over-general grammar (which allowed a complementizer in subject wh-questions) would never receive direct evidence that complementizers in subject wh-questions were not allowed – the few subject wh-questions they encountered (which would not have a complementizer) would be compatible with their incorrect grammar. Thus, to the extent that English speakers exhibit the *that*-trace effect, a poverty of the stimulus problem exists because the input data are compatible with a grammar that exhibits *that*-trace effects and also with a grammar that does not.

However, if the presence of *that*-trace effects followed from the same parameter setting that determined the nature of subjects, then there simply would be no learnability problem. The relevant data for determining the obligatory absence of complementizers in subject wh-questions would come from the requirement of overt subjects in declaratives and the necessity of expletive subjects. Observing those data, children would set

the parameter to the correct value, and then predict that complementizers were not allowed in subject wh-questions. There would be no overgeneralization based on the optionality of complementizers in declaratives and object wh-questions, because children would already have knowledge pertaining to complementizers in subject wh-questions due to this parameter's value, and would thereby circumvent the poverty of the stimulus problem.

The generalization that *that*-trace effects are linked to the nature of subjects has been questioned (Gilligan 1987; Newmeyer 2005), but for our purposes the example serves to illustrate the potential explanatory power of parameters. A parameter is best posited when (a) a cluster of seemingly unrelated properties varies in a systematic way across languages and (b) the parameter solves potential poverty of the stimulus problems.

Sometimes, suggested parameters only seem to connect to a single simple structural variation (for example, object before verb or verb before object in main clauses, or whether the main stress of a word is on the left or the right). Hopefully, at least one of the following happens: (1) these simple structural parameters turn out to be connected to other structural properties in the language (for example, perhaps object–verb order is connected to the order of heads and complements of phrases in general) and/or (2) these parameters are still a more compact representation of the generative system used to produce the language data than alternative options (for example, perhaps having stress parameters is simpler than individually memorizing stress contours for every word and using analogy to generate new stress contours). By doing either of these things, such parameters may help aid acquisition.

9.3 Using parameters to learn

Of course, even if the child's hypothesis space is constrained by parameters, this doesn't mean that the language acquisition problem is solved. Parameters do solve the problem of what hypotheses to consider about how a language works, and if they can be set correctly, they may alleviate certain poverty of the stimulus problems. Still, children must decide among the available alternatives – and this has proved to be a difficult decision process to model. As an example, suppose children's hypothesis space is defined by n binary parameters. This leads to a hypothesis space of 2^n possible grammars, one for each setting of the n parameters. If n is small, say 5, perhaps this doesn't seem so bad: 2^5 is only 32 options, after all. But given the variety of linguistic properties that appear cross-linguistically (see Baker (2001) for an accessible survey of some), n is likely to be larger than this. If n includes only 25 parameters, we already have a hypothesis space of 33,554,432 (2^{25}) grammars, which seems like a much more

difficult acquisition problem indeed. To converge on the right grammar in, say, eight years, a child would have to eliminate just under 11,500 potential grammars every day.

The simple fact is that, while parameters may help constrain the hypothesis space, every time we add a new one the hypothesis space doubles in size (e.g., a parameter space with 5 binary parameters is half the size of a parameter space with 6 binary parameters ($2^5 = 32$, $2^6 = 64$)). Clearly, the more we can reduce the number of parameters, the easier the acquisition problem will be. Still, from the learner's perspective, this is better than an unconstrained hypothesis space, where the number of potential grammars is potentially infinite.

Yet the number of parameters is not the only difficulty. Children must also decide among the grammars defined by these parameters using the available data, and this turns out not to be so easy sometimes. Because the parameters are abstract and govern a wide range of surface phenomena, it is not always obvious which data uniquely determine any particular parameter setting (Clark 1992; Gibson and Wexler 1994; Niyogi and Berwick 1996; Pearl 2008, 2009, 2011). Some parameters may interact, meaning that the effect of one parameter masks the effect of another parameter for a particular data point.

For example, consider the domain of metrical phonology, which describes the system for generating stress contours for words (that is, what makes “EMphasis” different from “emPHAsis”). Some researchers (Dresher 1999; Halle and Vergnaud 1987; Halle and Idsardi 1995, among others) believe that stress contours are generated by grouping syllables together into larger units called metrical feet, and then stressing various syllables based on properties of those metrical feet. Languages vary in the way that they form metrical feet and in the way they assign stress to metrical feet. Two parameters commonly associated with this view are extrametricality and foot headedness, shown in (4).

(4) Some metrical phonology parameters

- (a) Extrametricality: whether all syllables are included in metrical feet, or whether some are left out (and are therefore “extra”metrical).
- (b) Foot Headedness: whether the rightmost or leftmost syllable of a metrical foot is stressed (and so whether the “head” of the metrical foot is to the right or to the left).

Suppose a child is trying to determine both whether her language allows extrametrical syllables and what syllable within a foot is stressed. Suppose she hears the word “giRAFFE.” She might come up with the following analyses for explaining why that stress contour is observed:

(5) Analyses for “giRAFFE”²

- (a) +extrametrical (leftmost syllable), metrical feet headed on the left: The syllable “gi” is extrametrical and not included in a metrical

foot. There is one metrical foot, and it includes “raffe”. As it is the leftmost syllable in the metrical foot, it is stressed.

Analysis: gi raffe

(i) +extrametrical (left) gi (raffe)

(ii) foot-headed-left gi (RAFFE)

- (b) +extrametrical (leftmost syllable), metrical feet headed on the right: The syllable “gi” is extrametrical and not included in a metrical foot. There is one metrical foot, and it includes “raffe.” As it is the rightmost syllable in the metrical foot, it is stressed.

Analysis: gi raffe

(i) +extrametrical (left) gi (raffe)

(ii) foot-headed-right gi (RAFFE)

- (c) –extrametrical, metrical feet headed on the right: There is one metrical foot, and it includes “gi” and “raffe.” “raffe” is the rightmost syllable of the metrical foot, so it is stressed.

Analysis: gi raffe

(i) –extrametrical (gi raffe)

(ii) foot-headed-right (gi RAFFE)

Here, extrametricality seems to be masking whether the language has metrical feet headed on the left or on the right, and foot headedness seems to be masking whether the language has extrametricality. If the child knew the language was –extrametrical, the foot headedness value would be known (feet headed on the right (4c)); conversely, if the child knew that the language had feet headed on the left, then the extrametricality value would be known (+extrametrical on the leftmost syllable (4a)). However, neither value is known, and so it is difficult to tell what parameter combination generated this data point.

We can find a similar situation in syntax if we look at the parameters some researchers have called verb-second movement and head directionality. First, verb-second movement: Several linguists (Chomsky 1981b; Cook and Newson 1996; Guasti 2002; Sakas 2003; Yang 2004, among others) have described a parameter that leads to the transposition of subjects and auxiliaries in English questions (6) and to the tensed verb always being in second phrasal position in languages like German (7a,b). It should be noted that while English has this verbal movement for questions, it does not require the tensed verb to be in second phrasal position in declarative clauses (8). Thus, this verb movement parameter has different effects on observable word order, depending on clause type (declarative vs. interrogative).

- (6) Transposition of the subject and auxiliary verb in English question formation
- (a) Underlying sentence: The *penguin* must eat fish.
- (b) Yes/No question: Must the *penguin* eat fish?

(7) Second phrasal position of the tensed verb in German

- (a) Der Pinguin
- isst
- Fisch.

the penguin eats fish

“The penguin eats fish”

- (b) Fisch
- isst
- der Pinguin.

fish eats the penguin

“The penguin eats fish”

- (c) Der Pinguin
- muss
- Fisch essen.

The penguin must fish eat

“The penguin must eat fish”

- (d) Fisch
- muss
- der Pinguin essen.

fish must the penguin eat

“The penguin must eat fish”

(8) Non-second phrasal position of the tensed verb in English

- (a) The penguin usually
- eats
- fish.

- (b) Sometimes the penguin
- eats
- fish.

The head directionality parameter (Baker 2001; Cook and Newson 1996), concerns the position of phrasal heads (for example, the verb in a verb phrase) with respect to the phrasal complement (for example, the object of the verb in the verb phrase). The idea is that a language consistently has the heads of its phrases all on the same side of the complements of all its phrases, whether the head is first in the phrase (head-first: English, Edo, Thai, Zapotec) (9a) or the head is last in the phrase (head-last: Lakhota, Japanese, Basque, Amharic) (9b).

(9) Head-first vs. head-last languages

- (a) English is head-first

for the penguin: the preposition *for* is before its object *the penguin* in the preposition phrase

hugged the penguin: the verb *hugged* is before its object *the penguin* in the verb phrase

- (b) Lakhota is head-last

Jack wowapi k'uhe oyuke ki ohlate iyeye.

Jack letter that bed the under found

“Jack found that letter under the bed”

The verb *iyeye* is after its objects (*wowapi k'uhe* and *oyuke ki ohlate*) and the preposition *ohlate* is after its object (*oyuke ki*).

Suppose a German child hears the sentence in (10) and is trying to determine the value of the verb-second and head-directionality parameters.

(10) Example German sentence

Ich liebe Pinguine.

I love-1st-sg penguins

SUBJECT VERB OBJECT
 "I love penguins."

Here, the verb's complement is the object (*Pinguine*), and the child might come up with the following analyses to explain the observed word order:

(11) Analyses for *Ich liebe Pinguine*

- (a) –verb-second, heads precede their complements:

Underlying order:	Ich	liebe	Pinguine
	Subj	Verb	Obj
Observable order:	Ich	liebe	Pinguine
	Subj	Verb	Obj

- (b) +verb-second, heads precede their complements:

Underlying order:	Ich	liebe	Pinguine		
	Subj	Verb	Obj		
Observable order:	Ich	t_{Ich}	t_{liebe}	Pinguine	
	Subj	Verb	t_{Subj}	t_{Verb}	Obj

- (c) +verb-second, heads follow their complements:

Underlying order:	Ich	Pinguine	liebe		
	Subj	Obj	Verb		
Observable order:	Ich	t_{Ich}	Pinguine	t_{liebe}	
	Subj	Verb	t_{Subj}	Obj	t_{Verb}

We again find that one parameter seems be masking the effects of the other. If the child knew the language was –verb-second, then the value of the head directionality parameter would be clear for this data point (heads precede their complements (11a)); conversely, if the child knew heads followed their complements, then the value of the verb-second parameter would be clear for this data point (the language has verb-second movement (11c)). Since both parameters are undetermined, however, extracting the right values for them from this data point is not straightforward.

As we noted in the introduction, the main trouble is that children don't see the process that produces the data – they only see the data themselves. Children must infer the most likely parameter values from the observable data, and the observable data may be (and often are) ambiguous as to which parameter values were used to generate them. So, even if children know the set of parameters and the potential values these parameters might take, acquisition still has some obstacles left.

This leaves open a number of questions about the parameter-setting process, which we explore in the remainder of this section:

- Are parameter settings represented as deterministic or probabilistic? More specifically, at any one stage, does the child maintain a single hypothesis about which way the parameter is set (like a light switch)?

Alternatively, does the child maintain multiple hypotheses along with a confidence value (more like a dimmer switch between extremes)?

- Do children make inferences from a large quantity of data all at once, or do they make inferences incrementally as they encounter the data?
- Do children attempt to learn parameters individually, or are parameters learned only as part of an entire grammar that succeeds or fails at analyzing the observable data? If parameters are learned individually (rather than as an ensemble), what causes a change in a child's hypothesis for a parameter value?
- Which data are used to set parameters?
- Do children need to follow particular parameter-setting orders?
- What happens when children do not encounter sufficient informative data?

9.3.1 Light switches or light dimmers?

Suppose a child is attempting to learn the value for a particular parameter, such as extrametricality in the metrical phonology system. Two main options exist: The language either has extrametricality or it does not. We could imagine at least two ways children might explore the different hypotheses. Perhaps they choose a parameter value to start off with at random, maintain that value as their working hypothesis, and only discard it for the alternative hypothesis if it fails to analyze the input data successfully (Fodor and Sakas 2004; Gibson and Wexler 1994; Niyogi and Berwick 1996; Sakas 2003; Sakas and Fodor 2001; Sakas and Nishimoto 2002). Parameter-setting behaves as a light switch that can only choose one option at a time. One issue with this approach is that it predicts that the child's learning behavior should exhibit abrupt changes with each change in parameter value – and once the correct parameter value is acquired, the child should use it all the time.³ We would never expect children to go back to using the incorrect hypothesis if they are only maintaining the single correct hypothesis. This does not seem to map well to what we know of children's behavior – the trajectory of acquisition appears to be more gradual, with children intermixing correct and incorrect linguistic behavior for a period of time before they converge on the correct hypothesis alone (see, for example, Hochberg 1988).⁴

An alternative is that perhaps children place some belief in both hypotheses to begin with, and gradually alter their belief in which hypothesis is correct based on the ability of each hypothesis to account for the input data (Clark 1992; Legate and Yang 2007; Pearl 2009, 2011; Pearl and Lidz 2009; Pearl and Weinberg 2007; Yang 2002, 2004). Parameter-setting here behaves more like a light dimmer, with the “switch” resting at a position between two options and its relative position indicating which option is more likely. If this is more similar to how children set their parameters, children's learning behavior can easily intermix different parametric

options. The different options are simply accessed probabilistically, depending on the belief the child has in each option. For example, if a child places 60% probability in the language being extrametrical and 40% probability in the language not being extrametrical, this child might choose to produce linguistic data that use extrametricality 60% of the time and linguistic data that do not use extrametricality 40% of the time. As children are exposed to more input, they alter their beliefs in the different options until the correct option is most likely (a probability near 100%) and the incorrect option is not at all likely (a probability near 0%). Notably, this takes some time, during which the child will probabilistically access both hypotheses and so use both parameter values. This account thus seems to accord better with observable child language acquisition behavior.

9.3.2 Batch learning or incremental learning?

When children are updating their hypothesis about the correct parameter value, do they amass a quantity of data points and then update based on the inferences that come from those data as a group (batch learning) or do they update as the data come in (incremental learning)? The question of batch vs. incremental learning is really dealing with a larger issue in the language acquisition literature, specifically this: What question are we trying to answer about language acquisition?

The question of *learnability* is asking what is, in principle, possible to learn given the available data. This concerns what information may be useful given the available data and the knowledge that must be learned, but typically without considering constraints that humans have when learning the knowledge from the data. Models answering this question are called “ideal learner,” “rational,” or “computational-level” models (Foraker *et al.* 2009; Goldwater, Griffiths, and Johnson 2009; Hsu and Chater 2010; Perfors, Tenenbaum, and Regier 2006, 2011). Because these models assume no memory constraints and no processing constraints, they use batch learning (usually over an entire corpus’s worth of data) and may use inference procedures that are unlikely to be used by humans.

The question of *acquirability* is asking what is possible to learn given the available data, and the constraints children have when they use that available data to learn. Models answering this question are sometimes called “algorithmic-level” models (Fodor and Sakas 2004; Gibson and Wexler 1994; Legate and Yang 2007; Niyogi and Berwick 1996; Pearl 2009, 2011; Pearl, Goldwater, and Steyvers 2010, 2011; Pearl and Lidz 2009; Pearl and Mis 2011; Pearl and Weinberg 2007; Sakas 2003; Sakas and Fodor 2001; Sakas and Nishimoto 2002; Yang 2002, 2004) referring to Marr’s second level of representation for questions of information processing (Marr 1982). Because these models assume children have memory constraints, they often make the simplifying assumption that children

process data as they are encountered (incremental learning), rather than storing detailed data for analysis later on. These models also typically assume children have processing constraints and try to use inference algorithms or inference algorithm approximations that are more likely to be feasible given these processing constraints.

If we are interested in explaining children's observable behavior, computational-level models can help us determine if the acquisition problem is solvable in principle when framed a particular way (e.g. Can children learn the correct setting of the extrametricality parameter, given these data?). If the acquisition problem is not solvable even in principle, this suggests that something is amiss in the formulation of the acquisition problem – perhaps the knowledge to be attained is not what we think it is, or the child has additional restrictions on potential hypotheses that we are not considering (see Pearl 2011 for more discussion of this point for metrical phonology parameters). Once we determine that an acquisition problem is solvable in principle, we can then use algorithmic-level models to determine if it is solvable by constrained learners such as children. This allows us to explore what is required to make knowledge that is learnable in principle acquirable in practice.

9.3.3 Parameters: Ensembles or individuals?

Several researchers have viewed the acquisition problem as a search problem within the hypothesis space of possible grammars (Clark 1992; Gibson and Wexler 1994; Niyogi and Berwick 1996; Pearl 2009, 2011; Yang 2002) – which of these grammars are able to account for the data in the input, and which aren't? A learning algorithm like this is error-driven, with grammars treated as atomic units that either perform well on the data or perform poorly. While children recognize that grammars are comprised of parameters with particular values, and these values can be changed, the performance of the grammar as a whole is what counts. In essence, the child rewards a successful grammar and punishes an unsuccessful one.

Computational-modeling researchers have instantiated this idea in various ways. Clark (1992) imagines the child scoring grammars based on their relative “fitness”; grammars able to account for more data are viewed as more fit and rewarded, while grammars able to account for less data are less fit and punished. The child rewards and punishes grammars as a whole, rather than keeping track of the fitness of the parameters within the grammars. Gibson and Wexler (1994) and Niyogi and Berwick (1996) view the child as rewarding or punishing individual parameters within grammars, based on the grammars' ability to account for the data. If the current grammar cannot account for the current data point, the child may choose to consider a new grammar that differs from the old one by a single parameter value as long as this new grammar accounts for the data point in question. In this way, parameters that were not the problem do not get punished.

Yang (2002) and Pearl (2009, 2011) extend this idea by allowing parameter values to be rewarded or punished probabilistically, based on the performance of grammars using these values. Rather than getting rid of a parameter value if a new grammar without it can account for the data, the child simply lessens her confidence in that parameter by lessening her confidence in *all* the parameters involved in the unsuccessful grammar. Some parameter values are certainly unfairly punished this way, but the idea is that those unfairly punished will be part of successful grammars later on and be rewarded, while the real culprits will only be part of unsuccessful grammars, and so continually be punished. The upshot is that the child does not necessarily know which parameter was the culprit for any given failure of a grammar to account for data, but can still track how confident she is that any given parameter value is a good one for the language by the performance of grammars as a whole on the data she encounters. This can skirt the issue of blame assignment (sometimes known as the credit problem (Dresher 1999)) that arises for ambiguous data (see examples (5) and (11) above). Specifically, the child does not need to pinpoint which parameter is at fault in order to get information out of an ambiguous data point. She simply observes if the current parameter values under consideration are able to collectively analyze the data point, and updates her confidence in those parameter values accordingly.

This last part about tracking individual parameters (even indirectly) is quite useful because it helps reduce the hypothesis space for acquisition. To illustrate this, suppose a child knows there are 25 binary parameters. If the child views the problem as a search among grammars made up of these 25 parameters, there are 2^{25} (33,554,432) hypotheses. If instead the child views the problem as setting 25 parameters in one of two ways, the hypothesis space is more like $25 \times 2 = 50$. That is, there are 25 choices with 2 options each. Of course, these 25 choices can produce 2^{25} different grammars, but that is not what the child is explicitly tracking: She only cares which way each parameter is set. So, if the child focuses instead on what the choice is for each parameter, rather than on how grammars as a whole perform, the hypothesis space is much smaller.

9.3.4 Getting the most out of your data?

Several researchers have considered how children might explicitly track the values for specific parameters. Based on computational modeling results like Gibson and Wexler (1994) and Niyogi and Berwick (1996), researchers such as Fodor (1998), Dresher (1999), and Lightfoot (1999) propose that children are specifically keying into parts of the observable data that are linked to a specific parameter value. In particular, children are waiting for data that are unambiguous with respect to a given parameter. This requires that children already know what parts of the observable

data are important for a given parameter, or can derive it for each parameter value in some way.⁵ Still, once they have this knowledge, acquisition is much simpler. The main point is that the process of acquisition is inherently different for a child interested only in identifying the correct parameter values for her language: She is scouring the data for the relevant pieces, rather than worrying about which grammar can account for the most observed data.

Of course, this doesn't mean children would ignore the available data. Rather, they scour the data for the unambiguous "cues" (Dresher 1999; Lightfoot 1999) or designated structures (Fodor 1998), and reward parameter values that are best able to account for the distribution of these specific data. The difference is subtle, but important. A child interested in matching the data with an entire grammar will, naturally, choose the parameter values that belong to the grammar that fits the data the best. A child interested in tracking cues within the data for different parameter values may end up choosing a grammar comprised of parameter values that individually match the data the best, but which, when combined into a grammar, are less compatible with the data.

Let's consider an example from metrical phonology that demonstrates this from Pearl (2008). The English metrical phonology system is thought to have the rightmost syllable of the word be extrametrical (+extrametrical) (Dresher 1999). If a child tracks which grammar as a whole is compatible with the most English child-directed speech data, the answer is a grammar that has no extrametricality (−extrametricality). However, if a child is searching for unambiguous data regarding extrametricality, the cues for +extrametricality are more frequent than the cues for −extrametricality.⁶ In this way, a child looking at overall grammar "fitness" can end up with a different answer than a child looking for unambiguous cues to parameter values in the data – and in this case, we think it is the child using unambiguous data who ends up with the right answer for English.

9.3.5 Who goes first?

A factor that appears when the child is considering parameters individually this way is the "learning path" (Dresher 1999; Lightfoot 1989, 1999; Baker 2005; Pearl 2008), which is the order in which the child learns the values of the parameters. If the child is comparing grammars as a whole, this does not matter – parameters function solely as part of a grammar, and the grammar either succeeds or doesn't. However, when a child is learning parameters values individually, there are a variety of orders that parameter values might be learned in. Because parameters interact, learning the value of one parameter may influence the child's interpretation of the data to come. Let's consider an example from before, (5) repeated (more briefly) here as (12).

- (12) Analyses for “giRAFFE”
- (a) +extrametrical (leftmost syllable), feet-headed-left:
Analysis: gi (RAFFE)
 - (b) +extrametrical (leftmost syllable), feet-headed-right:
Analysis: gi (RAFFE)
 - (c) –extrametrical, feet-headed-right:
Analysis: (gi RAFFE)

Suppose the child did not know whether the metrical feet in the language were headed on the left or headed on the right. Given that lack of knowledge, this data point is ambiguous for extrametricality, since there are analyses for both +extrametricality and –extrametricality. A child learning from unambiguous data therefore learns nothing from this data point. However, suppose that same child encountered this data point after learning that the language had metrical feet headed on the left. Then, the only analysis remaining is (12a), with +extrametricality. This same data point is now perceived as unambiguous for the extrametricality parameter, where before it was not.

The learning path turns out to be crucial for learning the English metrical phonology systems using parameters. Pearl (2008) discovered that a child tracking unambiguous data probabilities will decide on the correct parameter values for English, but only if the parameters are learned in particular orders. This has a direct implication for acquisition – not only do English children need to know the parameters and values these parameters can take, but they also need to know what order to learn parameters in. Fortunately, the learning path knowledge sometimes turns out to be derivable from properties like the distribution of data in the input (see Pearl 2007 for discussion). And perhaps this is a small price to pay for being able to reduce the hypothesis space by learning about parameters individually.

One reason why unambiguous data are so useful is that they provide maximally informative data. Since unambiguous data, by definition, are compatible with only one value for a parameter, they strongly signal that this parameter value is the right one. This leads to another good reason to consolidate current ideas about parameters, so that more structural properties about the language are connected. If structural properties are linked through a parameter, unambiguous data for one structural property serve as unambiguous data for all the rest of the structural properties linked by that parameter. For example, suppose our current conception of language parameters results in the following situation: Parameter P1 has some unambiguous data while parameter P2 does not. This is problematic for an unambiguous learner. However, suppose that parameters P1 and P2 are really related, and so actually are both instances of parameter P*. Since unambiguous data exist for P1, and P1 tells us about P*, we then have unambiguous data for P2 (which is also an instance of P*). In general,

consolidating parameters seems to be a good idea, as unambiguous data are not all that common. Pearl (2008) shows that the most frequent unambiguous data for parameters in the metrical phonology domain investigated made up less than 5% of the child's available input. In fact, one of the criticisms of the unambiguous data approach was that unambiguous data may not really exist for all parameters. At that point, a child learning only from unambiguous data would be in big trouble.

One solution would be to back off on the claim that children only learn from unambiguous data, under the assumption that unambiguous data really are non-existent (or at least very rare) for some parameters. While children probably should not learn from the data if said data are completely ambiguous, there may be many data points that are only somewhat ambiguous, rather than completely ambiguous. For example, consider again our metrical phonology analyses for “giRAFFE,” repeated here as (13):

- (13) Analyses for “giRAFFE”
- (a) +extrametrical (leftmost syllable), feet-headed-left:
Analysis: gi (RAFFE)
 - (b) +extrametrical (leftmost syllable), feet-headed-right:
Analysis: gi (RAFFE)
 - (c) –extrametrical, feet-headed-right:
Analysis: gi (RAFFE)

A child considering these three analyses does not unambiguously know which value is correct for extrametricality and feet headedness. However, she still might be able to place some confidence in one value over the other based on the number of analyses that include one value vs. the other. In this case, since two of the three analyses involve +extrametricality, she might believe that +extrametricality is more likely than –extrametricality; similarly, since two of three analyses involve metrical feet headed on the right, she might believe right-headed metrical feet are more likely than left-headed metrical feet for the language. So, while an unambiguous learner would ignore this data point as uninformative, a child leveraging whatever information is available might find this data point partially informative. This means that even if no unambiguous data exist for a given parameter, some informative data may still exist. A child willing to learn from any informative data is not in nearly so tough a spot as a child waiting for unambiguous data (cf. Fodor and Sakas 2004 for a striking example of this in syntactic acquisition). Recent computational modeling research has shown that this is a powerful idea for learning structural properties such as the fact that language has hierarchical structure (Perfors, Tenenbaum, and Regier 2006, 2011) and the structure that the English referential element *one* has (Foraker *et al.* 2009; Pearl and Lidz 2009; Pearl and Mis 2011; Regier and Gahl 2004).

9.3.6 Insufficient data: Does not compute?

Still, there may be some cases where a language provides no unambiguous data and also has very little informative data of any kind. This can happen when a parameter is not used in a particular language. While all parameters may be available in all human minds, some may only come into play when certain other structural properties of the language hold. For example, Baker (2001) describes a parameter known as “adjective neutralization” which determines whether adjectives are treated like verbs or like nouns in the language. Importantly, this is only relevant for polysynthetic languages like Mohawk and Walpiri. Non-polysynthetic languages, like English, do not use this parameter. So, perhaps unsurprisingly, English does not include informative data that show adjective neutralization. Another example of a parameter with very little informative data in a given language is verb-raising in Korean.

Verb-raising concerns the position of the tensed verb with respect to adverbs/negative elements (Baker 2001, Cook and Newson 1996, Yang 2004, among others). When a clause has both tense (e.g., +present) and an adverb or negative element (e.g., *often*, *not*), languages vary on the position of the verb. In some languages, such as French, the verb “raises” or is “attracted” to the structural position of the tense, so that it ends up as a tensed verb preceding adverbs or negative elements (14a). In other languages, such as English, this does not happen, and the verb appears to follow adverbs or negative elements (14b). However, in a head-final language like Korean, very few data points distinguish whether Korean is +verb-raising or –verb-raising (14c), since the verb occurs in the same linear position independent of its structural height (see Han, Lidz, and Musolino (2007)).

(14) Verb-raising variation

(a) +verb-raising (French)

Underlying:	Jean	[+present]	souvent/pas	voir	Marie
	Jean		often /not	see-inf	Marie
Observable:	Jean	voit	souvent/pas		Marie
	Jean	see-present	often /not		Marie
“Jean often sees Marie”/ “Jean does not see Marie.”					

(b) –verb-raising (English)

Underlying:	John	[+present]	often	see	Mary.
Observable:	John		often	sees	Mary.
Underlying:	John	[+present]	not	see	Mary.
Observable:	John	does	not	see	Mary.

(c) ?verb-raising (Korean)

(i) Underlying:	Yuri	cacwu	Toli-lil	ttyali	[-n-ta]
	Yuri	often	Toli-acc	hit-inf	[pres-decl]

- (ii) Observable Yuri cacwu Toli-lul ttayli-n-ta
 Yuri often *Toli*-acc hit-pres-decl
 "Yuri often hits Toli."
- (iii) Analysis 1:
 +verb-raising Yuri cacwu Toli-lul t_{ttayli} ttayli-n-ta
 Yuri often *Toli*-acc t_{hit} hit-pres-decl
 "Yuri often hits Toli."
- (iv) Analysis 2:
 -verb-raising Yuri cacwu Toli-lul ttayli-n-ta t_{n-ta}
 Yuri often *Toli*-acc hit-pres-decl $t_{pres-decl}$
 "Yuri often hits Toli."

Except in very rare constructions, the effects of verb-raising are masked in Korean due to other structural properties of the language. Because of this, children and most adults may not encounter enough data to decide whether their language has verb-raising or not. More specifically, almost all data are compatible with both raising the verb and not raising the verb. What does this mean for Korean speakers? Do they simply pick one value in the absence of any informative data? Or do they leave it as undecided, so that if pressed to show knowledge of the parameter, they would flip flop between the two options with some probability? In the case of Korean, Han, Lidz, and Musolino (2007) found that there seemed to be two groups of adult speakers: those that raise their verb and those that do not. Very few speakers were willing to allow both options with some probability. So, this seems to indicate that Korean speakers had selected a parameter value despite having very little informative data. Still, perhaps the few data points they encountered in their lifetimes were enough to influence their choice – some statistical learning techniques are able to capitalize on very small amounts of data in the input (see Foraker *et al.* 2009 for an example of this). However, when Han, Lidz, and Musolino examined children's knowledge, children too seemed to split into a group that raises their verbs and a group that does not. It is possible that these children also may have all encountered just enough informative data, but the likelihood is much less. Instead, it may be that children really were choosing one parameter value in the absence of informative data. If so, this tells us something very interesting about how the human mind functions during language acquisition. Specifically, this suggests that the human mind prefers to make decisions and set a parameter – even for seemingly irrelevant parameters – rather than allow optionality to exist at this structural level of representation. That is, even if a parameter does not appear to be used in a language, the human mind prefers to set it than leave it unset.

An increasingly common way to explore how exactly the human mind learns with parameters is to use computational modeling. Computational modeling gives us very precise control over the language acquisition

process, including what hypotheses the child entertains, what data are considered relevant, and how the child changes belief in different hypotheses based on the data. For learning with parameters, we can control which parameters are considered, and which values of those parameters are considered. We can also control if children learn only from unambiguous data, or consider other kinds of data informative. In addition, we can control if data are immediately influential, or if a child waits until she has some confidence that the data really do indicate a particular parameter value is correct. A variety of data are available from experimental studies that show us what a child's input data really look like, and we can ground our models with this empirical data to make them as much like a child's language acquisition process as possible. When we have a model like this, we can embody ideas like "learning only from unambiguous data" and "waiting until the data are really conclusive." If some of these ideas are required for the model to behave like children do, this tells us that these ideas may do a good job of describing how the human mind acquires language. For instance, if we find that, when given Korean data, one model ends up setting the verb-raising parameter while another model prefers to leave it unset, this suggests that the first model is a more accurate representation of the language acquisition process.

This last point is relevant for computational models that use statistical analyses such as Bayesian inference when compared to models that use update algorithms such as linear reward-penalty (e.g., see work by Pearl (Pearl 2009, 2011) and Yang (Legate and Yang 2007; Yang 2002, 2004)). Suppose a learner begins with two hypotheses, and gives each a probability of 0.5. A Bayesian inference model that receives no input will not update the probabilities since both hypotheses are still equally likely. In contrast, a learner using a variational learning update process (see discussion of this update process in [Section 9.4](#) below) will end up converging to one hypothesis or the other. This is due to the random fluctuations that are part of this style of update, with small random preferences being magnified over time. Still, it should be noted that very little data are very different than no data for a Bayesian learner, while this difference is not so dramatic for a linear reward-penalty learner. Thus, to decide between these two kinds of updating processes, it may matter very much that there is very little data that children could notice, as opposed to there actually being no informative data in the input at all.

9.4 Specific examples of learning with parameters

Let's now look at a few examples of computational models that investigate language acquisition using parameters. We will look within the domains of syntactic acquisition and metrical phonology, and find that the same considerations seem to arise about what the relevant data are and how the

learning algorithm should work, even if the hypothesis space (consisting of particular parameters) is agreed upon. In addition, all these models share something besides using parameters to define the hypothesis space: They all seek to combine insights from statistical learning into a cognitively plausible model.

For syntactic acquisition, work by Yang (2002, 2004) has examined the problem of learning the correct parameter values from the data children encounter, such as whether or not the language has verb-second movement and movement of wh-words to the front of the clause in questions. All of Yang's models use the frequencies of data found in child-directed speech samples. As mentioned in the previous section, Yang's models, often called "variational learners," do not specifically look for highly informative data; instead, they consider all data relevant for learning. The particular algorithm used by Yang incorporates a model from mathematical psychology called the linear reward-penalty scheme (Bush and Mosteller 1951). The algorithm recognizes that each parameter has two competing values (e.g., ±wh-fronting). Initially, the model associates a probability of 0.5 with each, representing its belief that each parameter value is equally likely. This probability is then altered, based on the data encountered.

For each data point, the variational learner generates a grammar based on the current probabilities associated with all parameter values. For instance, when generating the value for the wh-movement parameter, the model uses the probabilities associated with +wh-movement and -wh-movement. Suppose they are 0.40 and 0.60 respectively; then, the model will use the +wh-movement value with 40% probability and the -wh-movement value with 60% probability. This value selection process continues until all parameter values have been selected and a complete grammar is formed. Using the probabilistically generated grammar, the model then generates a structure for the data point (e.g., generating the structure for a question and the word order associated with that structure). If the generated word order matches the observed word order for the data point, all parameter values that were selected to participate in the grammar are rewarded; if the generated order does not match, all participating parameter values are punished. Notably, this model does not attempt to assign credit or blame to a particular parameter value within the grammar. Instead, all participating values are rewarded or punished together. The model then moves on to the next data point.

Updating (whether rewarding or punishing) is specified by the linear reward-penalty scheme. The update equation involves a parameter γ that determines how liberal the model is. The larger γ is, the more probability the model shifts for a single data point.

(15) Updating using the linear reward-penalty scheme

$$p_v = \text{previous probability of parameter value}$$

$$p_o = \text{previous probability of opposing parameter value}$$

- (a) generated word order matches observed word order (reward)

$$p_{vnew} = p_v + \gamma(1-p_v)$$

$$p_{onew} = (1-\gamma)p_o$$

- (b) generated word order does not match observed word order (punish)

$$p_{vnew} = (1-\gamma)p_v$$

$$p_{onew} = \gamma + (1-\gamma)p_o$$

As an example, suppose we consider the probabilities of +wh-movement and –wh-movement for the wh-movement parameter. Initially, they are both 0.5. For the first data point, suppose –wh-movement is chosen to be part of the grammar and that grammar fails to generate the observed word order (e.g., the observed data point is “Who is that?” and the generated word order is “That is who?”). The –wh-movement value (and all other participating values) are punished. Suppose γ is 0.01. The new value of –wh-movement would be $(1-0.01)*0.5 = 0.495$ and the new value of +wh-movement would be $0.01 + (1-0.01)*0.5 = 0.505$.

If the model rewards or punishes parameter values every time a data point is encountered, this is known as the Naïve Parameter Learner (Yang 2002). The idea, discussed in the previous section, is that the incorrect parameter values will be punished more often than the correct parameter values since the correct parameter values are the ones that should be compatible with the language data. Yang also advocates a variant of this model called “batch learning” that is more conservative about changing its beliefs – instead of updating after every data point, the model waits to update a parameter value until a string of failures or successes has been observed for that value. It does this by keeping count of how many successful and unsuccessful parses a parameter value has been involved in. So perhaps a more transparent name for it is a “counting learner.” The benefit of a counting learner is that it smoothes the acquisition trajectory when parameters that interact are involved. Instead of jumping to conclusions after encountering ambiguous data, the counting learner waits for a while to see if it’s really true that a particular parameter value works or doesn’t work.

Let’s look at an example of this. Suppose we have a counting learner that has a count size of 5. If the count for a parameter value reaches 5, the parameter value is rewarded. If the count reaches –5, the parameter value is punished. Every time the parameter value is part of a grammar that generates a structure that matches the observed data point, the counter is increased; conversely, every time the parameter value is part of a grammar that generates a structure that mismatches, the counter is decreased. After the reward or punishment, the parameter value’s counter is reset to 0. Suppose now we again consider the probabilities of the values for the wh-movement parameter. Initially, both + and –wh-movement have a probability of 0.5, and their counters are both 0.

For the first data point, suppose +wh-movement is chosen to be part of the grammar and that grammar fails to generate the observed word order. The counter for +wh-movement is now -1. For the next three data points, suppose -wh-movement is chosen for the grammar and those grammars succeed at generating the observed word order. The counter for -wh-movement is +3 and the counter for +wh-movement is -1. Suppose the next two data points use +wh-movement and those grammars succeed: -wh-movement's counter is still +3, but +wh-movement's counter is now +1. Suppose then that the next six data points use +wh-movement and those grammars fail: -wh-movement's counter is still +3, but +wh-movement's counter is now -5, which is the count limit. The +wh-movement value is then punished using the appropriate update equation for the model. If the model uses a γ of 0.01, the new probability of +wh-movement is 0.495 and the new probability of -wh-movement is 0.505. The counter for +wh-movement is then reset to 0.

The conservativity of this learning model can be seen from the previous example – instead of updating for each of the twelve individual data points (punishing +wh-movement once, rewarding -wh-movement three times, rewarding +wh-movement two times, and then punishing +wh-movement six times), the model only punishes +wh-movement once. Importantly, this is only after the +wh-movement value has been involved in a string of failures, and so is more likely to really be failing.

Using this kind of learner, Yang discovered that a strong gauge of acquisition success was the quantity of unambiguous data for each parameter value in the input, even though his learners weren't specifically looking for just unambiguous data. The intuition is that ambiguous data for a parameter can be parsed by either parameter value, and so neither value is guaranteed to be punished (although either *might* be if some other parameter interacts and causes a mismatch with the data point). However, for unambiguous data, one parameter value is guaranteed to be punished every single time – by its very nature, an unambiguous data point is compatible with only one parameter value, no matter what other parameter are involved. Taking the idea that unambiguous data for parameter value is a key notion, Yang (2004) investigated the age of acquisition for a number of parameter values and the amount of unambiguous data available in child-directed speech for these parameter values. He found a strong correlation, where the more frequent unambiguous data were, the earlier a parameter value seemed to be acquired by children (16). This suggests that unambiguous data certainly are quite important for learning with parameters, even though they may not be the only data children use.

- (16) Correlations between age of acquisition and unambiguous data frequency⁷

- (a) wh-fronting in English questions (+wh-movement)

Example unambiguous data: “Who did you see?”
 Unambiguous data frequency: 25% of input
 Age of acquisition: As early as children can be tested
- (b) verb-raising in French (+verb-raising)

Example unambiguous data type: Jean voit souvent Marie.
 Jean sees often Marie
 “Jean often sees Marie.”

Unambiguous data frequency: 7% of input
 Age of acquisition: 1 year, 8 months
- (c) obligatory subject in English (-pro-drop)

Example unambiguous data: “There’s a penguin on the ice.”
 Unambiguous data frequency: 1.2% of input
 Age of acquisition: 3 years
- (d) verb-second in German (+verb-second)

Example unambiguous data: Pinguine liebe ich.
 Penguins like I.
 “I like penguins.”

Unambiguous data frequency: 1.2% of input
 Age of acquisition: 3 years
- (e) no medial-wh in English (-medial-wh)

Example unambiguous data type: “Who do you think is on the ice?”
 Unambiguous data frequency: 0.2% of the input
 Age of acquisition: after 4 years

A complementary line of computational work by Sakas and Fodor (Fodor 1998b; Fodor and Sakas 2004; Sakas 2003; Sakas and Fodor 2001; Sakas and Nishimoto 2002) investigates other learning strategies a child might have for acquisition, with particular concern for how much time it would take for a child to converge on the right grammar. They quantified the general notion of time as how many data points a child would need to observe. To provide their models with a realistic acquisition problem, they required their models to set 13 binary parameters that interact, for a hypothesis space of 3072 possible grammars. The data the models learned from represented the sentence types that could occur in the language, such as “Subject Verb” (a data point fitting this description in English would be *I laughed*). All of the strategies they investigated were also based on the idea previously discussed that the child is attempting to generate a structure that matches the observable word order. A summary of some of the different strategies is shown in (17) below. The learning strategies are divided into those that only learn when there is a failure to match the observed data (error-driven models (17a–c)), and those that are sensitive to whether the current data point is ambiguous for which grammar generated it (structural triggers models (17d–f)).

(17) Acquisition strategies

- (a) Error-Driven Blind Guess: Only change the current guess for the grammar if that grammar cannot generate the observed word order. At this point, make a new guess randomly for what the entire grammar is.
- (b) Trigger Learning Algorithm: Only change the current guess for the grammar if that grammar cannot generate the observed word order. At this point, optionally change some parameter values.
- (c) Error-Driven Variational Learner: Only change the current guess for the grammar if that grammar cannot generate the observed word order. At this point, probabilistically generate a new grammar based on probabilities associated with each parameter value. If the new grammar can generate the observed word order, reward all participating parameter values; otherwise, punish all participating parameter values.
- (d) Strong Structural Triggers Learner: The learner is aware of all possible structures capable of generating the observed word order. Only parameter values required by all these structures are selected as the correct one for the language, as the data point is unambiguous for these values.
- (e) Waiting Structural Triggers Learner: The learner is aware if more than one structure is capable of generating the observed word order, and where exactly the ambiguities in structure are. It only adopts parameter values that correspond to parts of the structure that occur before any ambiguities, and are therefore unambiguous.
- (f) Guessing Structural Triggers Learner: The learner is aware if more than one structure is capable of generating the observed word order, and where exactly the ambiguities in structure are. For the ambiguous parts, it chooses one parameter value, based on some structural heuristic.

Sakas and Fodor find that error-driven learners generally fare much less well than learners that care about the ambiguity of the data. Specifically, the error-driven learners (17a-c) take longer to converge on the correct grammar for the language. However, among the learners that are sensitive to the ambiguity in the data, it turns out that there is a trade-off between learners who only learn from unambiguous data and learners that are willing to make a guess even when the data are ambiguous. While the unambiguous learners (17d-e) will sometimes converge very quickly if the data distribution is favorable (that is, contains unambiguous data), they will take a very long time to converge if the data distribution is unfavorable. The less conservative learner (17f) may take longer on average to converge, but this learner type doesn't have the extremes in variation because it does not depend on unambiguous data being available. What

this tells us is that unambiguous data may be very useful, but if a child is attempting to generate the data she encounters rather than identify cues for parameter values, she may need to rely on somewhat ambiguous data as well.

Another line of computational work in the metrical phonology domain by Pearl (2007, 2008, 2009, 2011) examines an additional complication for the acquisition problem. In the models previously discussed, there has been very little noise in the input data – that is, there are very few misleading data points for the correct grammar of the language (which might occur as speaker errors, or as part of different dialects). However, in metrical phonology, this is not always the case. It may very well be that there is a set of data that are exceptional, but still part of the language. For example, Daelemans *et al.* (1994) note that 20% of the Dutch data they consider are irregular according to a generally accepted metrical analysis and so must be dealt with in terms of idiosyncratic lexical marking. When Pearl examined English metrical phonology data, she discovered that at least 27% of the English child-directed speech data were irregular with respect to the metrical analysis for English derived from Dresher (1999), Halle and Vergnaud (1987), and Halle and Idsardi (1995), again because of lexical exceptions. This makes acquisition a bit tougher than before – not only are the data often ambiguous, but some of them may be inconsistent with the target grammar because of lexically listed exceptions.

Pearl (2009, 2011) examined how well learners that probabilistically learn from all data would do on this data set, since this seemed another realistic example of the data children encounter. The learning models examined were the variational learner from Yang (2002) and an incremental Bayesian model, both non-counting and counting versions. The variational model uses the update equations described in (15), while the incremental Bayesian model uses the update equations shown in (18).⁸ If a parameter value participates in a grammar that generates a stress contour that matches the observed stress contour, the number of successes for that parameter value is incremented by 1. If a parameter value participates in a grammar that does not, the number of successes is left alone. Either way, the total data seen is incremented by 1 if the parameter value was part of the grammar used to generate the stress contour. The probabilities for opposing parameter values are then calculated and all probabilities are normalized so they sum to 1. So, for each parameter value, the model tracks (a) the current probability, (b) the number of matching stress contours that parameter value has been involved in generating, and (c) the total number of stress contours that parameter value has been involved in generating.

(18) Update equations for incremental Bayesian learning model

p_v = previous probability of parameter value

p_o = previous probability of opposing parameter value

$$p_{vnew} = \frac{1.5 + \text{successes}}{3 + \text{total data seen}}$$

$$p_{vnew, \text{ normalized}} = \frac{p_{vnew}}{p_{vnew} + p_o}$$

$$p_{onew, \text{ normalized}} = \frac{p_o}{p_{vnew} + p_o}$$

As an example, suppose we look at the extrametricality parameter. Initially, the probabilities for both + and -extrametricality are 0.5. For the first data point, suppose +extrametricality is chosen to be part of the grammar and that grammar fails to generate the observed stress contour. The +extrametricality value (and all other participating values) are punished. The non-normalized probability for the +extrametricality value is $(1.5+0)/(3+1) = 0.375$. The non-normalized probability for the -extrametricality value has not changed from 0.5 since it was not used for this data point. The normalized probability of +extrametricality is then $0.375/(0.375 + 0.5) = 0.429$ while the normalized probability of -extrametricality is then 0.571.

Pearl found that none of these learners succeeded with any kind of reliability, perhaps a somewhat disheartening discovery. The trouble turned out to be that the English grammar as a whole was compatible with less data than other grammars available in the hypothesis space, even though the individual parameter values may have been compatible with more data. This is again the problem of tracking overall grammar “fitness” with the data compared to identifying the parameter values involved in the system, based on cues from the data. If we believe children do end up choosing the parameter values in the English grammar, they must have some way of viewing the acquisition problem such that these parameter values are the most optimal ones for explaining the data that are relevant.

One idea is that children are sensitive to the unambiguous data, and the ability of the parameter values to account for the unambiguous data are what matters – not the overall fitness with the entire data set. Pearl (2008) found that children would choose the English grammar (and more particularly, the parameter values that make up the English grammar) if they specifically track the unambiguous data distributions. This was far more heartening news for acquisition for two reasons. First, it showed that unambiguous data do indeed exist in a very realistic acquisition scenario, which is always a concern if we believe children rely on them. Second, it showed that they appeared in the correct distributions to lead a child to the right parameter values. The reason why learning from unambiguous data worked is because the unambiguous data favor the English parameter values when the parameters are acquired in particular orders. So, if the parameters are acquired in one of those orders, the English parameter values are the fittest for the unambiguous data. In that case, a probabilistic

learning algorithm that prefers the optimal values (as most do) will converge on the English grammar. Interestingly, additional simulations suggested that it is not the ordering alone that causes the English parameter values to be optimal. When parameters were set in similar orders by the models that learn from all data (both ambiguous and unambiguous), there was no reliable convergence on the English grammar. Among the learners that do converge, there seemed to be no common learning path. This suggested that the culprit was the ambiguous data. Learning from these data (though granted in a rather naïve way) will mislead an English learner. These simulations also have led to a testable prediction about children’s acquisition trajectory: If children are using parameters and relying on unambiguous data, they must learn parameter values in certain orders. This prediction remains to be tested, but the key point is that it has given us something to look for in children’s behavior that we didn’t know to look for before.

These three sets of modeling studies have all investigated the acquisition of language using parameters in different ways. Though their general strategies of investigation differed somewhat, they all demonstrated both how useful unambiguous data can be and also some of its potential pitfalls for a parameter-setting learner. We have additionally seen how computational modeling not only assesses different learning strategies but also how it can generate testable predictions about acquisition.

9.5 Conclusion

In this chapter, we have discussed some ideas for what linguistic parameters are intended to be, highlighting their similarity to statistical parameters and describing how and why they would be beneficial for language acquisition. We have tried to emphasize that using parameters to characterize the space of possible languages does not in itself provide a model of learning. In addition, a learning theory needs a mechanism for drawing inferences from observed data to particular parameter values. A number of alternative approaches to parameter-setting highlighted the learning choices that remain, such as why learning by using individual parameters might be better than learning parametrically defined grammars that are treated as atomic units, and whether and how learners rely on strictly unambiguous data. We subsequently reviewed several computational modeling studies to demonstrate the contribution computational modeling can make to the parameter-setting enterprise. Specifically, we can use computational modeling techniques to explore what learning strategies make a parametric grammar acquirable. If we find certain parametric grammars are not acquirable from available child input, this can bear on the validity of the proposed parameters that comprise those grammars (cf. Frank and Kapur 1996). Even with the correct parameters, children

may still need to bring additional learning biases to the task of language acquisition in order to correctly set their parameters – another possibility that can be explored easily within a computational modeling framework. One of the benefits of explicit computational modeling approaches to parameter-setting is that alternatives may differ in the predictions they make about the time course of acquisition (e.g., based on the amount of data required to set the parameters as in Yang (2004)), which makes it possible to test the validity of parametric approaches against the behavior of actual learners. We hope that this chapter inspires continued research into the linguistic parameters that could make acquisition as swift and relatively easy as it seems to be.

10

Bilingualism beyond language: On the impact of bilingualism on executive control

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10.1 Introduction

Society is becoming more and more multicultural every day. This means that many individuals are able to speak more than one language, some of them even growing up in a completely bilingual environment. In this context, many questions about the potential implications of being a bilingual speaker are raised not only by scientists but also from society in general. One of the most provocative and exciting questions refers to the potential collateral effects that using two languages may have for the development of various cognitive abilities besides language. This issue leads to intense and often controversial debates and, beyond any socio-political interests, requires an empirical approach. It is the goal of the present chapter to review our current knowledge about this topic and provide some guidelines for further research.

Early research on the collateral effects of bilingualism for cognition was surrounded by obscure myths based on wrong assumptions about how the brain works. Especially between 1920 and 1960 the widespread view was that bilingualism resulted in cognitive deficiencies and mental disturbances. For example, Adler (1977) wrote about bilinguals: “Often (bilinguals) have split minds . . . all the particularities which language conveys, historical, geographical, cultural, are re-embodied in the bilingual twice: he is neither here nor there: he is a marginal man” (38). Adler also wrote about the bilingual child: “His standards are split, he becomes more inarticulate than one would expect of one who can express himself in two languages, his emotions are more instinctive, in short, bilingualism can lead to a split personality and, at worst, to schizophrenia” (40). It is a long time ago that

these arbitrary and experimentally uninformed views about bilingualism were expressed. Since then, years of experimental research have provided evidence that bilingualism actually has implications for general cognition, but they could not be farther from those assumed in earliest research. In particular, it has been demonstrated that bilingualism enhances executive control, which refers to a collection of high-level cognitive abilities enabling an individual to successfully engage in independent goal-directed behavior.

This review focuses on the phenomenon of bilingualism having positive collateral effects on general-domain executive control. We first provide a broad introduction to what causes bilingualism to affect general-domain executive control. Second, we briefly review the first experimental studies with children, which started settling the guidelines of the empirical research on the issue. Next, we discuss findings on the effect that bilingualism has on one executive control process in particular: conflict processing. Then, we also review evidence that being a bilingual entails the development of more efficient executive control across lifespan. Finally, we highlight critical issues for future research.

10.2 The impact of bilingualism on executive control: Introduction to the phenomenon

One of the remarkable abilities of bilingual speakers is that of keeping their two languages apart during speech processing. Bilinguals rarely show involuntary lexical intrusions from the non-intended language (see Poulisse and Bongaerts 1994). Thus, unlike monolinguals, bilinguals do not only need to select the appropriate words for the message they want to convey but also they have to guarantee that those words belong to the intended language. This is even more remarkable considering the speed and reliability with which speakers produce language (about 4 words per second, with less than 1 error per 1000 words produced). This extra cognitive process that bilinguals need to bring into play has been referred to as language control, and as we will argue later on, seems to depend on the involvement of executive control processes.

Furthermore, bilinguals show another striking ability that is more apparent in conversations in which the two languages are in play. On many occasions, although this depends on the sociolinguistic scenario in which bilinguals find themselves, individuals are involved in conversations in which the two languages are being used. That is, a bilingual speaker may use one language when speaking with one of the interlocutors and the other language with the other interlocutor. Certainly, this situation is only functional when the three interlocutors know the two languages, otherwise the conversation won't be successful. However, this is not an uncommon conversational setting, for example, in families

in which the two parents speak different languages to their children, or in societies in which the two languages are very widespread and used in a wide variety of contexts (as in the Catalan-Spanish community). At any rate, what this activity shows is the flexibility of bilingual speakers to switch between their two languages with, apparently, not a massive cognitive effort.

Both of these abilities of bilingual speakers, namely the ability to focus their lexicalization process in one language when needed, and the ability to switch between their two languages, are at the basis of language control. Without the involvement of such language control bilingual language processing would be chaotic. Functionally, this language control mechanism needs to incorporate processes that: (a) allow potential conflict between the two languages to be resolved and (b) keep track or monitor the language that is appropriate for each specific interlocutor in each conversational setting.

Perhaps the best example of how essential language control is for bilingual communication comes from neuropsychological bilingual patients who, after suffering brain damage, are unable to perform language control any more (e.g., Abutalebi *et al.* 2000; Mariën *et al.* 2005). The lack of language control in these patients results in mixing and constantly switching languages, even though patients are aware of the interlocutor being unable to understand one of their languages. This was the case of patient A.H., reported by Abutalebi *et al.* (2000), a 74-year-old polyglot woman with Armenian, English, and Italian who suffered from a stroke affecting her left caudate. As illustrated in this short transcription of her spontaneous language, A.H. was unable to perform language control any more: *I cannot communicate con you today; Oggi, I cannot say il mio nome to you* (portions in bold were said in Italian; taken from Abutalebi *et al.* 2000). The A.H. case illustrates that this extra and unavoidable step of language control is crucial for the efficient communication of bilinguals. Thus the mechanisms underlying bilingual language control have been the object of several studies.

In this chapter, however, we do not focus on how the language control mechanism works but on its potential collateral effects for cognition. Particularly, we focus on the hypothesis that language control performed by highly proficient bilinguals constantly exposed to their two languages influences non-linguistic cognitive processes, in particular general-domain executive control. This hypothesis has received the attention of many researchers motivating a large number of experimental studies, especially during the last decade. Before presenting the experimental evidence reported in the literature in support of this hypothesis, let us briefly expose what we refer to by executive control processes and why they should be affected by bilingualism.

General-domain executive control stands for a collection of cognitive processes which allow us to behave in a goal-directed manner. Some of these processes consist of the appropriate set-maintenance for strategic

planning, disablement of the strategy in response to changes in goals or changes in the task environment, attention, working memory, switching between mental sets, organized search, flexibility of action, suppression of prepotent responses, task monitoring, and updating working memory representations (e.g., Fuster 1980; Logan 1985; Luria 1973; Stuss and Benson 1984, 1987). To illustrate executive control by an easy example, let's imagine the host for the dinner tonight going out late from work. While walking to his car he tries to remember whether any guest is vegetarian or has any other special restriction. Then, he makes a guess about the fastest route to the supermarket and at the same time he gets into the car, starts the engine and releases the handbrake. While driving, he attends to the traffic, changes gear, adjusts the rear-view mirror's position, keeps the right direction and thinks about the ingredients for the menu. All these sequences of actions require many simultaneous executive control processes. Very broadly, we could identify some of these processes. For example, strategic planning allows him to decide the menu and the fastest direction to the supermarket; thanks to set-maintenance he can start planning the dinner while starting the car; visual attention is engaged while driving; working memory is required to mentally check the appropriate ingredients for the menu; and mental flexibility is crucial for switching from the mental set of the menu to the mental set of driving every time any alerting signal requires it (e.g., the sound of an ambulance, a sudden braking of the car in the front). Nevertheless, it is the orchestrated functioning of all these processes that allows our host to behave in a goal-directed manner. In other words, it is the engagement of the so-called general-domain executive control processes that leads him to successfully achieve his goal.

The question of interest in the present context is why should bilingualism affect the development of some of these general-domain executive control processes? In particular, why would the extra training in language control affect the development of executive control?

Perhaps the most important reason backing up such a hypothesis is the fact that functional and structural development of cognition is not only tied to maturational constraints but is also affected by environmental factors. For example, Maguire *et al.* (2000) observed that London taxi drivers with extensive navigation experience had different hippocampal volumes (enlarged in posterior and reduced in anterior regions) relative to control individuals without navigation experience. Similarly, Gaser and Schlaug (2003) found that gray matter volume in motor, auditory, and visual-spatial brain regions differed between professional keyboard players and both amateur and non-players. Another example is the study by Green and Bavelier (2003) showing enhanced visual attention skills in habitual video-game players relative to non-players.

All these examples indicate that at least some environmental factors can modify specific cognitive systems. Bilingualism, and more concretely the

processes involved in bilingual language control, might be one of these environmental factors. In particular, controlling two languages continuously probably acts as an environmental factor with a modeling role on the development of executive control processes. As highlighted by Abutalebi and Green (2007) through a meta-analysis based on neuroimaging data, bilingual language control involves some of the brain structures responsible for general-domain executive control. For example, it seems that bilinguals use the prefrontal cortex mechanisms, typically engaged to override prepotent irrelevant responses, to block L1 utterances when trying to produce speech in a less-dominant L2. Similarly, bilinguals would use the general-domain conflict detection mechanisms subserved in the anterior cingulate cortex to override conflict between the simultaneously activated translations of a concept (e.g., “dog” vs. “perro”). Also, parietal mechanisms typically engaged to bias attention away from a disused previous task would help bilinguals to switch from one language to the other.

In short, bilingual language control shares functional and neuronal mechanisms with general-domain executive control processes. The prediction derived from this (at least partial) functional and anatomical overlap is that bilinguals would employ general executive control processes more frequently than monolinguals, resulting in a different and/or more efficient development of executive control brain networks. In other words bilinguals would engage more efficiently (at least some of) the executive control processes required for a goal-directed behavior similar to the one of our host. This prediction would be experimentally confirmed by observing that bilinguals outperform monolinguals in non-linguistic executive control tasks.

Before reviewing the experimental evidence it is important to underline the type of bilinguals that have been, mostly, involved in research on executive control and bilingualism. Up to date experimental evidence has focused on speakers that grow up in a bilingual context (developing native-level skills in two languages) and, hence, need to perform bilingual language control continuously. The performance of this type of bilinguals in non-linguistic tasks requiring executive control has been compared to that of individuals who, despite having acquired foreign language knowledge in formal academic contexts, only use one language (their first and dominant language) on a daily basis. Hence, one should be careful when extrapolating the results that will be reviewed to other types of bilingual speakers (see section on future directions).

10.3 Impact of bilingualism on executive control: A brief review

The hypothesis that controlling two languages during speech would lead to developing more efficient executive control networks has been tested

by comparing the performance of bilinguals and monolinguals in tasks requiring non-linguistic executive control (e.g., Abutalebi *et al.*, *in press*; Bialystok 2006, 2010, 2011; Bialystok and Feng 2009; Bialystok and Viswanathan 2009; Bialystok *et al.* 2004, 2005; Bialystok, Martin, and Viswanathan 2005; Bialystok, Craik, and Ruocco 2006; Bialystok, Craik, and Ryan 2006; Bialystok *et al.* 2008, 2010; Calabria *et al.* 2011; Carlson and Meltzoff 2008; Colzato *et al.* 2008; Costa *et al.* 2008, 2009; Fernandes *et al.* 2007; Garbin *et al.* 2010; Hernández *et al.* 2010 [Experiment 1], 2012; Luk *et al.* 2010; Luo *et al.* 2010; Martin-Rhee and Bialystok 2008; Prior and MacWhinney 2010; Poulin-Dubois *et al.* 2011; Prior and Gollan 2011; Treccani *et al.* 2009). The earliest empirical evidence that bilingualism has a positive effect on executive control processes comes from studies with children conducted mostly in Canada by Ellen Bialystok and colleagues. In this early research, studies mainly focused on the effects of bilingualism on non-linguistic inhibitory processes due to the general assumption that these processes were crucial for preventing interference from one language to the other during bilingual speech production. In a series of experiments, Bialystok *et al.* (2004) compared bilingual and monolingual children (about 5 years of age) in different adaptations of the card sorting task originally developed by Zelazo and colleagues (Frye *et al.* 1995; Zelazo *et al.* 1996). In this study, children were asked to sort objects drawn on cards according to different target dimensions, the nature of which varied across the different versions of the task. In the perceptual target dimensions task, children were asked to sort objects by color (red vs. blue) or by shape (square vs. circle). In the semantic target dimensions task, children sorted objects by their functionality (“things to play” [e.g., bicycle] vs. “things to wear” [e.g., slippers]) or by their habitual location (“things that go inside the house” [e.g., slippers] vs. “things that go outside the house” [e.g., bicycle]). Crucially, the matching criterion changed at a given point so that children needed to switch from one target dimension (e.g., color) to the other (e.g., shape). Bialystok *et al.* (2004) found that bilinguals outperformed monolinguals in switching from one target dimension to the other in the perceptual version of the task while no group effect was observed in the semantic version. These results were interpreted as evidence that bilinguals and monolinguals were equally efficient in constructing mental representations of objects, which are “non-executive control” processes particularly necessary to extract the relevant semantic features of objects. In contrast, bilingual children seem to have more efficient inhibitory processes that allow them to better prevent attending to a prepotent mental representation, that is, the old relevant target dimension after a shift in the sorting criterion. The observed bilingual advantage was attributed to bilinguals being more experienced in inhibiting misleading information due to their constant need to inhibit word forms of the unintended language. The finding that bilingual preschoolers outperform monolinguals in inhibitory processes was replicated by means

of other tasks also requiring executive control. For example, Bialystok and Senman (2004 [Study 2]), examined inhibitory processes by means of the appearance-reality task (Flavell *et al.* 1983). In this task, children were presented with objects with perceptual features leading to a misperception of the real identity of the object (e.g., a book with the shape of a snowman or a marker with the shape of a whale). In order to correctly answer the question about the real identity of the object, children needed to engage inhibitory processes allowing them to ignore the object properties leading to incorrect object identification. In this study Bialystok and Senman (2004) observed that bilingual children performed better than monolingual children on identifying the real objects. In another study, Bialystok and Shapero (2005) observed that bilingual children (about 5½ years of age) outperformed monolinguals in two tasks requiring inhibiting misleading information. In the so-called “ambiguous figures task,” children were presented with figures with two alternative images (e.g., a figure that could represent the shape of a man or the shape of a rat). Bilinguals were more successful than monolinguals in perceiving the alternative image in these ambiguous figures. In the so-called “opposite world’s task” the inhibitory processes were examined by asking children to suppress the habitual name of objects and replace it by another (e.g., call each pig a cow and vice versa). Bilingual children outperformed monolinguals in this task as well.

Bialystok and colleagues’ research evidenced that bilingualism plays a crucial role in the development of inhibitory processes, and motivated the question of whether inhibition was the only executive control process influenced by bilingualism. Thus, at this point, new studies started to focus on many different executive control processes, either with children or adults, as potential targets for the effect of bilingualism (e.g., dual-task processing [Bialystok, Craik, and Ruocco 2006a]; reactive inhibition [Colzato *et al.* 2008]; task-switching [Prior and MacWhinney 2010; Prior and Gollan 2011]). Bialystok, Craik, and Ryan (2006) even tried to isolate different executive control processes (response suppression [the ability to withhold an habitual response], inhibitory control [suppressing interference from conflicting information], task switching [holding in mind two sets of instructions and executing the correct one in accordance with a cue]) in a single complex task, as well as to examine the extent to which bilingualism affects each of them.

The fact that the executive control construct is still not well understood renders the pattern of bilingual advantage observed in these studies challenging to interpret. The general problem comes from the difficulty to disentangle every single one of these executive control processes given that it is the orchestration between several processes that composes the theoretical construct of executive control. In this respect, conflict processing being one of the better understood executive control processes, many studies have focused on the effects of bilingualism on this particular

process (e.g., Bialystok 2006; Bialystok *et al.* 2004; Bialystok, Martin, and Viswanathan 2005; Bialystok *et al.* 2005; Costa *et al.* 2008, 2009; Hernández *et al.* 2010; Martin-Rhee and Bialystok 2008). Relative to other executive control processes, the anatomical and functional mechanisms involved in conflict processing, despite not being fully understood, have been widely described. Thus, results of studies on the effects of bilingualism on conflict processing have had a relatively clear theoretical basis for its interpretation. As exposed in the next section, these results have been crucial to advance knowledge on how bilingual language control may affect general-domain executive control processes.

10.4 Impact of bilingualism on conflict processing: The facts

One of the most studied executive control processes is that of processing a conflict between two competing responses. On a daily basis, we are constantly exposed to this sort of conflicting situation. For example, a traffic signal leading you to turn right located on the left side of the street would create a conflict between turning right (correct behavior) or turning left (conflicting behavior elicited by the location of the traffic signal). In experimental research, this type of conflict resolution process has been investigated by means of the so-called Stroop-like tasks, such as the Simon task or the flanker task (see [Figure 10.1](#) for a description of these tasks). These experimental tasks require engaging executive control processes to resolve the conflict between two competing responses. Specifically, there are two different components relevant for conflict processing and, hence, to deal with this sort of experimental task: conflict resolution and conflict monitoring. Conflict resolution is believed to reflect conflict overriding processes, particularly inhibition. It is indexed by comparing the time needed (i.e. the cost) to respond to incongruent trials relative to either congruent or neutral trials (cf. [Figure 10.1](#)). In contrast, conflict monitoring consists in constantly assessing the need to engage in conflict resolution. This component is typically indexed by the slower performance in contexts mixing similar proportions of incongruent (requiring conflict resolution) and congruent (free of conflict) trials relative to those contexts where most of trials are of one type (congruent or incongruent). This is believed to reflect the evaluation of the current level of conflict which is used by the monitoring system to trigger conflict resolution mechanisms such as inhibition.¹

While conflict monitoring was not considered to be an object of modulation by bilingualism until recently, conflict resolution has always been a good candidate for such modulation. The widespread hypothesis has been that, as a result of constantly inhibiting the lexical forms of the unintended language, bilinguals would become overtrained in general-domain inhibitory processes. The hypothesized bilingual advantage in

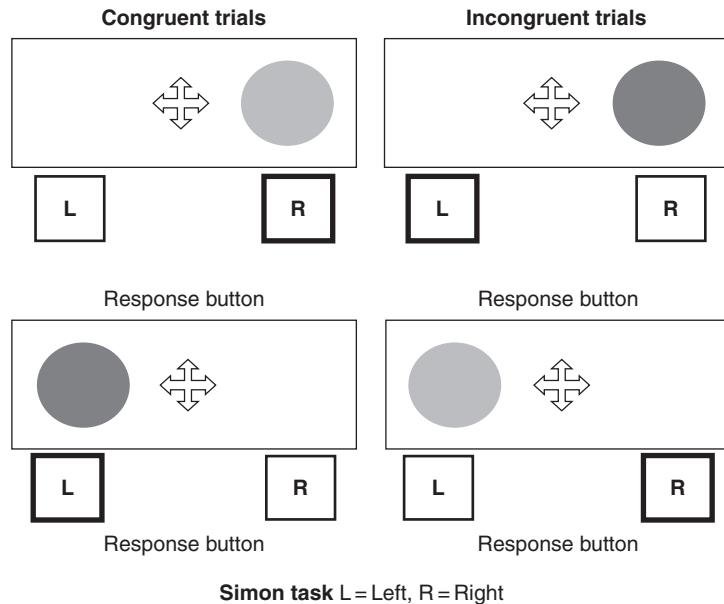


Figure 10.1 Stroop-like tasks as a tool to measure conflict processing. In a classic color-word Stroop task (Stroop 1935) participants are asked to name the ink color of written color words (e.g., say “red” to the word *blue* written in red ink). The typical result is that participants are slower in naming the ink color of incongruent color words (i.e. *blue*; conflict effect) than in naming the ink color of a row of neutral Xs or congruent color words (i.e. *red*). The conflict effect reflects the temporal cost of engaging executive control processes to resolve the conflict between two competing responses: the correct one (*red*) and the irrelevant one (*blue*). Several adaptations of this classical color-word Stroop task have been used during years of research on executive control processes involved in conflict processing. One example of a widely used adaptation of the Stroop task is the Simon task, which has been frequently used to investigate bilingualism effects on executive control. In a classical Simon task (Simon and Ruddell 1967) two targets are associated to different spatial responses (respond with the left hand to a black circle, and with the right hand to a white circle). Crucially, the stimulus can appear in the right or left position of the screen. The color of the stimulus is the target dimension and its position, the distracter dimension. The Simon effect stands for the faster reaction times (RTs) when the response required by the target dimension matches the location of the stimulus on the screen (congruent trials – black circle presented on the left of the screen) than when it does not (incongruent trials – black circle presented on the right of the screen). This effect reveals the time required to solve the conflict between the two contradictory spatial responses elicited by the target and the distracter dimensions. Another Stroop-like task is the flanker task (e.g., Fan *et al.* 2002) where participants are asked to determine the direction of a central target arrow. Surrounding the target arrow, there are flanker arrows that point to either the same (congruent trials →→→→) or opposite direction (incongruent trials →→←→). Incongruent trials lead to slower RTs than congruent trials (conflict effect), revealing the time cost associated with conflict resolution.

general-domain inhibitory processes has been indexed in studies showing a reduced conflict effect in bilinguals relative to monolinguals (cf. **Figure 10.2**; e.g., Bialystok *et al.* 2004; Costa *et al.* 2008, 2009 [Experiment 2]; Hernández *et al.* 2010 [Experiment 1]).

However, an additional effect of bilingualism has also unexpectedly been found across studies: bilinguals are faster than monolinguals

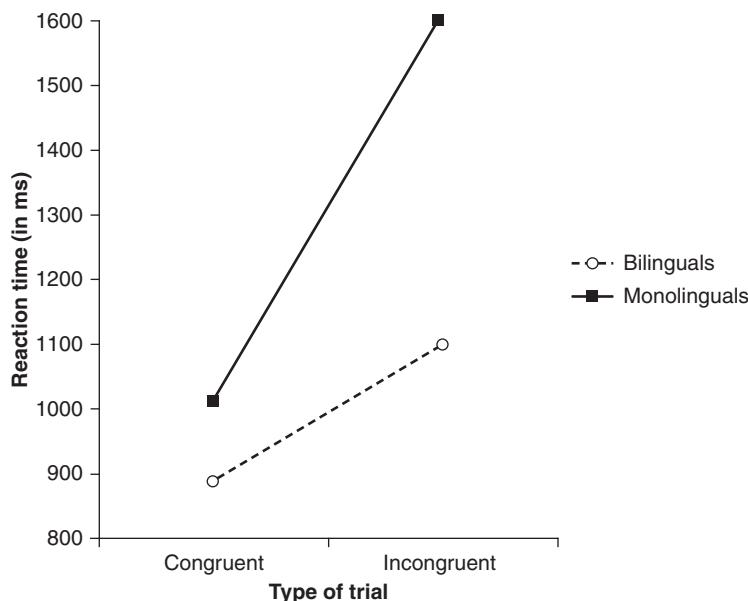


Figure 10.2 The two signatures of bilingualism in conflict processing. Data taken from Bialystok *et al.* (2004 [Study 2]) comparing the performance of bilinguals and monolinguals in a Simon task. These results clearly show that bilingualism modulates the magnitude of the conflict effect (RTs incongruent trials – RTs congruent trials), which was 212 ms for bilinguals and 583 ms for monolinguals. In addition, bilinguals were overall faster than monolinguals across the task regardless of the type of trial (213 ms faster in congruent and 494 ms faster in incongruent trials).

regardless of the presence of conflict, that is, in any type of trials (neutral, congruent, and incongruent; e.g., Bialystok *et al.* 2004; Bialystok, Martin, and Viswanathan 2005; Costa *et al.* 2008, 2009 [Experiment 2]; see Figure 10.2). Given that this bilingual advantage was present even in those trials free of conflict (congruent, neutral), it could not be attributed to inhibitory processes brought into play to override conflict. This effect remained practically unexplored until Costa *et al.* (2009) suggested that the overall faster reaction times (RTs) in bilinguals could reflect a better efficiency in conflict monitoring.

In a meta-analysis including all the Stroop-like tasks (cf. Figure 10.1) comparing the performance of bilinguals and monolinguals up to 2008, Costa *et al.* (2009) showed that the effect of bilingualism on overall reaction times was actually much more consistent across studies than the reduced conflict effect. This meta-analysis revealed that in 30% of the studies bilinguals were overall faster than monolinguals. In contrast, only in 13% of the studies was the bilingual advantage indexed by a reduced conflict effect, and always in addition to the overall reaction times effect. In other words, it turned out that no study reported a reduced conflict effect for bilinguals in the absence of overall faster reaction times (see Figure 10.3).

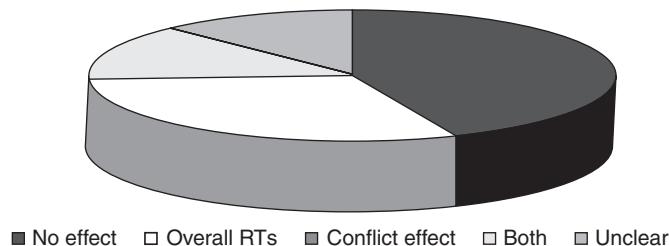


Figure 10.3 Schematic illustration of Costa *et al.*'s (2009) meta-analysis showing in which proportion bilingualism modulates performance in Stroop-like tasks. Out of 23 studies, bilingualism exerted no effect on performance in almost half of them. In all the studies where bilingualism influenced performance, bilinguals were overall faster than monolinguals. In a small proportion of these studies, bilinguals showed a reduced conflict effect in addition to the overall faster RTs. The impact of bilingualism was unclear in a couple of studies due to the lack of statistics or other ambiguous information.

The question then was why bilinguals should be overall faster than monolinguals. One possibility is that bilinguals are better at assessing the need to engage conflict resolution processes in every trial, that is, they are better at conflict monitoring. The rationale is that the more efficiency in conducting this assessment, the faster performance in overall trials. In other words, the potentially better ability of bilinguals to determine the specific behavior required in every single trial (engaging or disengaging conflict resolution processes, e.g., inhibition), would result in overall faster responses across the task. Costa *et al.* (2009) tested this hypothesis by means of a parametrical manipulation of the flanker task. In this study bilinguals and monolinguals were compared in four versions of the flanker task: two of them with similar proportions of congruent and incongruent trials, meaning high monitoring demands (i.e. 50% and 75% congruent task-versions). In two other task-versions the majority of trials were of one type, leading to low monitoring demands (i.e. 8% and 92% congruent task-versions). The authors predicted that the bilingual advantage in overall RTs would be more evident in contexts with high monitoring demands, based on the hypothesis that bilingualism improves conflict monitoring. Costa *et al.*'s prediction was confirmed as bilinguals were overall faster than monolinguals only in high monitoring demands task-versions. The difference between bilinguals and monolinguals' overall RTs increased as a function of monitoring demands (see [Figure 10.4\(a\)](#)), being especially evident when monitoring demands were maximal (i.e. 50% congruent task-version; see [Figure 10.4\(b\)](#)). These observations suggested that bilinguals have a more efficient capacity to evaluate the particular demands of every trial throughout the entire task in order to adjust behavior whenever needed.

What about conflict resolution, then? As pointed out above, conflict resolution in Stroop-like tasks has been typically taken as an index of inhibition. Therefore, a modulation of the conflict effect in bilinguals can be interpreted as evidence that bilingualism aids inhibitory processes.

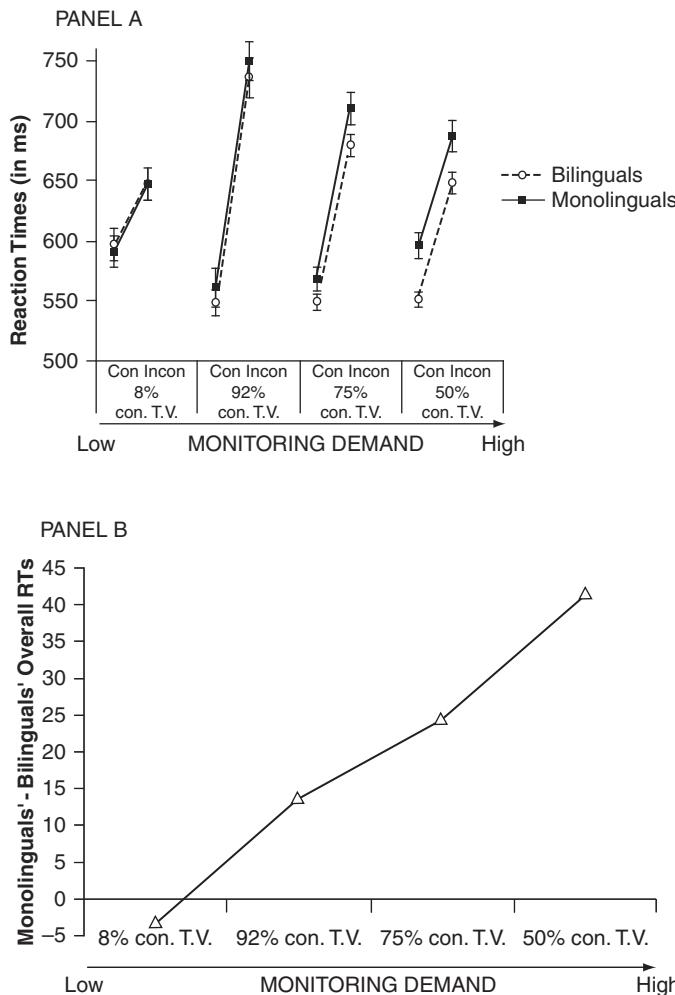


Figure 10.4 (a) Overall RTs broken by group of participants, task-version, and type of trial taken from Costa *et al.* (2009). The monitoring demands increase from left to right. Error bars represent standard errors. Con = congruent; Incon = incongruent; T.V. = task-version. (b) Difference between bilinguals and monolinguals in overall RTs (congruent and incongruent trials collapsed) as a function of task-version. The monitoring demands increase from left to right.

Costa *et al.* (2009) observed this modulation in only one task-version, the 75% congruent one, where the conflict effect was 27 ms smaller for bilinguals relative to monolinguals in the first block of the task.² This observation confirmed that bilingualism aids conflict resolution. However, conflict resolution does not seem to be the executive control component most influenced by bilingualism. Results from Costa *et al.* (2009) along with those from previous literature reported in their meta-analysis suggest that the bilingual advantage on conflict resolution is an elusive effect, only detectable in certain contexts. A detailed examination of the contexts in which this effect is mostly present reveals that it typically comes along

with specific proportions of congruent (free of conflict) and incongruent (requiring conflict resolution) trials. Those are the contexts where effects of bilingualism on conflict monitoring are also observed. In particular, it seems that conflict resolution is modulated by bilingualism only in those contexts with high monitoring demands in which incongruent trials are less frequent than congruent ones (although still keeping a high need of constant monitoring: e.g., 33% incongruent trials [e.g., Costa *et al.* 2008; Hernández *et al.* 2010], 25% incongruent trials [Costa *et al.* 2009] [Experiment 2]).

10.5 Impact of bilingualism on conflict processing: The explanation

As reviewed above, although with different robustness, there seems to be two processes related to conflict processing that are affected by bilingualism: (a) conflict resolution and (b) conflict monitoring.

The impact of bilingualism on conflict resolution seems to reflect a better functioning of inhibitory control mechanisms in bilingual compared to monolingual speakers. This better functioning would have its origins in the way the language control system works, in the sense that it would make use of inhibitory processing (Green 1998). However, note that not all models of language control agree on postulating inhibitory language control processing. In fact, there have been quite explicit proposals about how the language control system may shift from a reliance on inhibitory processing when proficiency in L2 is low, to a more built-in lexical control processing (that would not make use of inhibitory control) when proficiency in L2 increases (Costa and Caramazza 1999; Costa *et al.* 1999; Costa *et al.* 2006). Be this as it may, it appears that inhibitory processes seem to be involved in language control, at least at some points in time during language development. Consequently, the most ready explanation at this time to account for the bilingual effect in conflict resolution is the use of common inhibitory systems when resolving non-linguistic and linguistic conflicting situations.

Regarding conflict monitoring, we can find only one tentative explanation advanced in Costa *et al.* (2009). We argued that the cause of the bilingual advantage in such processes is the need for constantly evaluating which language to speak to whom, in accordance with the language-interlocutor associations previously established. This would require the engagement of monitoring processes. That is, bilinguals would need to perform language monitoring in every communicative situation in order to select the appropriate language for the utterances according to the language used by the interlocutor/s. This language monitoring becomes crucial in communicative situations with multiple interlocutors where a bilingual switches between his two languages as a function of the specific

interlocutor/s. In this kind of situation, the bilingual would need to go back and forth between his two languages, keeping track of the language used in the previous utterance, and constantly evaluating the need to suppress/ignore one language to engage the other. This language monitoring would be the cause for the enhancement of general-domain monitoring processes reported above.

10.6 On the effects of bilingualism through development

Having established that bilingualism has a positive impact on executive control, several issues appear to be relevant regarding the development of such an effect.

The first issue is whether bilingualism affects the functioning of executive control at all ages, or whether its impact is more restricted to some specific periods of life. In the earliest experimental studies, the effect of bilingualism on executive control was only observed during development (in childhood) and decline (during middle and old age). For example, Bialystok, Martin, and Viswanathan (2005; Studies 1 and 2) observed overall faster RTs and reduced conflict effect for bilingual relative to monolingual children. Overall faster responses for bilinguals relative to monolinguals were also observed in middle aged (30–60 years of age) and older adults (60–80 years of age; Bialystok, Martin, and Viswanathan 2005; Studies 4 and 5). However, when administering the Simon task to young adults (20–30 years of age) no difference was observed between bilinguals and monolinguals (e.g., Bialystok, Martin, and Viswanathan 2005; Study 3). These observations regarding the presence of a bilingual advantage across the lifespan led Bialystok, Martin, and Viswanathan (2005) to propose that the effects of bilingualism on executive control processes are undetectable in young adulthood due to both bilinguals and monolinguals being at the peak of their cognitive efficiency. In other words, when executive control abilities are at their maximal capacity, there is no room for bilingualism to exert any significant effect. However, this possibility was later disproved by Costa *et al.* (2008). These authors found the two signatures of bilingualism (i.e. reduced conflict effect and overall faster RTs) when comparing a group of 100 young bilinguals with a group of 100 monolinguals in a flanker task, all of them ranging between 17 and 32 years old. The effects of bilingualism in young individuals were replicated in posterior studies (e.g., Costa *et al.* 2009 – 75% congruent task-version; Hernández *et al.* 2010). Note that, despite being robust, the impact of bilingualism on conflict resolution during young adulthood is just a matter of a few milliseconds (e.g., 14 ms in Costa *et al.* 2008; 27 ms in Costa *et al.* 2009 – 75% congruent task-version). This small difference in the magnitude of the conflict effect between young bilinguals and monolinguals contrasts with the huge differences reported with middle-aged (ranging from 30 to 60 years old) and

older individuals (ranging between 60 and 80 years old). For example, the magnitude of the bilingual advantage of middle-aged individuals was 115 ms in the easiest and 88 ms in the hardest version of the Simon task in Bialystok *et al.* (2004 [Study 2]). In the same study, the bilingual advantage of older individuals was 371 ms in the easiest and 610 ms in the hardest version of the Simon task.³ Considering that executive control processes are sensitive to age-related changes (e.g., Diamond 2002; Nelson 1995; Pennington 1994; Welsh 2002), variations in the amplitude of the bilingual advantage across different stages of life are consistent with the idea that the less biologically mature executive control processes are, the more room there is for external factors to exert their impact. Thus, given these results we can conclude that bilingualism does actually have an impact on executive control across life-span, although such an impact is less important when bilinguals are at the peak of their cognitive abilities.

The second issue regarding the development of the bilingualism effect on executive control refers to whether bilingualism has an impact in children that do not produce language yet. In other words, do preverbal bilingual babies (babies exposed to two languages but speaking neither yet) have a faster development of their executive functions? The experimental evidence on this issue is very scarce but also very interesting. In a recent article by Kovács and Mehler (2009), 7-month-old bilingual infants outperformed monolingual infants in a task requiring switching responses. Babies were habituated to look at one side of the screen where a puppet intermittently appeared. After some training, babies tended to anticipate their gaze towards the side where the puppet would appear, even before the puppet was presented. After several trials, the puppet was presented on the other side of the screen without previous notice. Infants were supposed to stop anticipating their gaze towards the previous side and start anticipating towards the new location. The results showed that bilingual babies were able to do so and look at the new location, while monolingual babies were unable to do so. These results are very interesting not only because they show the very early effects of bilingualism on executive control, but also because they bring a potential new way to understand previous results. As discussed above, the most common explanation for the bilingual advantage in conflict processing stems from the need to control the two languages during speech production. If this argument is correct and if it is the only source of the bilingual advantage, how is it that babies who do not produce language already show an effect of bilingualism? Kovács and Mehler (2009) proposed that even preverbal bilingual infants need to perform control between the two languages even if they do not produce them yet. In particular, preverbal bilinguals would engage executive control processes in order to selectively construct and access the representations of the two languages. Thus, Kovács and Mehler's findings suggest that the origin of the effects of bilingualism on general-domain executive control could be related to the need to keep the two languages

apart even during language perception. In that sense, bilinguals would need, for example, to keep track of the language of each interlocutor to be able to associate a specific language with each interlocutor.

The third issue raised in the context of the development of the bilingual impact on executive control refers to the age at which the two languages have to be learned for such effects to be present. This is an extremely interesting issue since it can inform us about how critical periods in one cognitive domain may affect other cognitive domains. Unfortunately, we lack evidence on this issue at present.

10.7 Summary

Contrasting with the obscure connotations associated with bilingualism several decades ago, modern experimental research has revealed that bilingualism actually has a positive impact on general cognition. In particular, bilingualism affects general-domain executive control, a broad compendium of cognitive processes working in orchestration to allow individuals to achieve goal-directed behavior. The reason why bilinguals develop more efficient executive control processes is believed to be the anatomical and functional overlap between bilingual language control and general-domain executive control. Due to the availability of the Stroop-like paradigm and the well-known behavior associated with it, most studies to date have addressed the effects of bilingualism on one executive control process in particular, namely, conflict processing. This process includes conflict resolution (the ability to override interference from conflicting information) and conflict monitoring (the ability to keep track of the presence/absence of conflict in order to engage/disengage conflict resolution processes). In earliest studies, researchers mainly focused on the bilingual advantage in conflict resolution (indexed by a reduced conflict effect in bilinguals relative to monolinguals) which is thought to mainly depend on inhibitory processes. However, more recent studies have shown that the main executive control component affected by bilingualism seems to be conflict monitoring (indexed by overall faster RTs in bilinguals relative to monolinguals). At present, it is still unclear why conflict resolution (hence, inhibitory processes) is additionally but not mainly affected by bilingualism. A potential answer to this question is that keeping track of the languages used by the interlocutors during bilingual conversations may be more demanding in terms of bilingual language control relative to resolving conflict between the two translations of a given lexical form.

Despite being harder to detect when individuals are at the peak of their executive control capabilities (i.e. young adulthood), the effects of bilingualism on executive control have been observed in individuals at different stages of life. Thus, bilingualism seems to have an impact across

lifespan, even in preverbal stages, which suggests that not only speaking but also perceiving two languages is enough to develop more efficient executive control processes.

10.8 Future directions

At present, the bilingual advantage in general-domain executive control seems to be without question. The current efforts are mainly focused on determining bilingualism effects on different executive control processes (e.g., task-switching [Prior and MacWhinney 2010; Prior and Gollan 2011]); attentional guidance [Hernández *et al.* 2012]). Whether bilingualism has an impact on other cognitive processes tightly related to executive control (i.e. alerting and spatial orienting components of attention) is still an open question. Costa and colleagues have conducted some work on this issue (Costa *et al.* 2008, 2009; Hernández *et al.* 2010) indicating that the effect of bilingualism does not extend to processes of spatial orienting. However, the potential effects of bilingualism on alerting are still unclear. In the following, we will develop some unexplored issues that are crucial for advancing knowledge on the origin of the bilingual advantage on executive control.

1. Are there quantitative and/or qualitative differences in the bilingualism effect on executive control as a function of the “type of bilingualism”? Since the need to control two languages is believed to be behind the bilingual advantage in executive control, one of the most interesting questions is whether this advantage is present for bilingualism within a context of *diglossia* (i.e. every language of a bilingual is mainly associated with a specific context – e.g., L1 at home, L2 at work). In the context of *diglossia*, bilinguals do not alternate between languages and do not participate in bilingual conversations very often. For this population, language monitoring is probably not the most crucial aspect of bilingual language control. Thus, it is possible that the bilingual advantage in conflict monitoring is much reduced in “*diglossic*” bilinguals relative to those bilinguals using their two languages independently of the context.

Another interesting question is whether the bilingual advantage in executive control is modulated by the extent to which the two languages of a bilingual differ. For example, two Romance languages (e.g., Spanish and Italian) are more similar in terms of phonological overlap between equivalent translations and grammatical constructions than a Romance language and an Anglo-Saxon language (e.g., Spanish and English). There is a general assumption that the more similar the two languages, the less need of bilingual language control. This assumption comes from the observation that the phonological overlap between equivalent translations facilitates lexical retrieval in bilinguals with two Romance languages (i.e. cognate effect; see Costa *et al.* 2005). However, it is possible that this phonological

overlap also makes it harder to suppress the equivalent translation of the unintended language. For this reason, executive control processes might be differently affected depending on the overlap between both languages of a bilingual speaker.

The question of whether late bilinguals experience an enhancement of executive control as early bilinguals do is also relevant to understanding the origins of the phenomenon. If we consider the enhancement of executive control in bilinguals as a consequence of overtraining, we could expect late bilinguals to develop executive control processes at the same pace as they develop proficiency in language control.

2. *To what extent can we associate bilingual language control with the development of more efficient general-domain executive control?* Some studies suggest that the extra step of language control necessary for language processing in bilinguals compared to monolinguals is reflected in slower and less accurate performance in tasks requiring language production (e.g., picture-naming tasks [e.g., Gollan *et al.* 2005], even when producing language in L1 [e.g., Ivanova and Costa 2008]). As exposed in this chapter, the same mechanisms of bilingual language control are also responsible for the bilingual advantage in executive control, an assumption objectively evidenced by means of Abutalebi and Green's (2007) meta-analysis. However, to date, no study has compared the performance of the same bilinguals and monolinguals in both language production and non-linguistic executive control tasks. In addition to the bilingual disadvantage in language production along with the bilingual advantage in executive control, bilinguals should show a negative correlation between the two tasks.

3. *To what extent is the bilingual advantage in executive control functional on a daily basis?* As exposed above, the bilingual advantage in executive control experimentally observed is a matter of few milliseconds. Nevertheless, there are some clues suggesting that bilingualism has indeed a functional impact in everyday life. For example, Bialystok, Craik, and Freedman's (2007) study reveals that bilingualism could act as a factor of cognitive reserve against dementia (see also Gollan *et al.* 2011; Schweizer *et al.* in press). Further research is needed to overcome some potential confounding effects as subjective values regarding the onset of dementia or socio-cultural differences between bilinguals and monolinguals.

Part II

Mind, brain, behavior

11

The role of experimental syntax in an integrated cognitive science of language

Jon Sprouse and Diogo Almeida

11.1 Introduction

Acceptability judgments form the primary empirical foundation for generative syntactic theories (Chomsky 1965; Schütze 1996). As such, the methodology of acceptability judgment collection has been a topic of research since the earliest days of generative syntax (e.g., Hill 1961, Spencer 1973). However, the past fifteen years have seen a dramatic increase in the number of articles devoted to the topic. It seems clear that the recent increase in interest in methodological issues is related to advances in technology that have made it easier than ever to construct, deploy, and analyze formal acceptability judgment experiments, which following Cowart (1997) have come to be called *experimental syntax* (a practice that we will follow in this chapter). The question at the center of this literature is deceptively simple: *How can formal acceptability judgment experiments help achieve the goals of generative syntax?* As we will see in this chapter, answering this question is surprisingly complex. A comprehensive answer to this question requires (at least) three components: (1) an explicit formulation of the goals of generative syntax, (2) an enumeration of the potential obstacles to those goals, and (3) an empirically driven evaluation of the ability of formal experiments to eliminate those obstacles. In this chapter we will present a comprehensive review of the recent acceptability judgment literature with respect to these three components in an attempt to provide (our version of) an answer to the question of how formal judgment experiments can help generative syntactic theory.

11.2 The goals and obstacles of generative syntactic theory

Our starting assumption is that all cognitively oriented language researchers share the goal of constructing a theory of language that integrates all three of Marr’s famous levels of analysis: the computational level, the algorithmic level, and the implementational level (Marr 1982; see also discussion in Phillips 1996, Phillips and Lewis 2010, Kluender 1991, Frazier 1978, Embick and Poeppel 2005, and many others). Marr (1982) used cash registers as an illustrative example to define these three levels for information processing devices (such as the human brain). The computational level of the theory is a description of the properties of the problem that must be solved by the device, as well as the operations that the device must perform, that abstracts away from the exigencies of actually solving the problem in practice. For a cash register, the computational level description is the theory of addition, with properties such as commutativity and associativity, abstracting away from the precise algorithms that are necessary to carry out addition. For the sentence level phenomena of language, syntactic theories are computational level descriptions, as they describe the properties of the final syntactic structures that must be built, as well as the properties of the structure building operations that are required to build them, but abstract away from the requirements of real-time sentence processing. The algorithmic level of the theory is a description of the actual operations that must be deployed to solve the problem (i.e. an algorithm). For a cash register this could be the base-10 addition algorithm that we learned in school: start from the right, and “carry over the ones.” For language, parsing theories are algorithmic level theories, as they describe the specific parsing operations that must be deployed during real-time sentence processing, including the strategies that dictate the deployment of those operations, and the ways in which parsing resources constrain the operation of the parser. Finally, the implementational level of the theory is a description of how the processes/strategies/resources are implemented in the hardware of the device. For a cash register, there are several hardware options that can influence this level (e.g., spinning drums versus electronic processors). However, for (a cognitive approach to) language, there is only one set of hardware, the human brain. Neurolinguistic theories, which seek to identify the cortical networks involved in various linguistic computations, are a first step toward implementational level descriptions (Embick and Poeppel 2005; Sprouse and Lau 2012).

There are at least two major obstacles to the construction of an integrated theory of language. The first is the black box problem: there is no method to directly measure cognitive mechanisms. What this means in practice is that researchers must (i) identify observable data types (behavior, electrophysiological responses, hemodynamic responses), and (ii) identify linking hypotheses that license (empirically valid) inferences

from the observable data to the unobservable cognitive mechanisms. The black box problem affects all three levels of the theory; however, in this chapter we will focus on the data and linking hypotheses underlying syntactic theory (see Sprouse and Lau 2012 for a description of the data types and linking hypotheses at the algorithmic and implementational levels). Crucially, the black box problem presents a framework for investigating the empirical contribution of experimental syntax by focusing the discussion on the following questions: *To what extent are the data underlying current incarnations of syntactic theory sound?* And, *What types of inferences are licensed by the linking hypothesis between acceptability judgment data and syntactic theory?*

Whereas the first major obstacle to the construction of an integrated theory of language, the black box problem, presents a framework for investigating the empirical contribution of experimental syntax, the second major obstacle presents a framework for understanding the historical and sociological context of recent investigations of experimental syntax. Even a cursory glance at the experimental syntax literature suggests that significantly more attention has been devoted to the question of the soundness of the data underlying syntactic theory than to the question of what inferences are licensed by the linking hypothesis between data and theory. As we will see in this chapter, we believe that this has been a distraction for the field, as there appears to be no evidence that the existing data are faulty, and growing evidence that traditional collection methods are appropriate for the majority of phenomena of interest to syntacticians. We believe that this distraction can be (at least partially) traced to the second major obstacle to the construction of an integrated theory of language: the difficulty in establishing linking hypotheses between the levels (computational, algorithmic, implementational) of the theory (see also Phillips 1996; Townsend and Bever 2001; Ferreira 2005).

Establishing a level-level linking hypothesis, for example between syntactic theories (computational) and parsing theories (algorithmic), requires the resolution of at least two complex theoretical issues. The first is to determine exactly how much of the sentence processing system should be captured by the syntactic theory; in other words, a line must be drawn that separates the aspects of the processing system that will be abstracted away from in building a syntactic theory, and the aspects of the system that will be directly captured by the syntactic theory. The second issue is to determine exactly what the linking hypothesis will be between the mechanisms in the syntactic theory and the mechanisms in the parsing theory. One early attempt at an integration of syntactic and parsing theories was the *Derivational Theory of Complexity* (DTC) (Miller 1962; McMahon 1963; Miller and McKean 1964; Gough 1965, 1966; for reviews see Fodor, Bever, and Garrett 1974; Berwick and Weinberg 1984, Pritchett and Whitman 1993; Phillips 1996, Townsend and Bever 2001). The DTC

assumed an early version of transformational syntactic theory that contained structure building operations (e.g., transformations), but abstracted away from other aspects of sentence processing such as meaning, parsing strategies, probabilistic information, etc. The DTC also assumed an isomorphic linking hypothesis between structure building operations in the syntactic theory and parsing operations in the parsing theory. Under this view, for every transformation that was necessary for a given sentence in the syntactic theory, there was a complementary process in the parsing theory to “un-do” the transformation during sentence comprehension. In this way, the DTC predicted that behavioral responses that tracked parsing difficulty (such as reaction times) would be directly affected by the number of transformations that were necessary to derive a given sentence in the syntactic theory, as each transformation would trigger complementary processes during sentence comprehension. As is well known, this prediction did not hold for many types of complex sentences.

The failure of the DTC as a linking hypothesis between syntactic and parsing theories continues to shape the interaction of syntacticians and psycholinguists, as there is some truth to the observation that each side of the computational/algorithmic divide internalized a different lesson from the failure. Though it is clear that the failure of the DTC was likely due to problems with all three components (the syntactic theory, the parsing theory, and the isomorphic linking hypothesis between the two; see Phillips 1996, Townsend and Bever 2001, Phillips and Lewis 2010), syntacticians tend to be more suspicious of the veracity of parsing theories, and psycholinguists tend to be more suspicious of the veracity of syntactic theories. This latter suspicion, coupled with a long tradition of formal experimentation in psycholinguistics, may be the cause of the increased attention given to the soundness of acceptability judgment data, as unsound data would obviously lead to unsound theories (Edelman and Christiansen 2003; Ferreira 2005; Gibson and Fedorenko 2010a,b). However, as will become clear in the next section, the soundness of acceptability judgment data does not appear to be a true impediment to an integrated theory (see also Phillips and Lasnik 2003; Phillips 2009; and Culicover and Jackendoff 2010); instead, the real impediment seems to be the complexity of the problem, as the space of possible syntactic theories, the space of possible parsing theories, and the space of possible linking hypotheses that can account for the data that we do have are still all relatively large.

11.3 To what extent are the acceptability judgments underlying syntactic theory sound?

Perhaps the most obvious target of criticism for researchers who are skeptical of syntactic theories is whether the judgments reported in any

given paper can be trusted to be a true reflection of the acceptability of the sentences in question. We will call this the *reliability* of judgment data. Establishing the reliability of judgment data is no easy task: the fundamental problem is that, unlike the properties of physical objects, there is no device that can objectively measure the properties of cognitive objects. Instead, cognitive scientists must rely on behavioral experiments to indirectly establish the quantity or quality of the cognitive objects in question. In the case of acceptability, the behavioral experiments in question actually ask the participants to report their judgment of acceptability; however, it should be clear that this report of acceptability is not necessarily the “true” acceptability response generated by the cognitive system of language. The process of establishing the reliability of judgment data is actually the process of establishing confidence that the reported values of acceptability accurately reflect the “true” acceptability response (see also Featherston 2007, Myers 2009a, and Schütze and Sprouse *in press*). The question then is how can experimental syntax help establish confidence in the acceptability judgments reported in the syntactic literature.

11.3.1 Criticisms of the reliability of syntactic data

To begin to see how experimental syntax can help to establish confidence in the data underlying syntactic theories, we can use recent criticisms of syntactic data as a roadmap. Perhaps the most well-known of recent criticisms is that of Gibson and Fedorenko (2010b). Gibson and Fedorenko argue that traditional data collection techniques have led to a preponderance of faulty data in the syntactic literature. As evidence for this, Gibson and Fedorenko discuss three phenomena that were originally reported using traditionally collected acceptability judgments. The first phenomenon is a preference for right-branching relative clauses over center-embedded relative clauses from Gibson (1991), as in (1):

- (1) a. *The man that the woman that the dog bit likes eats fish.
b. ?I saw the man that the woman that the dog bit likes.

The second phenomenon is the triple-wh amelioration of the Superiority effect reported by Kayne (1983):

- (2) a. *I'd like to know where who hid it.
b. ?I'd like to know where who hid what.

The third phenomenon is a comparison of two sentences involving the Superiority effect from Chomsky (1986a):

- (3) a. What do you wonder who saw?
b. *I wonder what who saw.

Gibson and Fedorenko (2010b) re-tested each of these contrasts using formal experiments, and report that all three failed to replicate (i.e. the

experiments detected no significant difference between the two conditions in each pair). From these results, Gibson and Fedorenko (2010b) conclude that the syntactic literature is rife with unreliable data, and that formal experiments are required to correct the situation.

There are at least two fundamental problems with the Gibson and Fedorenko (2010b) studies that we can use to create a roadmap for applying experimental syntax to the question of reliability in syntactic data. The first problem is that we don't know how representative these three phenomena are of the data in the field as a whole. It could be the case that these are three examples from a large set of replication failures in the literature; or it could be that these are three examples from a small set of replication failures. The problem is that these three were chosen with bias (i.e. because they are replication failures). An unbiased test of the replication failure rate in syntax would either test the entire set of data points in the field (as in Sprouse and Almeida (2012), discussed in Section 11.3.2), or test a truly random sample of data points from the entire set (as in Sprouse, Schütze, and Almeida (submitted), discussed in Section 11.3.3). One could then compare the number of replication failures to the number of replications to derive a replication failure rate for the field as a whole, and ask whether that rate is substantially higher than the rate in other domains of experimental psychology. Without such comprehensive tests, the Gibson and Fedorenko (2010b) examples are not very informative.

The second problem is that Gibson and Fedorenko (2010b) assume that when traditional methods and formal experiments yield conflicting results, as is the case with their three case studies, we should accept the formal experimental results as "true." This is in many respects begging the question: if the goal of the study is to determine which method is more reliable, then one can't simply assume that one method is *a priori* more reliable. To illustrate this problem, Sprouse and Almeida (in press) re-tested the Gibson and Fedorenko (2010b) phenomena using a more powerful judgment task (the Forced-Choice task, see Section 11.3.4), and found that two of the three phenomena do show significant differences identical to those originally reported in the literature: 62 of 98 respondents favored the right-branching structure (1b) from Gibson (1991), $p=.006$ by sign test, and 58 of 98 respondents favored the triple-wh construction (2b) from Kayne (1983), $p=.04$ by sign test. In other words, two of the three replication failures reported in Gibson and Fedorenko (2010b) may not be replication failures at all, but instead may be examples of false negatives that arose due to insufficient statistical power in the reported experiments. This suggests that a systematic investigation of the relative statistical power of traditional methods and formal experiments is necessary to determine under what conditions each experiment type should be considered an appropriate tool for assessing acceptability (see Section 11.3.4).

11.3.2 The reliability of textbook data

Given the problems raised by biased selection of phenomena in Gibson and Fedorenko (2010b), Sprouse and Almeida (2012) set out to provide a more accurate estimate of the reliability of data in syntax. They tested all 469 US-English data points from an introductory syntax textbook (Adger 2003) in formal experiments using 440 naïve participants, the magnitude estimation (Stevens 1957; Bard *et al.* 1996) and yes-no tasks, and three different types of statistical analyses (traditional frequentist tests, linear mixed effects models (Baayen *et al.* 2008), and Bayes factor analyses (Rouder *et al.* 2009)). The results of that study suggest that at least 98% of the data points in Adger (2003) replicate using formal experiments. Even following the assumption of Gibson and Fedorenko (2010b) that formal experiments provide the “true” results, this means that the maximum replication failure rate of the traditionally collected judgments from Adger (2003) is only 2%.

11.3.3 The reliability of journal data

Although the replication rate for judgments in Adger’s (2003) introductory textbook is impressive (at least 98%), it is logically possible that the replication rate for judgments in journal articles could be substantially lower. To test this possibility, Sprouse, Schütze, and Almeida (submitted) identified every (acceptability-judgment-based) data point published in the journal *Linguistic Inquiry* from 2001 to 2010, for a total of 1743 data points. They then randomly sampled 292 data points (forming 146 pairwise phenomena), or about 17% of the full set of data points in *Linguistic Inquiry* 2001–2010. They then tested these 146 phenomena in formal experiments to estimate a replication rate for data points from *Linguistic Inquiry* 2001–2010. They found that 95% of the sampled phenomena replicated using formal experiments. Based on the size of the sample in relation to the full set of data points, this suggests that *Linguistic Inquiry* 2001–2010 has a minimum replication rate of $95\% \pm 5$. Taken together with the textbook replication from Sprouse and Almeida (2012), these results suggest that there is no evidence of a reliability problem in the syntax literature, and that the concerns raised by Gibson and Fedorenko (2010b) were empirically unfounded.

11.3.4 A comparison of the statistical power of traditional methods and formal experiments

Given the role that statistical power played in the interpretation of the Gibson and Fedorenko (2010b) results (two of the three case studies were false negatives due to low statistical power, see Sprouse and Almeida (submitted)), the next logical step is to compare the statistical power of traditional methods and formal experiments. Intuitively speaking,

statistical power is the ability of an experiment to detect a difference between conditions when in fact there is a true difference. Statistical power is often expressed as a percentage: for example, 80% statistical power would indicate that an experiment would detect a true difference 80% of the time. Cohen (1962, 1988, 1992) has suggested that well-powered experiments in psychology should strive for 80%, although in practice most experiments in psychology are closer to 60% (Clark-Carter 1997). Many different factors influence the statistical power of an experiment, from the task chosen, to the size of the difference to be detected, to the number of participants in the experimental sample. As such any study interested in assessing the statistical power of acceptability judgment experiments must manipulate all of these factors to arrive at a comprehensive picture.

Sprouse and Almeida (submitted a) conducted just such a study in an effort to directly compare the statistical power of traditional methods and formal experiments. They tested 95 phenomena taken from Adger (2003) and *Linguistic Inquiry* 2001–2010. These 95 phenomena span the full range of effect sizes in syntactic data, allowing for a comparison of statistical power at every possible effect size, from very small differences to very large differences. In order to compare the power of traditional methods and formal experiments, the phenomena were tested using two different tasks: magnitude estimation, which is commonly used in formal experiments, and forced-choice, which is commonly used in traditional methods. Over 140 participants were tested using each task, and then resampling simulations were used to empirically estimate the statistical power for each phenomenon at every possible sample size between 5 and 100 participants. Figure 11.1 below presents the results of these resampling

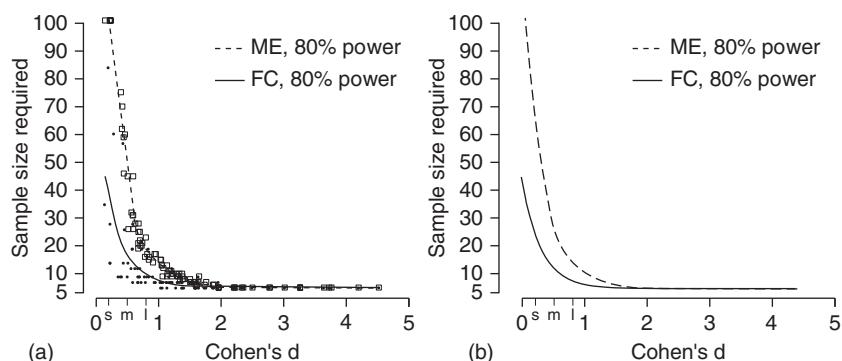


Figure 11.1 (a)(b) The sample size required (y-axis) to reach 80% power for the combined set of 95 effect sizes (x-axis) for both forced-choice (solid dots) and magnitude estimation (empty squares) experiments. The solid line is a non-linear trend line for the forced-choice results. The dashed line represents a non-linear trend line for the magnitude estimation results. The criteria for small, medium, and large effect sizes (following Cohen 1988, 1992) are indicated on the x-axis in a smaller font. The left panel includes both points and trend lines; the right panel presents the trend lines in isolation.

simulations by plotting the sample size required to reach 80% power (along the y-axis) for each effect size (the x-axis).

Contrary to the claims of critics, the results of the Sprouse and Almeida (submitted a) study suggest that traditional methods may be more powerful than formal experimental methods, at least with respect to detecting difference between conditions, as traditional methods require substantially fewer participants to reach 80% power (i.e. the suggested power level in experimental psychology; see Cohen 1988, 1992). Sprouse and Almeida (submitted a) also present a discussion of these results in relation to the distribution of effect sizes in syntactic theory (based on the *Linguistic Inquiry* sample from Sprouse, Schütze, and Almeida (submitted)), arguing that the results suggest that the phenomena of interest to syntacticians tend to be substantially larger than those of interest to other areas of psychology (nearly 90% of phenomena are large enough to be visible to the “naked eye” according to the metrics of Cohen 1992), and that traditional methods will lead to at least 94.5% power for the mean effect size (as compared to 59% power for the mean effect size in other areas of psychology, Clark-Carter 1997). In other words, traditional methods should be seen as a valid, reliable, and well-powered set of methods for the investigation of the phenomena of interest to syntacticians.

11.3.5 Cognitive bias

Perhaps one of the most contentious aspects of traditional judgment collection is the use of professional linguists as participants. Several critics of traditional methods have suggested that this introduces the logical possibility of cognitive bias on the part of the participants: as professional linguists, the participants will likely be aware of the theoretical consequences of their judgments, and this awareness may impact the judgments that they ultimately report (Edelman and Christiansen 2003; Ferreira 2005; Wasow and Arnold 2005; Gibson and Fedorenko 2010a,b). Supporters of traditional methods counter this possibility with two logical arguments. First, acceptability judgment experiments are easily replicable, as they require no special equipment. This means that any given data point can be replicated on the spot: audiences at conferences, reviewers of articles, and even the readership of journals can quickly and easily check the reported judgments for accuracy, and thus identify any influence of cognitive bias. Second, the theoretical awareness of professional linguists may provide a type of expert knowledge that increases the reliability, and possibly the sensitivity, of linguists’ judgments over non-linguists’ judgments (Newmeyer 1983, 2007, as well as Fanselow 2007, Grewendorf 2007, and Haider 2007 for possible examples in German, and Devitt 2006, 2010, Culbertson and Gross 2009, Gross and Culbertson 2011 for a discussion of what could be meant by “expert knowledge”). The empirical question then

is whether there is any evidence of cognitive bias in the judgments of professional linguists.

There are at least two methods for assessing the role of cognitive bias in syntactic theory. One method would be to compare the judgments of linguists with differing theoretical dispositions to see if theoretical knowledge is indeed affecting their judgments. To our knowledge, the only study to directly compare judgments from linguists with differing theoretical dispositions is Dabrowska (2010). Dabrowska compared the judgments of self-identified generative linguists with self-identified functional linguists in a rating study that included Complex NP islands (**What did John make the claim that Mary bought?*). One plausible prediction of the cognitive bias hypothesis is that generative linguists would rate the examples of island violations lower than the functional linguists because island constraints are a core part of the generative theory of syntax, whereas several functional linguists have argued that island effects are an epiphenomenon of language use (e.g., Kuno 1973; Deane 1991; Kluender and Kutas 1993; Goldberg 2007). In other words, generative linguists have a motivation to confirm the reliability of Complex NP islands, whereas functional linguists have a motivation to disconfirm the reliability of Complex NP islands. Dabrowska (2010) actually found that generative linguists' ratings of Complex NP islands were higher (i.e. more acceptable) than the functional linguists' ratings, which, contrary to the cognitive bias hypothesis, suggests that the generative linguists were actually biased *against* their own theoretical interests.¹

The second method is to compare linguists' judgments with the judgments of naïve participants. This method is necessarily more complicated, as a simple difference between two groups is never enough to establish which is correct, and as we've already seen, there are real statistical power differences between the two methods. One plausible prediction of the cognitive bias hypothesis would be that linguists' judgments would go in the opposite direction than naïve participants' when it is theoretically advantageous to do so. If this prediction holds, then large-scale comparisons of the two groups, such as the studies by Sprouse and Almeida (2012) and Sprouse, Schütze, and Almeida (submitted), should reveal a large number of sign-reversals – that is, instances where the direction of the difference between two conditions reported by one group is the exact opposite of the direction of the difference reported by the other group. Out of the 250 pairwise phenomena investigated by these studies, only two sign-reversals were observed (less than 1% of cases), suggesting that cognitive bias has not had an influence on existing syntactic data.

11.3.6 The interpretation of variation across participants

Another issue that arises in the critical literature (e.g., Wasow and Arnold 2005, Gibson and Fedorenko 2010b), but often goes unexamined, is the

question of how to interpret the variability in acceptability judgments across participants. To make this discussion concrete, imagine that a researcher is interested in the difference between two sentence types, as is typical in acceptability judgment experiments. In general statistics terminology, this difference is the *treatment effect*. When investigating the treatment effect, the researcher can ask if the sample as a whole shows a treatment effect by comparing the mean response of the sample for each condition. If the two means are different enough, traditional statistical significance testing (SST) will report that there is a significant treatment effect for the sample. However, finding a statistically significant treatment effect for the sample does not mean that every participant demonstrated the treatment effect. In practice, given sufficient statistical power, very few participants need to show the treatment effect in order for the sample to show a significant treatment effect. A recurring question in the experimental syntax literature is what to make of this variability. If 100% of the participants show the treatment effect, it is pretty clear that the effect is a robust fact for all of the members of the sample. However, what if 75% show the effect, and 25% do not? What if only 25% show the effect, and 75% do not?

There seem to be three different approaches to this question in the literature:

1. Since measurement involves noise, only the central tendency of the sample matters, and it is expected that not every participant or every item in the sample show the treatment effect.
2. If a large enough proportion of participants do not show the predicted treatment effect, this might be evidence for a different dialect.
3. Given a strong theory-data linking hypothesis that ungrammatical sentences should be overwhelmingly judged to be unacceptable, a large enough proportion of participants that fail to show the predicted treatment effect, or that judge supposedly ungrammatical sentences no worse than awkward will be taken as evidence that the theoretical prediction is disconfirmed.

The first approach assumes that the participants who do not show the treatment effect are simply being influenced by random noise. This is the default assumption in most domains of cognitive science, as it is assumed that all behavioral responses are the result of a combination of the experimentally manipulated behavior, and various sources of random noise (sometimes called unsystematic variation). Under this approach, it only matters whether the sample as a whole shows the treatment effect: if SST reveals a treatment effect in the sample, then there is a real treatment effect. This is by far the most common approach in the experimental syntax literature, as many of the best practices of experimental syntax, including the use of SST, have been directly adapted from experimental psychology. A second approach is to investigate whether participants who

do not show the treatment effect are actually drawn from a different population than the participants who do show the effect. In most domains of cognitive science, the population of interest is all humans; in linguistics, the population of interest is all speakers of a given language. It is always a logical possibility that the participants who do not show an effect have a different grammar than the speakers who do show the effect (see also den Dikken *et al.* 2007). A third approach is to assume that only manipulations that yield a treatment effect in (almost) 100% of participants are real. While this is certainly a strong criterion to impose on experimental results, it is not without a certain logic. It is common in the syntactic literature to talk about *possible* sentences and *impossible* sentences. If one truly believes that a given sentence is impossible in a certain language, then one could also conclude that no amount of random noise should be enough to cause participants to rate that sentence as acceptable (see also Hoji 2010). This approach nonetheless makes several assumptions: (i) that acceptability judgments directly reflect the grammaticality of the sentences, without contamination from other cognitive systems, (ii) that fatigue and distraction do not affect judgments, (iii) that the crucial analysis is categorical (sentence A is acceptable or unacceptable), as opposed to relative differences (A is better or worse than B), and (iv) that the empirical domain of syntactic theory is only the difference between possible and impossible sentences.

Because of the domain-specific issues related to syntactic theory (e.g., language variation, possible/impossible sentences), it is critical to keep these three approaches to variation in mind when interpreting the results of formal acceptability judgment experiments. Failure to do so can lead to substantially different interpretations of the data. For example, Langendoen *et al.* (1973) investigated the claim made by Fillmore (1965) and others that the first object of a double-object construction cannot be questioned:

- (4) *Who did you buy a hat?
(cf. What did you buy Mary?)

Langendoen *et al.* performed an answer completion task to test this claim formally. They asked 109 students to answer questions like (5) with a complete sentence:

- (5) Who did you show the woman?

Their hypothesis was that if questions like (4) are indeed unacceptable, then the answers to (5) should consistently place the answer NP at the end of the sentence (*I showed the woman my daughter*). Langendoen *et al.* reported two findings: that one-fifth of the participants responded with the NP in the first object position (*I showed my daughter the woman*) and these participants were all from the metropolitan New York City area. Langendoen *et al.* (1973) considered following approach two,

i.e., concluding that there are two dialects at work in the sample: speakers from NYC, who can question first object, and everyone else, who cannot. Their favored conclusion, however, was more nuanced. Noticing the theoretical difficulty in incorporating the necessary restrictions in the grammar of English to explicitly rule out questions from the first object of a double-object construction (a point previously raised by Jackendoff and Culicover, 1971), Langendoen *et al.* (1973) proposed that these constructions are in fact licensed by the grammar of English. The difference between the population that finds them acceptable and the population that does not, they argue, is due to a different parsing strategy employed by the two groups. Taking a different perspective, Wasow and Arnold (2005) and Gibson and Fedorenko (2010a, b) have interpreted Langendoen *et al.*'s (1973) result under approach three, and concluded that Fillmore's (1965) original claim is incorrect: it is, in fact, possible to question first objects. Of course, it is also possible to assume approach one: the one-fifth of participants who created first object answers did so because of random noise in the experiment. This means that 87/109 participants responded in accordance with Fillmore's (1965) claim. A one-tailed sign test yields $p = .000000018$ – a significant result. What should be obvious here is that the problem is not with the data itself, since no experimental result disputed the fact that, by and large, speakers of English found questions constructed from the first object of a double-object construction to be unacceptable. The problem is with the interpretation of what these results might mean for the theory of grammar: Langendoen *et al.*'s (1973) favored interpretation was motivated first and foremost by theory-internal considerations, while Wasow and Arnold's (2005) and Gibson and Fedorenko's (2010a, b) conclusions were dictated by their data-theory linking hypothesis.

A similar situation arises with Wasow and Arnold's (2005) test of a claim from Chomsky (1955/1975) that the complexity of a noun phrase strongly determines the position of that noun phrase within a verb-particle construction. Chomsky's claim is twofold. First, he claims that the most natural place for multi-word NPs is after the particle, therefore both (6a) and (6b) below should be more acceptable than both (6c) and (6d). Second, he claims that complex NPs (relative clauses) are less acceptable than simple NPs when they occur between the verb and particle, therefore (6d) should be less acceptable than (6c).

- (6)
 - a. The children took in all our instructions. [3.4 out of 4]
 - b. The children took in everything we said. [3.3 out of 4]
 - c. The children took all our instructions in. [2.8 out of 4]
 - d. The children took everything we said in. [1.8 out of 4]

Wasow and Arnold (2005) ran a formal rating experiment, the results of which are in square brackets in (6). According to approach one, which assumes that only a difference in means is necessary to verify a claim, the

formal results match Chomsky's informal results perfectly: there is a significant interaction between particle position and NP type ($p < .001$). However, Wasow and Arnold (2005: 1491) interpret the results as problematic because "17% of the responses to such sentences [d.] were scores of 3 or 4." It seems that Wasow and Arnold (2005) were assuming approach three, which requires that sentences be judged unacceptable close to 100% of the time if we are to accept their status as unacceptable.

It is crucial for the language community to be explicit about their assumptions regarding the variability of acceptability judgments moving forward. As we have already seen, several high profile criticisms of informal experiments rest upon the assumption that there should be little or no variability among participants (Wasow and Arnold 2005; Gibson and Fedorenko 2010b), but it is important to notice that when this very strong assumption about the relationship between the theory and the data is relaxed (for instance, to allow for things like sampling error), the exact same set of results can be seen as providing strong evidence for the opposite conclusion (see also Labov 1996 for a similar discussion of the *wanna* contraction, and Raaijmakers 2003 for a similar discussion of the interpretation of variation across participants in the sentence processing literature).

11.3.7 The relative costs and benefits of traditional methods and formal experiments

In this section we have seen that experimental syntax techniques provide a useful toolkit for exploring precisely which properties of formal experiments should increase our confidence in the veracity of the results (i.e. whether there is indeed a true difference between conditions). Though it is relatively common to assume that formal experiments provide "better" results than informal results, the current state of the field suggests that many of the perceived benefits of formal experiments ultimately disappear under closer empirical scrutiny. This raises the very real possibility that the problem facing acceptability judgment data is a sociological one, not an empirical one: researchers who are accustomed to formal experiments are disinclined to have confidence in the results of traditional methods, regardless of whether the informal experiments are empirically appropriate for the research questions that they are intended to address.

Choosing the appropriate methodology (in any field) requires the researcher to balance the costs and benefits of different methodologies relative to their specific research question. The benefits of the traditional methods over formal experiments are well known: (i) traditional methods are cheaper – formal experiments cost \$2.20–\$3.30 per participant on AMT; (ii) traditional methods are faster, at least with respect to participant

recruitment – although AMT has diminished this advantage significantly (e.g., Sprouse 2011a reports a recruitment rate of 80 participants per hour on AMT); (iii) the tasks used in traditional methods, such as the forced-choice, appear to be more powerful than the tasks used in formal experiments, such as magnitude estimation; and (iv) this increased statistical power often makes traditional experiments the only option for languages with few speakers (Culicover and Jackendoff 2010) or for studies of variation between individuals (den Dikken *et al.* 2007). On the other hand, the benefits of formal experiments typically revolve around the types of information that are necessary to answer the research question of interest (see also Section 11.4). For example, the numerical rating tasks typically used in formal experiments provide more information than the forced-choice and yes-no tasks used in traditional methods, such as the size of the difference between conditions (see also Schütze and Sprouse in press, though as Myers 2009b points out, non-numerical tasks can be used to approximate size measurements if necessary). Furthermore, if one wishes to construct a complete theory of the gradient nature of acceptability judgments, an enterprise which has gained in popularity over the past decade (e.g., Keller 2000; Featherston 2005) then one will clearly need numerical ratings of acceptability. The bottom line is that there is no single correct answer when it comes to choosing a methodology. Syntacticians (and indeed all researchers) must be aware of the relative costs and benefits of each methodology with respect to their research questions, and be allowed to make the decision for themselves. Science cannot be reduced to a simple recipe.

11.4 What types of inferences are licensed by the linking hypothesis between acceptability judgments and syntactic theory?

The majority of experimental syntax studies have focused on the reliability of the data underlying syntactic theory. As the previous section made clear, we believe that this has been a (necessary) distraction: there appears to be no evidence that the existing data is faulty, and growing evidence that the informal methods are appropriate for the majority of phenomena of interest to syntacticians. However, there is reason to believe that experimental syntax techniques also provide new tools to investigate the inferences licensed by the linking hypothesis between acceptability judgments and syntactic theory. In this section we will review two ways in which experimental syntax has added to our understanding of the nature of syntactic theory: (i) testing reductionist claims about the correct locus of acceptability judgment effects (so-called “processing” explanations), and (ii) examining the complex theoretical issues surrounding the interpretation of continuous acceptability judgments.

11.4.1 Reductionist approaches to acceptability judgment effects

The fundamental component of the linking hypothesis between judgment data and syntactic theories is the assumption that manipulations of the structural properties of a sentence will lead to modulations of acceptability. Regular readers of the syntactic literature are aware that it is relatively common for syntacticians to establish the structural nature of acceptability differences; by holding non-syntactic factors constant (semantics/plausibility, phonetics/phonology, morphology/lexical properties), syntacticians can be relatively certain that it is the structural manipulation that is driving the effect. However, because acceptability judgments are the result of successful sentence processing, and because the operation of the parser is (by definition) intricately tied to structural properties of sentences, there is always a possibility that acceptability effects may be driven by properties of the parsing system rather than grammaticality per se (cf. the conclusion reached by Langendoen *et al.* 1973 for the questions based on the double dative construction mentioned in the previous section). The question then is how experimental syntax techniques can help tease apart acceptability differences due to grammaticality effects, and acceptability differences due to properties of the parsing system.

The first step in teasing apart this ambiguity is to be clear about what is meant when one suggests that acceptability differences are driven by properties of the parsing system. These types of accounts are sometimes called “processing explanations” to contrast with “syntactic explanations,” but as several researchers have remarked, this label is less than ideal (Phillips 2011; Sprouse *et al.* 2012). The problem with this label is that, by definition, structural manipulations will result in different behavior by the parser. This is precisely what we want: the theory of syntax (a computational theory of structural properties of sentences) should be closely related (by a level-level linking hypothesis) to the theory of parsing (an algorithmic theory of syntactic structure building). Viewed from this perspective, every “syntactic explanation” is a “processing explanation,” as the syntactic theory is a form of abstraction or idealization of the structure-building component of the parser. Because of this tight relationship between the syntactic theory and the structure-building component of the parser, the syntactic properties of a sentence will necessarily affect the behavior of the syntactic structure-building component of the parser. This means that in order for the so-called “processing explanations” to be distinct from “syntactic explanations,” the “processing explanation” must not be related to the syntactic structure-building component of the parser.

To clarify the content of these types of questions, Phillips (2011) and Sprouse *et al.* (2012) suggest the term *reductionist* instead of “processing explanation.” They argue that the logic of these types of explanations is clearly reductionist, in that the acceptability effect is argued to be reducible to non-structure-building components of the parser, such as parsing

strategies or parsing resource capacity. Under a reductionist approach, the relationship between the acceptability effect and the structural manipulation that is normally assumed to be driven by the syntactic system is actually epiphenomenal; the true causal nexus lies between the extra-syntactic factors, such as parsing strategies or parsing resources, and the acceptability effect. The second-order correlation between acceptability and the structural manipulation arises because structural manipulations necessarily affect parsing strategies or parsing resource allocations. In this way, the complexity of the syntactic system is reduced in favor of extra-syntactic components. To the extent that these extra-syntactic components are independently necessary, *reductionist explanations* may be preferred to syntactic explanations according to theory-building metrics such as Occam's razor.

As a concrete example, take the two island effects that we have discussed previously: Whether islands and Complex NP islands (see also Alexopoulou and Keller 2007, Sprouse 2008, and Sprouse *et al.* 2011 for other examples of the parsing system affecting acceptability judgments). The standard analysis within the syntactic literature is that these island effects are rated unacceptable by native speakers because there is a syntactic constraint, such as the Subjacency Condition, that rules these structures out as ungrammatical. However, several researchers have proposed alternative explanations that do not involve syntactic constraints at all, but rather potentially independently motivated properties of the parsing system such as working memory (Kluender and Kutas 1993; Kluender 1998, 2004; Hofmeister and Sag 2010), attention (Deane 1991), and focus (Erteschik-Shir 1973; Goldberg 2007). For example, Kluender and Kutas (1993) argue that island violations such as (2b) and (3b) are in fact grammatical structures, but that the unacceptability reported by speakers is in fact the result of a combination of two relatively resource-intensive processes that are necessary to successfully parse the sentences. These two processes require more resources than are available to the parsing system, and therefore cause the parsing system to fail to successfully parse the sentences, resulting in the perception of unacceptability.

Kluender and Kutas (1993, see also Kluender 1998, 2004) are very explicit about the two processes that they believe are the cause of the unacceptability, and about the resources in question. They argue that the first process is the maintenance of a displaced wh-word in working memory during the processing of the sentence between the wh-word and the downstream gap site. The second process is the construction of the island structure itself, which as can be seen in (2b) and (3b) involves a CP clause that is in some ways more complex than CPs headed by *that*. Kluender and Kutas (1993) argue that each of these processes requires a certain amount of working memory resources to be deployed. Although each of these processes can be deployed in isolation, when deployed simultaneously, the combined resource requirements are greater than the pool of available

Table 11.1 Independent manipulation of dependency length and island structures

Length	Structure	Example
short	non-island	Who – thinks that John bought a car?
long	non-island	What do you think that John bought –?
short	island	Who – wonders whether John bought a car?
long	island	What do you wonder whether John bought –?

resources. At this point it should be clear that this reductionist theory defines island effects as a *psychological interaction* of two (sets of) parsing processes that occurs because the processes rely upon a single pool of resources. Sprouse *et al.* (2012) suggest that this psychological interaction can be translated into a *statistical interaction* between two factors, each with two levels: LENGTH (short, long) and STRUCTURE (non-island, island) (see also Myers 2009b for a discussion of the use of factorial designs in syntax). The factor LENGTH manipulates the length of the wh-dependency at two levels: within a bi-clausal constituent question, a short dependency is created by extraction of the matrix subject; a long dependency is created by extraction of the object argument in the embedded clause. The factor STRUCTURE refers to the STRUCTURE of the embedded clause (Table 11.1).

Defining island effects in this way has several advantages. First, it allows us to isolate the effect of each of the individual factors on continuous acceptability ratings. For example, the effect of processing long-distance wh-dependencies can be seen by comparing the short, non-island condition to the long, non-island condition, and the effect of processing island structures can be seen by comparing the short, non-island condition to the short, island condition. Second, it allows us to quantify the statistical interaction of the two factors. If there were no statistical interaction between the two factors (i.e. if the two sets of processes impact acceptability ratings independently), we would expect a graph like that in Figure 11.2(a). Figure 11.2(a) is an example of simple linear additivity between each factor in which the cost of each process leads to a decrement in acceptability ratings, and in which each cost sums linearly with respect to the short/non-island condition. This linear additivity in decrements leads to two parallel lines. However, if there were an interaction between the two factors, we would expect a graph like that in Figure 11.2(b): super-additivity when the *long* and *island* levels of the two factors are combined, leading to non-parallel lines. (The hypothetical ratings in Figure 11.2 are displayed in terms of standardized z-scores, which can be derived from any approximately continuous rating measure, such as Likert scales or magnitude estimation.)

Figure 11.2(b) is in fact the pattern that is consistently observed when the factors LENGTH and STRUCTURE are independently manipulated in acceptability experiments, although there is variation in the size of the

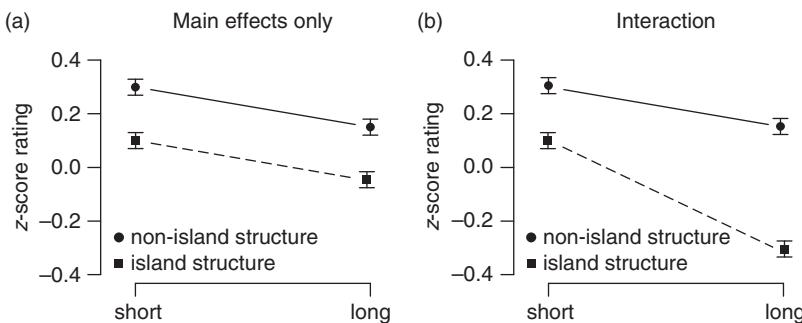


Figure 11.2 Example results for main effects (a) and interaction (b).

effect of island structures alone (Kluender and Kutas 1993; Sprouse 2007a; Sprouse *et al.* 2012). In this way, island effects can be defined as a statistical interaction between two structural factors, exemplified by a super-additive decrease in acceptability in the long, island condition.

Sprouse *et al.* (2012) used this interaction-based definition to test the role of working memory resources in the acceptability of island effects. They argued that the Kluender and Kutas (1993) analysis would predict an inverse relationship between working memory capacity in individuals and the strength of the super-additive interaction because the super-additive interaction (as opposed to linear additivity) arises due to insufficient working memory capacity. Therefore one would expect participants with higher working memory capacity to have smaller interactions, and participants with lower working memory capacity to have larger interactions. Sprouse *et al.* (2012) tested over 300 participants with two different working memory tasks and two different acceptability judgment tasks (Likert scales and magnitude estimation), and found no relationship between working memory capacity and the size of the super-additive interaction. From these results, they conclude that it is unlikely that working memory capacity is driving the unacceptability of island effects. In this way, a combination of factorial designs, numerical acceptability judgment tasks, and parsing resource tests (working memory tests, etc.) can be used to investigate the probability of reductionist versus grammatical explanations for acceptability effects, and help establish the data-theory linking hypothesis between acceptability judgments and syntactic theory.

Though this is just one example of using experimental syntax to investigate a reductionist explanation for acceptability effects, it does suggest a general methodology for future studies. The first step is to identify a set of (non-structure-building) parsing-related factors that are hypothesized to drive the effect. The simplest possible reductionist explanation is one in which the acceptability effect of each of the factors will sum (linearly) to the full effect (e.g., the lack of interaction in Figure 11.2(a)). If the factors do not sum (linearly) to the full effect (e.g., the super-additive interaction in Figure 11.2(b)), then there must be an explanation for the super-additivity. In

the case of island effects, the super-additive interaction was explained by the assumption that the two processes draw on the same limited pool of working memory resources. The explanation for the super-additivity can then be tested by searching for correlations between the strength of the interaction and the relevant parsing properties (e.g., working memory capacity).

11.4.2 Gradient acceptability and the nature of syntactic theory

Although it is possible to categorize sentences as either acceptable or unacceptable in qualitative tasks such as the yes-no task, the results of numerical judgment tasks such as magnitude estimation have suggested that acceptability is better described by a continuous scale, with sentence types taking values at any point along the scale. The fact that acceptability is a continuous measure has led several researchers to investigate to what extent the nature of the grammar itself may be continuous (Keller 2000; Sorace and Keller 2005; Featherston 2005; Bresnan 2007). It is not uncommon to encounter those who believe continuous acceptability necessitates a continuous (or gradient) syntactic system. However, there is no necessary link between the nature of acceptability and the nature of the syntactic system. The question in fact hinges on (at least) three complex theoretical issues: (i) What is the relationship between acceptability judgment data and syntactic theories? (ii) What is the correct level of abstraction for a computational theory? (iii) What is the continuous syntactic property that could give rise to continuous acceptability?

Up to this point, the discussion of the relationship between acceptability judgment data and syntactic theories has been one of inference from data to theory: when certain extra-syntactic factors (such as parsing strategies, parsing resources, and issues related to acceptability judgment tasks themselves) are held constant, modulations in acceptability judgments are interpreted (via a linking hypothesis) as evidence about the properties of the syntactic system. However, it is also possible to reverse the direction of the relationship and investigate how well the syntactic theory predicts the acceptability judgment data. While this approach is more common in the computational modeling literature, where the term *generative* is used to refer to models that can be used to generate the observable data, it seems clear that the question of how best to account for the continuous nature of acceptability judgments is a predictive/generative question. This can be seen in the structure of a common argument that syntactic theories should be gradient: (i) acceptability is continuous, (ii) categorical grammars predict categorical acceptability, (iii) gradient grammars predict continuous acceptability, (iv) therefore the grammar must be gradient. Crucially, this style of argument assumes that the syntactic theory is the correct locus for the mechanisms that lead to continuous acceptability. It is clear that this is not a logical necessity: all of the extra-syntactic components of the language faculty that must be controlled in order to make careful inference

from acceptability data to syntactic theory (such as parsing strategies, parsing resources, and the conscious mechanisms that underlie judgment tasks) may be contributing to the gradience in the acceptability judgment data, and therefore are potential sources of the mechanisms that generate continuous acceptability. In short, once one adopts a predictive/generative approach to modeling acceptability judgment data, the question is to what extent should the continuous mechanisms be part of the syntactic theory, and to what extent should the continuous mechanisms be part of the extra-syntactic components of the language faculty.

In order to determine to what extent the syntactic theory should predict continuous acceptability, we must be explicit about what a syntactic theory is a theory of, and which aspects of that theory can give rise to continuous acceptability. In [Section 11.1](#), we presented a view of syntactic theory as a computational level description of a part of the human language faculty. In other words, syntactic theory is a formulation of the properties of the syntactic structure-building mechanisms of the human parser that abstracts away from parsing strategies, parsing resources, and other issues that are specific to the real-time implementation of parsing algorithms. From this perspective, there are only two options for including gradient mechanisms within the syntactic theory. The first option is to actually abstract away from the algorithmic level less, such that one or more of the gradient mechanisms of the algorithmic level actually exists in the computational level description. Bresnan (2007) has advocated a probabilistic approach to the syntax of the dative construction in English that may be an example of this sort of “weaker” abstraction, as the syntactic theory appears to include information about the probability of the dative construction under various morphosyntactic, semantic, and information-structure environments – information that has previously been of primary interest to algorithmic level sentence processing theories (see also Bresnan and Hay 2008, Bresnan and Ford 2010).

The second option is to include an additional property in the syntactic theory that can capture gradience. Once again, this is a complex ontological issue, as any property that is included in the syntactic (computational) theory must be mapped to the actual language faculty as it is implemented in the human brain (i.e. there is a *mentalistic commitment*). This leads to a stark contrast between syntactic theories in which *grammaticality* is a purely theoretical construct, and syntactic theories in which *grammaticality* is a mentalistic construct. In the former, *grammaticality* is not a property that is available to the mental system, but rather a label that theoreticians can apply to sentences rather than using the non-technical terms “possible/impossible” (e.g., Chomsky 1957). In the latter, *grammaticality* is a property that is available to the mental system in some form, such as when different syntactic constraints are assumed to lead to different levels of unacceptability (e.g., Huang 1982; Chomsky 1986a), or when structures are assumed to be marked as ungrammatical (e.g., the star feature in Chomsky 1972a). As

Keller (2000) demonstrates for Optimality Theory, and Featherston (2005) demonstrates for the Decathlon model, syntactic theories that assume that grammaticality is a mentalistic construct can be used to directly predict continuous acceptability without altering the level of abstraction in the computational/algorithmic divide; however, the cost is assuming that grammaticality is a mentalistic construct rather than simply a theoretical one.

On the one hand, it is clear that experimental syntax techniques, especially numerical tasks like magnitude estimation and Likert scales, yield a new form of continuous acceptability data that provide researchers with the opportunity to reverse the normal direction of data-theory inference, and construct predictive/generative syntactic theories. On the other hand, the discussion in this subsection suggests that the interesting questions raised by this approach are not data-driven questions. In other words, the data enable this line of questioning, but the data don't determine the answer (see also Sprouse 2007b). The questions raised in this section (such as: What is the right level of abstraction for a computational theory? and Should syntactic theories include a gradient, mentalistic property called grammaticality?) are theoretical questions. And like all theoretical questions, they can only be answered through careful comparison of the empirical adequacy of competing theories.

11.5 Conclusion

Our goal in this chapter was to review the role of experimental syntax in the construction of an integrated cognitive science of language. While this role is undoubtedly still evolving, it seems clear that experimental syntax is well positioned to make substantial contributions to two questions that are central to the integration of syntactic theories with parsing theories: *Is the data underlying existing syntactic theories sound?* and, *What types of inference are licensed by the linking hypothesis between acceptability judgments and syntactic theories?* Although both questions are scientifically relevant to the theory, the current state of evidence suggests that questions about the reliability of existing judgment data may have been a (historically driven) distraction: there appears to be no evidence that the existing data is faulty, and growing evidence that the traditional methods are appropriate for the majority of phenomena of interest to syntacticians. This suggests that the contribution of experimental syntax in the coming years will be as a tool for investigating what the acceptability judgment data reveal about the nature of syntactic theory. We have seen two examples of this approach in this chapter: the question of reductionist approaches to complex syntactic phenomena (e.g., island effects), and the question of gradient approaches to syntactic theory. Undoubtedly there are more questions waiting to be discovered as the field progresses toward an integrated theory of language.

12

Working memory and language processing: Theory, data, and directions for future research

Matthew W. Wagers and Brian McElree

12.1 Introduction

The goal of this chapter is to relate contemporary theories of human memory and linguistic structure. Memory has often featured in linguistic explanations, usually via the linking hypothesis that our ability to interpret or express complex sentences can be bounded by certain properties of the computational system involved in recognizing or interpreting those sentences (Miller and Chomsky 1963). The general logic supporting this hypothesis is straightforward. Comprehending or producing language involves the construction of novel representations out of simpler, familiar parts. In doing so, a series of decisions is implicitly made about what the goals of analysis are and how to manipulate the set of representations to achieve those goals. These decisions often need access to information that is neither sequentially nor hierarchically adjacent to the signal that triggers the decision. Take as a concrete example the sentence in (1):

- (1) The caterers inquired **which appetizers** Billy would prefer to serve at the party.

The way in which a comprehender sets out to interpret the underlined VP depends both on remembering that a *wh*-phrase (in bold face) was encountered in the prior clause and also on being able to gain access to its contents. If the comprehender forgets that the *wh*-phrase exists, then the VP will be incorrectly assigned its intransitive meaning. If the comprehender remembers that an argument has been displaced but loses access

to its contents, then that argument will potentially be assigned the wrong thematic role. The example in (1), though readily understood, illustrates the demands an adequate working memory system must meet to process language. Moreover, there is substantial evidence that forgetting occurs in ordinary language comprehension and growing evidence that the causes of forgetting are the same as those found in other domains outside language use. We thus believe that combining models of memory dynamics with models of linguistic form represents a fruitful means of answering core questions of representation.

The key idea that will organize our discussion is that there is a significant limit on the amount or scope of information that can be concurrently maintained and made available to on-going processing (Broadbent 1958). Such information is said to be in focal attention. There is consequently a functionally important interaction between the maintenance of the readily accessible focal representations and the retrieval of other representations to displace or transform the current contents of focal attention. We can untangle this interaction by answering the following questions:

- (2) (Q1) When is information retrieved?
- (Q2) How is information retrieved?
- (Q3) What factors determine the success of retrieval?

(Q1) amounts to asking how much information can be concurrently maintained in focal attention. The greater the capacity of focal attention, the less often its current contents will have to be shunted to make way for new information. Therefore the capacity of focal attention contributes to the expected frequency with which memory retrieval operations occur. There is broad agreement that focal capacity is restricted. Specific estimates vary depending on task and stimulus structure (Cowan 1995, 2001; Garavan 1998; McElree 2001, 2006; Oberauer 2002). The data we will review in this chapter suggest that only one task-relevant representation is typically maintained in focal attention. Information not contained in that representation can only directly influence processing if it is retrieved to replace the focal representation.

(Q2) asks for the mechanism by which other previously constructed representations are restored to the focus of attention. The answer to (Q2) depends in part on the architecture of working memory: that is, what lies “beyond” the focus of attention. McElree (2006) draws a distinction between bi-partite and tri-partite working memory architectures. In bi-partite architectures, information can only occupy two states: *active*, in the focus of attention, or *passive*, in its durably encoded long-term memory (LTM) state. In tri-partite architectures, an intermediate state exists for information not currently being processed which is nonetheless not simply in LTM. For some models, like Baddeley (1986), this intermediate state corresponds to a short-term memory store that is separate from long-term memory – like a buffer. Other models, like Cowan (1995, 2001) or Oberauer

(2002), reject a separate short-term store but claim that a small amount of recently used information is so highly activated as to be functionally distinct from other LTM encodings. In Section 12.2.2, we discuss behavioral evidence from memory dynamics that favors the bi-partite architecture, in the following sense: when the speed of processing is appropriately measured in a number of memory-retrieval tasks, there appears to be a dichotomous split between the speed with which focal and non-focal information can influence processing.

The bi-partite architecture makes a clear prediction about the mechanisms of retrieval. Because encodings outside of the focus of attention are in the same state as LTM, then it is predicted that the retrieval of recently encoded information will proceed in largely the same way as information encoded in LTM. Though it is not likely the case that there is a “single mechanism” of retrieving LTM, that is, extra-focal encodings, the dominant mechanism appears to be content-addressable retrieval. Content-addressable retrieval refers to the use of the contents of memory encodings themselves in the access procedure. Inherent features of the desired encoding are used as probes to identify matching candidate encodings. Content-addressability is implementable in a variety of architectures and is characteristic of most contemporary human memory models (Clark and Gronlund 1996; Gillund and Shiffrin 1984; Hinton 1989; Hintzman 1988; Kawamoto 1988; Murdock 1982; Plaut 1997; cf. Gallistel and King 2009). Content-addressability may be compared with another mode of access, in which arbitrary address labels serve as indexes to information. In the absence of knowing the address label, the memory must be searched to locate matching data. In a random-access memory the hardware is so structured for accessing memory by location; however, data structures and algorithms exist, like hash tables, for achieving forms of content-addressability or at least for obviating exhaustive searches (Kohonen 1980). The key advantage of building content-addressability features into a memory system is that, when information of a certain type is desired, it is often not necessary for the system to consult or otherwise be influenced by irrelevant information. This contrasts with search procedures that require pairwise comparisons of the desired information with each memory record in the search set. Content-addressable retrievals yield retrieval times that are independent of the size of the search set. Searches, in contrast, yield retrieval times that are proportional to set size (Sternberg 1966, 1975; Theios 1973; Treisman and Doctor 1987). The second advantage of content-addressability is thus its speed.

What answer does content-addressable retrieval provide to (Q3)? The major determinant of retrieval success is the match between the information used at retrieval – the cues – and the desired encoding. If the combination of cues used at retrieval is sufficiently distinct, then retrieval success will be high. However, if they apply to many different encodings in memory, than the wrong encoding may be retrieved. This phenomenon

is referred to as similarity-based interference. This is the trade-off for the fast access times associated with content-addressable memories: irrelevant encodings that are similar to the desired encodings can negatively impact processing. As we'll see, this problem is potentially very acute for linguistic representations. Because linguistic representations are recursive, compositional objects built out of a relatively small repertoire of atomic parts, they contain highly self-similar subparts. On the one hand, much available evidence suggests that content-addressability nonetheless prevails in memory retrieval for language processing. Moreover, there is growing evidence of similarity-based interference in certain environments. On the other hand, there is a large body of evidence indicating that much of sentence processing is grammatically accurate, and interference-robust. An important research question is how these two sets of empirical observations might be reconciled. It may be that different linguistic phenomena are processed by distinct memory mechanisms. According to this view, fast, interference-prone memory operations characterize some kinds of dependencies in language; while slower, search-style operations characterize others. A second (non-exclusive) possibility is that the systems which encode linguistic representations and manage the cues used at retrieval have become effectively adapted to the nature of linguistic representations, such that optimally diagnostic cue sets are used in retrieval. We will consider evidence for both of these possibilities below.

In [Section 12.2](#), we discuss the evidence for focal attention from basic memory research. We first introduce the main source of data we draw upon in our own research, the speed-accuracy trade-off technique. Then we discuss its application to language processing. In [Section 12.3](#), we discuss evidence that content-addressable retrieval is used in many natural language dependencies. In [Section 12.4](#), we review the evidence that similarity-based interference is a central cause of forgetting in language. In [Section 12.5](#), we conclude with some new theoretical assumptions about how linguistic structure interacts with a capacity-limited memory and outline questions for future research.

12.2 The focus of attention in memory and language

12.2.1 Background

The search for a systematic relationship between the “grain-size” of compositional representations and the processor’s capacity limitations was one of the earliest research questions in modern psycholinguistics. Though the link has never been explicitly drawn to focal attention, a number of researchers have asked how large structured representations are “chunked” into smaller pieces for analysis and storage. At issue in the earliest studies were the principles of perceptual segmentation – that is, how perceivers divided linguistic input into units for further analysis.

Researchers deployed a variety of tasks to show that when attempting to recall or respond to sententially embedded words, behavioral discontinuities could be observed which were aligned with important details of syntactic structure – very frequently the sentence boundaries. An excellent introduction to these studies can be found in Fodor, Bever and Garrett (1974) or more recently in Townsend and Bever (2001). We will mention only a few of the most relevant here.

Perhaps best known among the attempts to relate principles of perceptual organization to linguistic descriptions are the click dislocation experiments (Fodor and Bever 1965; Garrett, Bever, and Fodor 1966; Bever, Lackner, and Kirk 1969; Reber and Anderson 1970; Chapin, Smith, and Abrahamson 1972; Bever 1973; among others). Ladefoged and Broadbent (1960) observed that perceivers are relatively inaccurate in recalling the location of a short burst of noise (i.e. a click) when it has been inserted in a speech stimulus. Crucially, participants were found to be more accurate in locating clicks when the stimulus was a string of unrelated words than when it constituted a grammatical sentence. This contrast suggested that the ability to accurately locate the click was affected by higher-level influences on stimulus encoding. Fodor and Bever (1965) then showed that participants tended to recall clicks as occurring at or attracted to major constituent boundaries near the words into which they were inserted – and these nearly always corresponded to clausal boundaries. For example, in the following sentence, clicks were spliced into either the middle of the word “but” or in the word “fast.” Regardless of serial position, however, participants tended to recall the click occurring between “fast” and “but.”

- (3) Peter ran quite fast but we caught him in the narrow alley.

Bever, Lackner and Kirk (1969) considered further sentence structures. Particularly intriguing was the distinction obtained between clauses embedded by verbs like “persuade” versus verbs like “expect,” as in the bracketed sentences (4a) vs. (4b). In (4a), the DP that immediately follows is VP-internal whereas in (4b), the DP originates inside the verb’s TP argument. Despite the surface similarity of the two sentences, Bever, Lackner, and Kirk found that nearby clicks were reported as occurring between “Bill” and “to” more often in (4a) than in (4b), consistent with the syntactic and semantic distinctions between the strings.

- (4) (a) John persuaded [^{DP} Bill] [^{TP} to leave]
(b) John expected [^{TP} [^{DP} Bill] to leave]

The authors took this for evidence that deep structure sentence boundaries, and not just surface structure boundaries, influenced segmentation. This claim is perhaps moot from a contemporary viewpoint about syntactic process and representation, though the data remain relevant. Depending on whether or not there is raising-to-object in (4b) (Postal 1974), we might say that the derivational history of the sentence matters

in determining perceptual segmentation; or it may be that something less directly related to constituent structure matters, like the argument structure of the two verbs involved.

The insights guiding these experiments are not very dissimilar from the implications of a focal/non-focal distinction: the analysis of the input is capacity limited and the products of analysis will reflect that constraint. This reasoning was inherited from Broadbent's single-channel hypothesis (1958). It may be helpful to think of a car trying to enter the freeway – it can only be done (without accident) when there is a break in the flow of traffic. In the case of the clicks, it was reasoned that study participants could only encode their location when a perceptual segment boundary was passed. Passing the boundary briefly freed up the input channel and allowed the participant to register the click.

Other researchers applied word recall techniques (Jarvella 1971) or whole-sentence recognition probes (Sachs 1967) to gain insight into the units used to encode linguistic stimuli. These studies all identified clause membership as an important determinant of what constituents were immediately or faithfully accessible. Jarvella's study illustrates this pattern: when participants were asked to recall words in a sentence, recall was nearly perfect if the words occurred in the immediately preceding clause. Accuracy fell modestly for words that occurred in a prior clause which was nonetheless a constituent of an immediately preceding root sentence. However, recall accuracy dropped precipitously for words occurring in a clause or sentence that did not most recently precede the recall prompt.

Based on all of this evidence, Fodor, Bever, and Garrett (1974) concluded that potential surface clause boundaries initially delimited the units of encoding. To comprehend a sentence, the clause boundaries must first be (heuristically) located, after which the material within their span can be analyzed more deeply and assigned a fuller constituent structure. Carroll and Tanenhaus (1978) argued instead that segmentability fell along a cline and that it was not deterministically related to fixed boundaries. For example, they provided evidence that gerunds were not analyzed separately from the clause containing them, unless an overt subject was present. Regardless of the exact principles governing segmentation, most proposals of the era concluded that fairly large grammatical domains delimited the memory encodings.

Based on the experiments of the 1960s and 1970s, we might conclude that the clause, or perhaps something larger, like the category CP, defines the span of focal attention. These experiments consistently showed that clause boundaries exerted a strong influence on many mnemonic properties of linguistic materials. However such a conclusion would be premature, for two reasons. The first stems from the observation that the comprehension processes that assign a syntactic analysis and deliver an interpretation operate well below the clause level. With the advent of

word-by-word experimental techniques, it soon became clear that processes at all levels of analysis quickly interweave as single words are incorporated into a sentence, delivering the relevant representations incrementally (Marslen-Wilson 1975). Incremental syntactic analysis and semantic interpretation, occurring in step-sizes much smaller than a clause, suggested that compositional structure must initially be encoded in smaller chunks (see Frazier and Fodor 1978). However, there have been few direct empirical attempts since the 1960s to measure the scope of structured encodings within a clause. Working memory constraints have still played an active role in theory-building (e.g., Gibson 1998, 2000, Just and Carpenter 1992, Lewis 1996), but inferences about encoding accessibility has usually been indirectly based on the moment-to-moment complexity measures derived from reading-time studies.

The second caution has to do with how we characterize working memory. The phrase “working memory” has many senses in common use, at least among psycholinguists. For our purposes, it is important to draw a distinction between “working memory” as the sum of the many component parts and processes that support the encoding, retention, and recollection of recently encountered information; and “working memory” as the particular mechanism for maintaining information over the shortest term (sometimes, “short-term memory,” or STM; but usage is not always consistent; see Cowan 2005, for a clear account). It is the latter sense that pertains to focal attention – the label we prefer. Because we can talk about the working memory system as a whole, or a component of the system, like focal attention, it is underinformative to describe the span of working memory merely in terms of typical task success – for example, how many words can be recalled – since task success will be a function of many component processes. Thus the fact that recall from language stimuli is strongly clause-delimited does not allow us to conclude that maintenance occurs in clause-sized pieces. Therefore we now turn to methods for more directly measuring the accessibility of information, as that information is being used in language processing.

12.2.2 Measuring processing speed

Issues of how accessible different representations might be, how different representations are accessed, and what factors control access to a representation are usually best addressed with measures of processing speed. In this section, we will spend some time discussing how to best measure processing speed. In doing so, it is necessary to introduce the response-signal speed-accuracy trade-off procedure (SAT). We will refer to SAT measures throughout the remainder of the text.

A seemingly natural candidate for estimating processing speed is response time (RT), a measure that is often used to investigate hypotheses about the dynamics of information processing. Unfortunately, however,

RT is not a pure measure of processing speed, as it can vary with the quality of the underlying memory representations alone (e.g., Dosher 1976, 1981; McElree and Dosher 1989; Ratcliff 1978; Wickelgren 1977). Information quality is often captured by such constructs as strength, distinctiveness, or fragility – properties that can be ascribed to individual representations or to the match between individual representations and the cues used to retrieve them in context. For language processing, another relevant determinant of information quality is the likelihood that a sequence of operations, each with its own failure rate, produces a usable output. Because RT depends on the quality of representations, and not only on the time necessary to manipulate them, it is impossible to uniquely infer a difference in processing speed given only a difference in RT.

To measure processing speed directly, one needs to derive a full time-course function that measures how accuracy varies with processing time. This enables speed to be measured with co-varying differences in information quality (Luce 1986; Wickelgren 1977; Reed 1973, 1976). The response-signal speed-accuracy trade-off (SAT) procedure derives such functions by tracking changes in the accuracy of a response as information accrues over time. Participants are trained to judge some key aspect of a test stimulus (e.g., whether a probe word was present in a studied list, whether a sentence is acceptable) within several predefined 100–300 ms windows, marked by the presentation of a response cue (a tone). The windows are chosen so that the full time-course of processing is sampled, from times when performance is at chance to times when performance has reached maximum (asymptotic) accuracy. Accuracy is usually scaled in d' units, by taking the difference between the z-scores of the probabilities of correctly responding that a stimulus property is present (a *hit*) and incorrectly responding that a stimulus property is present (a *false alarm*; see MacMillan and Creelman 1991). By penalizing the false alarms, d' corrects against response bias.

Figure 12.1 illustrates two typical SAT functions. A good model of typical SAT data is given by the following shifted, saturated exponential function, with the parameters λ , δ , and β :¹

$$(5) \quad \begin{aligned} d' &= \lambda(1 - e^{-\beta(t-\delta)}) && , t > \delta, \\ d' &= 0 && , \text{otherwise} \end{aligned}$$

SAT data typically display three identifiable phases:

- (6) a. A period of chance performance

In the model function, this period is captured by the SAT intercept parameter, δ (ms). Where $t \leq \delta$, d' is defined as 0.

- b. A period of increasing accuracy

In the model function, the rate of increase is captured by the SAT rate parameter, β (ms^{-1}). $1/\beta$ is sometimes called the function's time constant. When $t = (1/\beta) + \delta$, then d' is $\sim 63\%$, or $(1 - e^{-1})$, of ultimate attained accuracy.

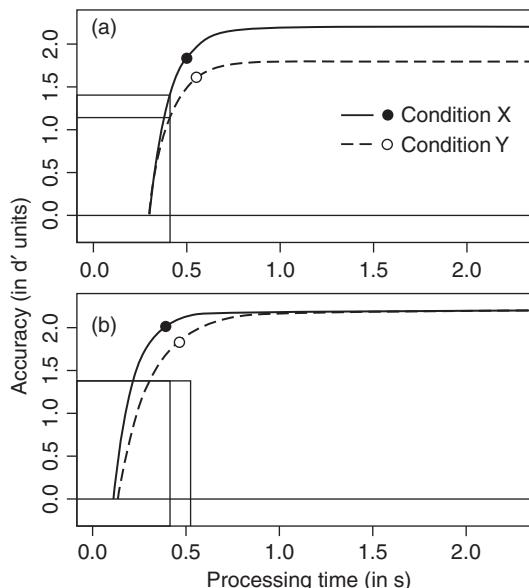


Figure 12.1 Hypothetical SAT functions. Panel (a) depicts two functions that differ in asymptotic accuracy alone. Panel (b) depicts two functions that differ in rate and intercept alone. Horizontal lines intersecting the y -axis indicate performance levels at $(1 - e^{-1}) \cdot \text{asymptote}$ ($\sim 63\%$, reflecting to the function's time constant). Vertical lines intersecting the x -axis indicate the time at which 63% asymptote is attained. Note that in Panel (a), the latencies do not differ but asymptotic accuracy does; whereas in Panel (b), the latencies differ but the asymptotic accuracies are identical.

c. An asymptotic period

In the model function, asymptotic accuracy is captured by the SAT parameter, $\lambda (d')$.

The SAT asymptote parameter, λ , measures the overall probability of a certain response, such as correctly retrieving an item; and it provides estimates of information quality. [Figure 12.1\(a\)](#) depicts a case in which asymptotic accuracy varies between two conditions, while other parameters are held constant. Processing speed is jointly measured by the point at which information first becomes available, the SAT intercept, δ , and the rate at which accuracy grows from chance to asymptote, the SAT rate, β . [Figure 12.1\(b\)](#) depicts two functions with different intercepts and rates of rise to asymptote (here, the functions are shown rising to a common asymptote).

A difference in either rate or intercept will give rise to disproportional SAT dynamics, with the functions reaching a given proportion of their respective asymptotes at different times (vertical lines in [Figure 12.1\(b\)](#)). Disproportional dynamics, whether due to differences in intercept or rate, indicate underlying differences in either the rate of continuous information accrual if processing is continuous or the distribution of finishing times if processing is discrete (Dosher 1979, 1981, 1982, 1984; Meyer *et al.*

1988; Ratcliff 1988). Crucially, we can assess whether conditions differ in processing speed by examining the intercepts *and* rates of the SAT functions, regardless of whether or not they differ in asymptote (e.g., Dosher 1981, 1984; Hintzman and Caulton 1994, 1997; McElree 1996, 1998, 2001; McElree and Dosher 1989, 1993; Ratcliff 1978; Reed 1973, 1976; Wickelgren 1977).

12.2.3 Diagnosing information in focal attention

The information that is directly and concurrently accessible to cognitive processes is said to be in focal attention. Several lines of evidence, using measurements derived from a variety of cognitive and perceptual tasks, support the idea that only a limited amount of information can be maintained in focal attention (Broadbent 1958; Cowan 2001, 2006). Measures of the speed of accessing information provide perhaps the most direct, unequivocal evidence for a unique representational state involving focal attention (McElree 1998, 2001, 2006). The logic is straightforward: information in focal attention should be processed quickly, as it does not need to be retrieved before it can be used in on-going operations. Memory retrieval studies provide strong support for this claim.

For example, consider an item recognition study. The task for participants is to study a list of words, presented sequentially, and then indicate whether or not a test item was present in the list. When no other activity intervenes between the last item studied and test, then the item is accessed at an exceptionally fast rate: 30–50% faster than other items in the list, as measured by SAT rate, intercept or both (McElree 1996, 1998, 2006; McElree and Dosher 1989, 1993; Öztek and McElree 2007; Wickelgren *et al.* 1980). [Figure 12.2](#) illustrates this phenomenon with data from McElree (1996).

In the studies depicted in [Figure 12.2](#), participants studied 6-word lists, presented word by word. After the final word, a visual mask was first displayed and then a test probe ([Figure 12.2\(a\)](#)). Depending on the trial, participants judged whether the test probe was in the memory list (Panel (b)), rhymed with an item in the list (Panel (c); the sample trial depicted in (a)), or was a synonym of an item in the list (Panel (d)). In each case, responses to the last item studied were highly accurate and, crucially, were associated with fast processing dynamics. In this case, the faster dynamics were associated with an earlier intercept, but the difference often is expressed in rate (see McElree and Dosher 1989; McElree 1996, 1998). Asymptotic accuracy, a measure of the likelihood of retrieving an item, showed the standard bowed serial position functions consistent with standard forgetting models (*ibid.*). Accuracy increased with the recency of study, with a small “primacy” effect for the first list item. However, retrieval speed showed a sharply dichotomous pattern: processing speed was fast for the last item, but all other items were retrieved with a slower speed, which did *not* vary with recency.

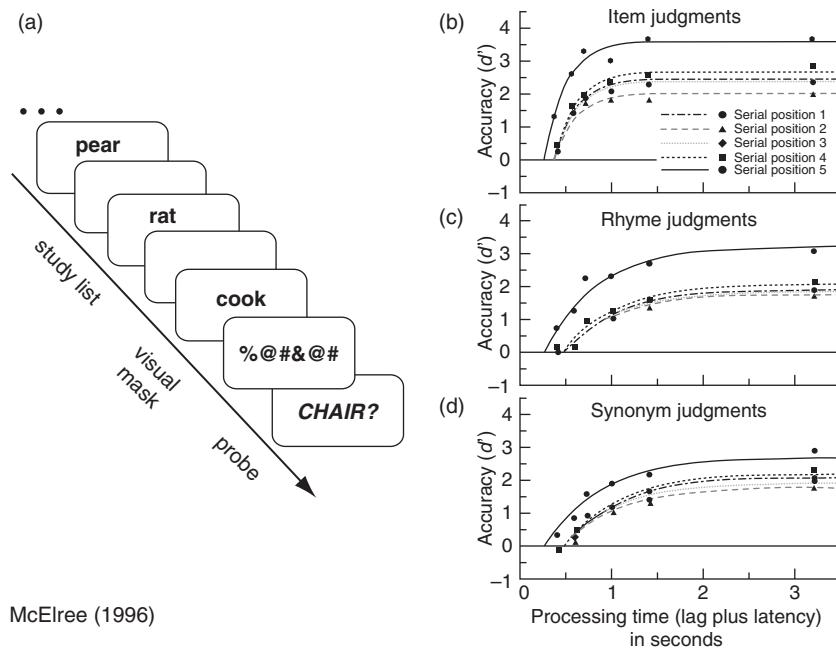


Figure 12.2 The focal attention advantage in three trial types.

Could the speed advantage observed for the final item be attributed to a relatively low-level perceptual effect, rather than an encoded memory of the stimulus? Setting aside the fact that study and test were interrupted by a visual mask, the data from McElree (1996) makes the case that what matters for obtaining the speed advantage is that the test response depend on the last *task-relevant* representation to occupy focal attention. For the rhyme test, focal attention must include information about the word's phonology. For the synonym test, it must include information about the word's semantic features. Note that these results do not necessarily imply that all conceivable information about a word was available – upon presentation of the mask, participants were cued about which judgment was required so they could have transformed the last representation accordingly.

Furthermore, several findings indicate that the speed advantage is uniquely linked to whether a test item was in focal attention, not simply that it was in final position. Procedures that encourage subjects to reinstate items from different list positions in focal attention provide direct support for this claim. McElree (2006) found a dynamics advantage for items from an earlier part of the list when subjects were pre-cued to retrieve these items just before a test. Further, in a controlled rehearsal study, in which subjects were instructed to covertly rehearse items on the list to an external signal, McElree (2006) found that the advantage tracked with the items that subjects would have been rehearsing at test time.

Importantly, clear correlates of this speed advantage are found in other measures. In RT tasks, items associated with fast SAT dynamics not only show distinctly lower mean RTs, but also that the entire RT distribution is being shifted toward earlier times (McElree 1993, 1998; Oberauer 2002, 2006). In fMRI, Öztekin *et al.* (2008) found that conditions that engender fast SAT dynamics are associated with less activity in the hippocampus, a region involved with successful episodic retrieval, and less activity in the inferior frontal gyrus, a region thought to be involved with retrieval effort (e.g. Cabeza *et al.*, 2003). These patterns suggest that retrieval operations are not required for information in focal attention.

12.2.4 Language and focal attention

There have only been a few attempts to confirm the influence of focal attention on language processing. The three studies we will describe here each test different hypotheses about the relation between constituent identity and size and what can be maintained in focal attention. Before moving on to those studies there is one finding from the basic memory studies that is an important predecessor to pursuing linguistic hypotheses. A reasonable question to ask is whether or not anything larger than a word can occupy focal attention. If the answer were no, then it is unlikely that there is any systematic relationship between linguistic description beyond the lexical level and what can concurrently be attended. The best “pre-linguistic” evidence comes from McElree (1998). In this study, a list of nine words was presented to participants, followed by a mask and test probe (as depicted in Figure 12.2). However, the words were not randomly selected. Instead they comprised three triplets of category exemplars from nine categories which were used experiment-wide. For example, the list in (7) can be described as a sequence of nine words, or a sequence of three categories (*animal*, *body part*, *article of clothing*).

- (7) [^{ANIMAL}DOG HORSE PIG]
[^{BODY PART} LEG HEAD FOOT]
[^{CLOTHING} HAT DRESS TIE]

McElree (1998) measured the speed of processing using the SAT procedure and found that a rate advantage was found not simply for the last word (i.e. “tie”) but for any word in the last category (i.e. “hat,” “dress,” “tie”). This result is, in one sense, unsurprising. As Miller (1956) and many others have observed, large sets of information can more effectively be retained if there is an encoding scheme that reduces the dimensionality of the set – the phenomenon broadly referred to as “chunking.” Similarly, Jonides *et al.* (2008) emphasize that the restriction of maintenance processes to single, “atomic” representations is simply cognitively implausible, as few operations can be conceived as unary relations. McElree (1998) is a useful result, however, since it increases our confidence that the SAT rate

advantage truly tracks something functionally relevant: the attended contents in working memory and not merely single words.

McElree, Foraker, and Dyer (2003) extended the findings of McElree (1998) by asking whether focal attention subtended linguistic categories. In Experiment 2 of that study, sentences were created which systematically extended a DP in subject position, as the set in (8) illustrates:

- (8) (a) SUBJECT + Ø + VERB
The editor laughed.
(b) [SUBJECT + OBJECT RELATIVE CLAUSE] + VERB
The editor that the book amused laughed.
(c) [SUBJECT + PP + O.R.C.] + VERB
The editor of the prestigious journal that the book amused laughed.
(d) [SUBJECT + [O.R.C. [+ SUBJECT RELATIVE CLAUSE]]] + VERB
The editor that the book that won the award amused laughed.
(e) [SUBJECT + [O.R.C. + [O.R.C.]]] + VERB
The editor that the book that the journalist wrote amused laughed.

The SAT procedure was adapted to sentence materials by presenting the sentences one word at a time (Rapid Serial Visual Presentation, Potter 1988). At the tone cue which followed the final word, participants were trained to make an acceptability judgment (McElree and Griffith 1995, 1998). Unacceptable foils to the acceptable sentences in (8) involved anomalous unaccusative clauses in the case of animate subjects (“the editor ripped”) or anomalous unergative clauses in the case of inanimate subjects (“the book laughed”). Both inanimate and animate subjects were used in this study. Since a verb like “laugh” might be acceptable paired with one subject phrase and unacceptable paired with another, it was impossible for experimental participants to guess the acceptability of the sentence before processing the final word. Thus the SAT speed parameters can reasonably be said to measure the accessibility of information contributed by the head of the subject phrase at the onset of the verb phrase.

McElree, Foraker, and Dyer’s results are plotted in Figure 12.3. There was a steady decline in asymptotic accuracy as the subject phrase increased in length. This was not surprising, since accuracy can reflect the compounded success rate of a series of operations. In McElree and colleagues’ stimuli, longer phrases were also syntactically more complex, and so it is plausible more analysis was required. Importantly, however, rates did not steadily decline with length. Firstly, there was a large speed advantage for adjacent subjects and verbs (8a) compared to subjects that had been modified (8b–e). This was reflected in the rate parameter of the best fitting functions. These data suggest that the properties of the subject phrase necessary to discriminate acceptable sentences from unacceptable sentences were displaced from focal attention during the process of modifying the subject head.² Sentences (8b–d) shared a common rate parameter, which is consistent with our earlier claim that accessibility in memory is essentially a dichotomous variable:

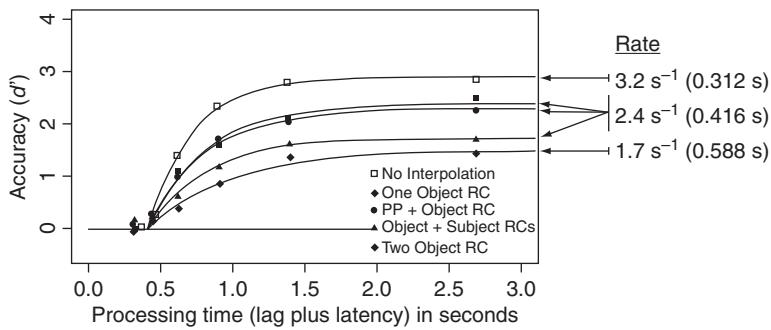


Figure 12.3 McElree, Foraker, and Dyer (2003) Focal attention and subjects. The best-fitting model for these data assigned five different asymptotes to the five conditions; three rates; and a common intercept. Speed-of-processing comparisons can thus be made on the basis of the rate parameter, β , alone. β 's three values and how they map to the conditions is indicated in the right margin

either a representation is focal or it is not. As we'll discuss in Section 12.3, it has important consequences for our understanding of memory access operations: increasing the number of elements which intervene between dependents does not necessarily affect processing speed. However, (8e) was processed at an ever slower rate than (8b–d). The authors argue that this derives from the high likelihood of misanalyzing the two relative clauses in those sentences (consistent with their center self-embedded status, which is exacerbated for two object relative clauses; see Gibson and Thomas 1999). It is known that SAT measures are sensitive to reanalysis (Bornkessel and Schlesewsky 2004), so this is a plausible explanation.

The pattern established by McElree, Foraker, and Dyer (2003) seems in many ways compatible with the data on clause-bounded processing. However the conclusion is more nuanced: the study does not show that any information immediately contained in the current clause is equally accessible. It shows, instead, that an embedded clause is sufficient to displace details about its embedding environment from focal attention. However clauses are large, complex linguistic domains – so it is perhaps not surprising that they are sufficient. The question naturally arises whether anything smaller than a clause can displace the contents of focal information. McElree and Wagers (2009) included an attempt to replicate McElree, Foraker, and Dyer (2003). Crucially they included a condition missing from the original experiment: a non-clausal modifier. In this condition, a subject-attached PP was inserted between the subject and the verb, as in “The editor of the journal laughed.” Overall the pattern of rates replicated McElree, Foraker, and Dyer. Verbs separated from the subject by a relative clause were processed more slowly, compared to an adjacent subject-verb baseline. Crucially, though, verbs separated from their subjects by a PP were processed as quickly as verbs adjacent to the subject. Thus, not all attachments displace the subject from focal attention.³

McElree, Foraker, and Dyer (2003), and its recent replication and extension by Wagers and McElree (2009), demonstrated that language processing shows the same functional signatures of focal attention as do more basic laboratory memory tasks. The properties of the subject phrase necessary to discriminate between acceptable and unacceptable stimuli were more immediately available when the subject was adjacent to the verb, or when the subject was only separated by a PP. Thus intervening material can displace information currently being maintained in the focus of attention. However, the non-clausal modifier condition in Wagers and McElree (2009) showed that not just any intervening material is sufficient to displace the subject. What the relevant difference is that leads to displacement in the case of relative clauses but not in the case of PP remains open to investigation. Some candidate hypotheses include the sheer difference in size and complexity; the typical argument status of PPs used in the study compared to the adjunct status of relative clauses; or the fact that relative clauses themselves include a subject–verb relationship. The last hypothesis is compatible with a hypothesis according to which the encodings of multiple relationships may be maintainable in the focus of attention, but that those relationships cannot be too similar. We return to this idea in Section 12.5.

One difficulty with the subject–verb data is that it is unclear what, exactly, is shunted from focal attention. It could be the entire subject phrase (at least, the phrase minus its relative clause sub-part), or it could be those features of the subject phrase necessary to make the discrimination. Heuristically the relevant factor is likely to be animacy or a related notion. Unfortunately, though the materials were normed for overall acceptability, it is not possible to pinpoint the precise features necessary to perform the task. In a separate study, Wagers and McElree (2010) tested a different type of dependency, one for which the relevant discriminative property could be more objectively established: determiner–noun agreement in English demonstrative DPs. In the case of agreement discrimination, it is clearer what information is necessary to complete the task: the phi-features of the two agreeing elements. Wagers and McElree presented participants with DPs of three different lengths in sentence-final position. An example set is given in (9).

(9) The detective was mistaken about the location of ...

- (a) ADJACENT: Dem_φ + NP_φ
... that burglar/*burglars.
- (b) +1 INTERVENOR: Dem_φ + AP₁ + NP_φ
... that risk-taking burglar/*burglars.
- (c) +2 INTERVENORS: Dem_φ + AP₁ + AP₂ + NP_φ
... that clever, risk-taking burglar/*burglars.

A full set of conditions included both number values (*that burglar*, *those burglars*) and the materials were evenly split between proximal and distal demonstratives. The results showed the familiar dichotomous pattern: faster discrimination for agreement contrasts in D-NP adjacent DPs, than

for DPs in which a modifier intervened. The absolute rate difference between adjacent and non-adjacent conditions was 94 ms, which was quite similar to the focal slow-down obtained in the subject verb studies (see note 2). The fact that the three agreement discrimination conditions were best described by two rates, and not three, suggested that the slowing could not be attributed merely to extra compositional steps required for the additional modifiers. Control conditions in which a semantic anomaly was present (e.g., “the clever, risk-taking jewels”) did show the incremental cost for extra modification: the +2 intervenor condition was approximately 200 ms slower than the +1 intervenor condition.

The DP agreement data showed that a focal/non-focal split could obtain for a grammatical domain much smaller than a clause. Of course, DPs are themselves complex grammatical domains (Abney 1987; Leu 2008). Moreover the possibility remains that the use of complex participial modifiers exacerbated the maintenance of determiner information. Nonetheless the results highlight two possibilities: (1) that displacement from focal attention is possible even for dependencies that span one or two words; and (2) that being dominated by the same TP/CP is itself not sufficient for concurrent maintenance. There was, however, a further important qualification to the agreement data: a two-rate pattern only held for singular DPs. For plural DPs, the same fast rate was associated with both adjacent and non-adjacent conditions. There was thus an interaction with the value of the number feature and its survival in focal attention. Wagers and McElree argued that the markedness of the plural number value could be responsible for the fact that it was maintained over all DP lengths. This observation is not unfamiliar and is consistent with a large body of data on agreement processing. It has often been observed that marked feature values are liable to intrude upon agreement processes in a phenomenon called agreement attraction, leading to productions like “The path^{SG} to the monuments^{PL} are^{PL} littered with bottles” (Bock and Miller 1991; Eberhard 1997; Eberhard, Cutting, and Bock 2005, Badecker and Kuminiak, 2007; Franck 2009). An analogue of agreement attraction can be found in comprehension, reflected in reduced reading times for anomalous strings (Pearlmutter, Garnsey, and Bock 1999; Wagers, Lau, and Phillips 2009) or insensitivity to agreement errors in acceptability judgments (Wagers, Lau, and Phillips 2009). Analyses have also been proposed that generate the effect via purely grammatical mechanisms (Kayne 1989; den Dikken 2001), though these accounts have been challenged (Wagers, Lau, and Phillips 2009). One explanation for the plural number value’s longevity in focal attention comes from the use of a privative feature vocabulary. Maintaining a marked feature value preserves information about that feature, even if other features are shunted. The presence of [PL] dominated by the determiner unambiguously signals plurality in English.⁴ In contrast, the

absence of a number feature is ambiguous: it could derive from a singular determiner perfectly maintained or from a plural determiner whose number feature had been shunted. The comprehender faced with an absence of number information could retrieve the determiner's full feature set to ascertain whether the default number value is appropriate, or whether a plural feature was simply forgotten. Thus long singular DPs would tend to be processed more slowly than shorter ones, since retrieval would be necessitated more often. Long plural DPs, on the other hand, will have rates more comparable to short plural DPs, since retrieval will be necessary relatively less often. Of course it remains a coherent possibility that greater longevity in focal attention is simply another property that clusters with other indicators of markedness. For example, marked forms are typically rarer, and the surprise of encountering a rare form could flag it as worthy of preservation.

Though it is not yet possible to formulate a comprehensive theory of linguistic focal attention given the present sparse data set, we can entertain some preliminary conclusions. We began our discussion with the idea of clause-based processing, according to which a relatively fixed structural domain determined the scope of concurrent processing. However, the comparison of the subject-verb studies with the DP agreement studies favors the idea that the decision about whether or not to maintain information can be relativized to feature types and values – and is not determined merely by the inclusion of a constituent boundary. The results would otherwise be surprising, since the intervenors in the subject-verb studies were arguably larger and more complex than the intervenors in the DP agreement studies. To be concrete, we suppose (1) that it was animacy information that was necessary for discriminating between acceptable and unacceptable stimuli in the subject-verb studies; (2) that it was number information that was necessary for making the same discrimination in the determiner agreement studies; and (3) that animacy information is simply more likely to be maintained in focal attention than number information. Of course, the clearest prediction of such an account is that the focal/non-focal split should appear at different lengths for different feature types *across the same constructions*. In other words, the focal rate advantage for subject phrases containing a PP could shift if the basis of discrimination were number. In [Section 12.5](#) below, we return to this problem. There is, however, one final data set that tells us something about what linguistic focal attention is, or, more accurately, what it is not.

Recall that McElree ([2006](#)) showed that it need not be the final element in a series which occupies focal attention. The structure of the task impacts how attention is divided: rehearsal instructions or the use of an *n*-back paradigm can shift the rate advantage to other elements in the series (or, more accurately, shift the distribution of the rate advantage over elements in the series). The studies discussed above were essentially about how much information could be processed that was relevant to the

last encountered element before other information had to be shunted. However, we should also ask whether any information independent of the last element can be maintained. There have been proposals, for example, about the relationship between information structure and cognitive mechanisms of maintenance and activation (Deane 1991; Gundel 1999). For example, Gundel (1999) proposed that focused information, in the information structure sense, is literally in the focus of attention. As we've seen, that participants can sometimes maintain a target item in focal attention while processing intervening information demonstrates that active maintenance of constituents in focal attention during comprehension is within the bounds of known cognitive abilities. Foraker and McElree (2007) have considered this question with respect to syntactic clefting, a linguistic device that typically brings its clefted constituent into Focus (capitalized here to emphasize its information structure sense). Using both eye-tracking and the SAT technique, they asked whether cross-sentential anaphora was resolved any more quickly when the appropriate referent was the clefted constituent of a preceding sentence. Each stimulus consisted of an antecedent sentence and a short continuation sentence. The example materials set in (10) illustrates their design, which crossed cleft type (it-cleft, wh-cleft), target referent (clefted or not), and acceptability. Presentation was phrase-by-phrase, and so the resulting responses were cued by display of the entire two-word continuation sentence. If clefted constituents could be better maintained in focal attention than non-clefted constituents, then faster processing is expected for resolution to clefted referents.

(10) IT-CLEFT

It was the ardent boyfriend who contemplated the engagement ring.

(a) ANAPHORA TO CLEFTED REFERENT

He stared. / # He sparkled.

(b) ANAPHORA TO NON-CLEFTED REFERENT

It sparkled. / # It stared.

WHAT-CLEFT

What the ardent boyfriend contemplated was the engagement ring.

(c) ANAPHORA TO CLEFTED REFERENT

It sparkled. / # It stared.

(d) ANAPHORA TO NON-CLEFTED REFERENT

He stared. / # He sparkled.

Anaphora to the clefted referent did result in higher asymptotic accuracy but it was not associated with faster processing dynamics. The higher asymptotic accuracy does support the idea that clefted constituents can be cognitively more salient or have a more distinctive memory representation; or at least it confirms the intuition that the minitexts (10a,c) are more coherent. However, these data suggest that being the Focus constituent may not in fact induce maintenance in focal attention.

12.3 Retrieving information outside the focus of attention

Thus far we have spoken mostly generally of retrieval, as a cover term for the means by which passive representations can be restored to focal attention. But what exactly is the nature of retrieval?

Access to information in long-term memory has generally been regarded as direct, with representations being content-addressable (Clark and Gronlund 1996). Direct access occurs when a cue or set of cues makes contact with memory in a unitary process without recourse to a sequence of searches through irrelevant memories (e.g., Clark and Gronlund 1996; Dosher and McElree 2003; Kohonen 1984). To retrieve an encoding in a content-addressable architecture, it is necessary to have some information on hand that is contained in the encoding or that is associated with its contents. This can be contrasted with a location-addressable memory in which specific encodings are (arbitrarily) associated with a storage location. To retrieve an encoding in a location-addressable memory, it is necessary to have the location or address of the encoding. Short-term or working memory has often been argued to be more like the latter – with retrieval from short-term memory either involving a series of comparisons to all currently active concepts (i.e. a *search*, e.g., Sternberg 1966, 1975; Theios 1973; Treisman and Doctor 1987) – or access to a known location, like the top of a pushdown stack or a designated buffer. However, more recent investigations of memory retrieval in both short- and long-term domains, reviewed in McElree (2006), have identified two different ways in which information is retrieved. A relatively slow, serial search does appear to be necessary for recovering temporal and spatial order information (Gronlund *et al.* 1997, McElree 2001, 2006; McElree and Dosher 1993). In contrast, accessing information about the item itself appears to be direct, with the representations themselves being fully content-addressable (McElree and Dosher 1989, 1993; McElree 1996; 1998; 2006; Oberauer 2002, 2006; Öztek and McElree 2007). Content-addressability can be implemented in models with rather diverse storage architectures, including those with highly localized representations and those with highly distributed representations (Clark and Gronlund 1996).

In the memory model we have been elaborating, there is no mechanistic distinction between access to encodings in long-term memory and access to encodings outside of focal attention. The null hypothesis is thus that non-focal information can be restored to the focus of attention via direct, content-addressable access (without precluding the possibility that searches can be deployed). In language processing we might expect *a priori* to find examples of both direct access and serial search and that certain factors bias the comprehender toward using one mechanism or the other. What could those factors be?

On the one hand, direct access to content-addressable encodings is the cheap and fast alternative, because it does not depend directly on

how many encodings reside in memory. Moreover, content-addressable retrieval only activates representations that are associated with the retrieval cues, thus avoiding activation of the many other encodings in memory which are irrelevant to a given process. The success of content-addressable retrieval depends on the quality and discriminability of the information used as retrieval cues. As an example, consider subject–verb attachment in which the verb initiates the retrieval of the subject. If the comprehension system used as its only cue something like “Category is DP” then multiple conflicting phrases would be activated and potentially the wrong phrase selected. The situation improves if a combination of cues can be found which targets just the subject; for example (“Category is DP” & “Case is NOMINATIVE” & “Clause is the same as the current clause”). As we review in [Section 12.4](#), a major cause of forgetting – similarity-based interference – stems from the use of cues that contact too many representations. An analogy can be drawn to using an Internet search engine. Suppose you are looking for pictures of US President Barack Obama. The rate of finding relevant pictures will increase dramatically if the search term “obama” is used compared to the broader term “president.” Given the observation that linguistic representations contain constituents at least abstractly similar both to other constituents in the same expression and to constituents in recently encountered expressions, it is an important question whether an adequate theory of retrieval cues can be formulated.

A particular challenge that content-addressability faces has to do with relational predicates like CLOSEST-TO-ME or C-COMMANDS-ME, because the extension of those predicates changes depending on the element that cues the retrieval. The c-commanders of one category might be totally distinct from the c-commanders of another category. It is not impossible to devise an encoding scheme to capture this fact – for example, by generating a unique ID with which all heads in a command domain would be marked. Retrievals could be restricted to a head’s c-commanders by incorporating this ID into the cue set. However it is a natural advantage of performing a search that the constituents can be searched in a particular order, and that order can be used to satisfy relational predicates (see [Wagers 2008](#), for a longer discussion). Moreover because searches can proceed through the encodings in a syntactic representation in a particular order, notions of distance can be easily captured as well – since the search can stop at the first element to satisfy a description.⁵ The disadvantage of searches is the time they require (perhaps several hundreds of milliseconds for discriminating the relative order of two displaced phrases; see [McElree, Foraker, and Dyer 2003](#)).

Pressure to maintain rapid, fluent comprehension seems to favor direct access. The pressure to construct grammatically accurate dependencies seems to favor searches – at least where heuristics are not reliable enough. On balance, the available evidence suggests that content-addressable access is probably the norm, but that searches can be deployed in specific

contexts. The available evidence comes from two domains: resolution of A-bar dependencies and identification of VP ellipsis antecedents.

Consider once again the example of A-bar dependency formation from the Introduction, with example (1) repeated here:

- (11) The caterers inquired **which appetizers** Billy would prefer to serve at the party.

Assume for the moment two hypotheses about the state of the comprehension system as it begins to analyze the underlined VP: (1) the VP is believed to contain a gap; but (2) no information is maintained in focal attention about the contents of the displaced *wh*-phrase. How would the comprehender restore the information in the *wh*-phrase in order to compose the VP? McElree, Foraker, and Dyer (2003; MFD henceforth) sought to establish whether comprehenders deployed a search to recover the filler or retrieved it in a content-addressable fashion. They constructed stimuli that crossed two factors: the thematic goodness-of-fit between the critical verb and its displaced theme; and the number of clauses between filler and gap:

- (12) It was the scandal that ...
(a) SAME CLAUSE: the celebrity relished/ *panicked.
(b) +1 CLAUSE: the model believed that the celebrity
 relished/*panicked.
(c) +2 CLAUSES: the model believed that the journalist reported
 that the celebrity relished/*panicked

Participants judged the acceptability of these sentences in the SAT paradigm. MFD predicted that, if comprehenders were using a search, the rate parameter of the resulting SAT curves would vary with the distance manipulation. Based on work in the list memory literature, it is known that retrieval times vary directly with set-size under a variety of search algorithms (McElree and Dosher, 1989). If, by contrast, the filler were retrieved in a content-addressable fashion, the rate parameter should be constant across the length manipulation. The results favored content-addressable access: the best-fitting model of the data assigned only one rate parameter for all three length conditions: 412 ms^{-1} . Asymptotic accuracy smoothly declined with length, however – suggesting that the longer the dependency, the less likely it was to be successfully completed. This latter finding is consistent with a large body of data (King and Just 1991; Gibson 1998, 2000; Phillips, Kazanina, and Abada 2005).

The shared rate parameter for the three length conditions is inconsistent with the set-size effect predicted by a search. MFD argue that it is most consistent with a content-addressable access procedure, in which the filler representation is contacted by retrieval cues that uniquely target it. There are some related explanations of the data consistent with the shared rate: one is that the filler is maintained in a distinguished representational state,

like a stack or the HOLD buffer postulated by Wanner and Maratsos (1978). In terms of the memory model articulated in this chapter, this amounts to saying that the contents of the filler are maintained in the focus of attention. Theoretically, given the strong capacity limits on the focus of attention, this possibility seems unlikely. Empirically, cross-modal lexical priming tasks (Swinney *et al.* 1988) and probe recognition studies (McElree 2000) have found that a filler is only in an active state immediately after its introduction and following retrieval at the gap site.

Another possibility is that the distance manipulation was in fact linguistically inert. There is considerable evidence that movement is successive cyclic (Chomsky 1973), with long-distance dependencies being decomposed into a series of local dependencies. It is possible the parser respects successive cyclicity (Frazier and Clifton 1989; Gibson and Warren 2002), in which case the parser may be re-encoding or re-tokenizing a representation of the filler at successive clause edge positions. If lexical features of the filler were recoverable from an encoding of the clause-local representation, then all three length conditions might show the same rate constant whether a search were deployed or a content-addressable retrieval. Further research may be needed to disentangle these alternatives. If an implementation of successive cyclicity includes repeated reactivation, the question arises whether the decline in asymptotic accuracy can be adequately accounted for, however. This cannot be answered on the basis of general considerations, but must be the subject of an experiment or computational modeling (e.g., Vasishth and Lewis 2005).

Martin and McElree (2008, 2009) have considered another phenomenon that involves retrieval: VP ellipsis and how the parser identifies an appropriate antecedent once an ellipsis site is detected. In the representative study reported below (Martin and McElree 2008, Experiment 1), the authors contrasted ellipsis resolution in short sentences to resolution in more complex sentences in which the ellipsis site was hierarchically further from its antecedent:

- (13) (a) NEAR The editor admired the author's writing but the critics/*binding did not.
- (b) FAR The editor admired the author's writing, but everyone at the publishing house was shocked to hear that the critics/*binding did not.

The predictions for this experiment are identical to MFD: if the VP antecedent is identified via a search, then a set-size effect should emerge. In other words, resolution should be slower in (13b) compared to (13a). Content addressability predicts identical resolution rates. Drawing upon convergent evidence from SAT studies and eye-tracking studies, the authors find no evidence that ellipsis sites that are further from their antecedent trigger slower antecedent resolutions. Instead there was only a decrement to asymptotic accuracy: ellipsis resolution becomes less likely

to succeed in longer or more complex sentences. As we'll argue in [Section 12.4](#), the adverse effect complexity has on retrieval can be explained as stemming from retrieval interference: that is, similar encodings competing for retrieval.

The empirical landscape presented suggests that retrieval speed is independent of the number of constituents internal to the sentence. No manipulation on either hierarchical or linear distance has been found to affect retrieval dynamics in either the resolution of A-bar dependencies or VP ellipsis. Of course, while A-bar dependencies and VP ellipsis are important syntactic constructions, we should remain duly skeptical until the empirical landscape is broadened. As we've suggested for A-bar dependencies, the possibility of successive cyclicity effects potentially blunts a distinction between search and content-addressable retrieval. However it must be added that how successive cyclicity might proceed during parsing is not well specified (see Richards, [1999](#), for a suggestion). As well, VP ellipsis of the type tested is relatively weakly hierarchically constrained – so one might expect that content addressable retrieval would be the most adaptive mechanism. Recently, Dillon *et al.* ([2010](#)) have reported a locality effect in antecedent retrieval triggered by the Mandarin reflexive element *ziji*, a finding which could indicate that the parser deploys a search for certain kinds of antecedents.

However the observation that, under several conditions, retrieval dynamics do not vary with the size or complexity of a linguistic stimulus should not be understated. The number of primitive elements that compose a typical syntactic description is substantial. Thus, effective navigation strategies for syntactic representations must be an important contributor to the ultimate explanation for how language processing is as fast, robust, and generally grammatically accurate as it is (Phillips, Wagers, and Lau [2012](#)).

12.4 Interference

In [Sections 12.2](#) and [12.3](#), we have argued that two important general properties of working memory can be observed in language processing. The first property is that very little information is actively maintained at any time. There is limited information, in the so-called focus of attention, which may be currently involved in a computation. Information outside the focus of attention must be retrieved. The second property is that, in many cases, the retrieval mechanism appears to be content-addressable and not dependent on the number or size of encodings that span the representation. In this [last section](#), we will consider what factors affect the success of retrieval. In the unitary model of memory we advocate, a major determinant of success is cue diagnosticity: the precision with which information used during retrieval pinpoints encodings in memory.

If the combination of retrieval cues used at retrieval is sufficiently distinct, then retrieval success will be high. If retrieval cues are associated with multiple encodings in memory, then those encodings are said to interfere (Anderson and Neely 1996). This is the trade-off for fast access times associated with content-addressable memory: irrelevant encodings that are similar to the desired encodings can negatively impact processing.

In their comprehensive article on interference as the cause of forgetting, Anderson and Neely (1996) provide a very practical example of retrieval interference from day-to-day life: forgetting where you have parked your car. Suppose you take a trip to a shopping mall, from which you emerge several hours later only to face near befuddlement as you attempt to locate your vehicle. This experience is common to many of us – why does it happen? Anderson and Neely (1996) propose the following understanding. The memory of where you have parked your car is encoded conjointly with a number of other potentially salient features of the parking event: details about your car, the goals of your shopping trip, time, weather, and other incidental observations. In order to recall where you've parked your car later in the day, you must provide cues for retrieval. Some of these cues could uniquely identify the encoding of your car's location in memory – for example, the fact that you parked next to a rare vintage car, or you saw an opossum in the bushes nearby. Other cues will be associated with many other similar shopping trips – you've been to this mall many times before, often at similar times, for similar reasons. If you happen to also recall the unique details about the parking event before you attempt to recall the location of your car, then you will likely effectively retrieve the location. If you happen to use features that are typical of a parking event to cue retrieval, then you will likely fail to recall the correct location of your car, or perhaps any location. In short, it is easy to forget where you parked because any specific instance of parking is very similar to any other instance of parking that casual recollection will often fail.

It is interesting to note that the basic idea that interference is the cause of forgetting has been around for quite some time (McGeoch 1936, 1942) yet it is still believed to be its most potent source. Another potential source of forgetting is time-based decay. The inability of representations to be integrated into an on-going computation, according to this view, is that an inherent quality of the representation, its activation, has decayed over time (Baddeley 1986; Page and Norris 2000). Though it has been difficult to entirely exorcize a role for time-based decay in forgetting, much recent research indicates that interference provides nearly all of the explanatory power needed (Dosher and Ma 1998; Jonides *et al.* 2008; Oberauer and Lewandowsky 2008). This suggests that if we want to understand the role working memory plays in the interpretation of complex representations – such as linguistic representation – we need to pay closest attention to the similarity structure of that representation.

The question therefore arises whether we observe interference in natural language processing. Perhaps a plausible case which reflects similarity-based interference are certain center-embedded clauses (Yngve 1960; Miller and Chomsky 1963). It is worth noting that it is not center embedding per se, but center *self-embedding*, that leads to the greatest difficulty (Miller and Isard 1964; Lewis 1996; cf. de Roeck *et al.* 1982). This is congenial to the idea that language processing is sensitive to similarity when it must construct syntactic relations via retrieval (see Lewis and Vasishth, 2005). We will set center self-embedding aside, however, precisely because such sentences are so pathological; that is, they are often nearly impossible to interpret or, in the worse cases, to even recognize as communicative linguistic expressions. Moreover, after half a century of research the exact generalizations remain surprisingly cloudy.

Fortunately there are a number of recent studies which document retrieval interference in sentences that are not overall impossible to comprehend; and which involve a variety of common syntactic relations, including agreement, subject–verb attachment, and unbounded movement dependencies (Gordon, Hendrick, and Johnson 2001; Wagers, Lau, and Phillips 2009; van Dyke and Lewis 2003; van Dyke and McElree 2006). As an example of similarity-based interference and its relevance to language processing, consider one of the experimental sentences from van Dyke & Lewis (2003), in (14). In this sentence, the subject of the embedded clause, *the student*, should be paired with the predicate, *was standing*.

- (14) The secretary forgot that ...
the student who thought that the exam was important
was standing in the hallway.

However the presence of a full lexical subject (*the exam*) in the intervening relative clause can impact the dependency formation process. Reading times on the critical predicate, *was standing*, increase and comprehension accuracy decreases, compared either to a condition with no intervening relative clause or one in which the intervening relative clause contains a DP in object position (van Dyke and Lewis 2003; van Dyke 2007). Wagers (2008) contrasted cases in which the [Spec,TP] position of the intervening RC was occupied by a lexical DP (15a) to those in which *there*-insertion had taken place (15b). Evidence for interference was present in (15a), but not in (15b).

- (15) The politician was displeased that the report [...] was covered
 on the evening news
 (a) that support was widespread for her opponent.
 (b) that there was widespread support for her opponent.

Based on these results, it seems that occupying a similar position can lead to interference in the dependency completion process.

Van Dyke and McElree (2006) considered a more direct manipulation of retrieval interference. They examined the processing of cleft constructions

under a memory load manipulation. In half of the experimental conditions, participants were presented with a list of three nouns at the start of the trial, which would have to be recalled after the sentence comprehension task. For example, participants would see the following list:

- (16) TABLE – SINK – TRUCK

Then they would read (17) and answer a comprehension question.

- (17) It was the boat that the guy who lived by the sea sailed / fixed in two sunny days.

Two critical verbs were possible in the sentence (underlined). For half of the conditions, exemplified by the verb *sailed*, the critical verb was not a good fit for the nouns on the memory recall list. For the other half, exemplified by the verb *fixed*, the critical verb and memory load items were good fits for one another. They found that reading times for the two verb types were identical when there was no memory list. However, when there was a memory list, reading times were higher on a compatible verb like *fixed* than on an incompatible verb like *sail*. van Dyke and McElree (2006) argued that, (a) the displaced *wh*-phrase had to be retrieved; and (b) that the specific semantic properties of the verb would be used to form cues for retrieval. Though alternative interpretations of the data are possible,⁶ if retrieval interference is to blame in van Dyke and McElree's experiment, it is somewhat surprising that a bare list of lexemes can intrude upon sentence comprehension. It may be important that there were three of them, such that even a small amount of interference from each word would gang up to lead to a measurable behavioral effect. However Gordon, Hendrick, and Johnson (2001, 2002, 2004) have reported similar interference effects for relative clauses.

Finally, both Wagers, Lau, and Phillips (2009) and Badecker and Lewis (2007) have argued that retrieval interference could play a role in generating agreement attraction illusions. Comprehenders often fail to notice when singular subjects are paired with plurally marked verbs, just in case a plural noun occurs in a relatively local syntactic context. The classic example, which has been the subject of numerous production studies (Bock and Miller 1991; Pearlmuter, Garnsey, and Bock 1999), is given in (18):

- (18) The key to the cabinets were on the table.

Perhaps the most salient fact about agreement attraction is that it essentially only occurs with plurally marked verbs (Eberhard 1997). Cross-linguistically, the broader generalization seems to be that it is the morphologically marked features on nouns (gender, number, or case) which can intrude upon verb–argument dependencies (Bader, Meng, and Bader 2000; Harrison *et al.* 2005; Badecker and Kuminiak 2007). Wagers, Lau, and Phillips (2009) suggested that marked features (perhaps when privatively specified) were potential sources of retrieval interference.

The second salient fact about agreement attraction is that the attracting noun can stand in a variety of relationships to the grammatical subject. The preponderance of production studies had considered cases in which the subject contained the attractor (see Eberhard, Cutting, and Bock 2005). This led to a prominent account of attraction which derived attraction from an over-application of the head-to-phrase feature percolation rule. However, Wagers, Lau, and Phillips (2009) showed that the attractor could contain the subject. Drawing on work by Kimball and Aissen (1971), they showed that sentences like the following were often misperceived as grammatical:

- (19) The *runners* who the driver see each morning always wave.

Because it derives from the content-addressable memory system, retrieval interference is a natural candidate to explain a phenomenon which can be insensitive to hierarchical relationships. However, recent work by Franck *et al.* (2010) suggests that the attractor needs to be in some relatively local syntactic domain. Their studies show that an attractor in a root clause has no effect on agreement inside an embedded complement clause. Notice, however, that a restriction that an agreeing head must occur in a specified domain (e.g., within the current CP) is not the same as a restriction that an agreeing head must occur in a relative position with respect to the agreement target (e.g., be a c-commander).

There is one case of dependency formation which seems curiously immune to retrieval interference: identifying the antecedent of a reflexive anaphora. There have been a number of studies which test whether the parser considers grammatically inappropriate antecedents upon encountering a reflexive. Most studies conclude it does not (Nicol and Swinney 1989; Sturt 2003; Xiang, Dillon, and Phillips 2009; Dillon *et al.*, submitted, but cf. Badecker and Straub 2002). Dillon *et al.* (in prep) compared agreement attraction and reflexive binding in the same eye-tracking study with the same syntactic structures; they found clear evidence of agreement attraction but not evidence in early measures that inappropriate antecedents interfere. Note that prior work by Dillon and colleagues (2010) showed that processing reflexive *ziji* in Chinese was associated with a dynamics profile in which local antecedents could bind *ziji* faster than non-local antecedents. Thus processing reflexive anaphora seems to bear the hallmarks of a strongly syntactically guided antecedent identification process. In contrast many other cases we have considered are ones in which syntactic constraints typically prevail but nonetheless allow for constituents favored by other constraints to intervene early in the comprehension process.⁷

12.5 Conclusions

This chapter summarizes a series of relatively recent experimental results on the nature of memory used in language processing. We have argued

that the memory mechanisms used in natural language processing are fundamentally similar in character to memory in other domains. For even relatively local linguistic domains, only a portion of the information represented in that domain is concurrently accessible ([Section 12.2](#)) – the focal attention effect. Thus it is frequently necessary to restore information to a state of accessibility. Content-addressable retrieval appears to prevail as the mechanism for doing so ([Section 12.3](#)). As a consequence, similarity-based interference is a significant determinant of processing complexity ([Section 12.4](#)). The amount and complexity of information encoded in memory primarily affects retrieval success, and not retrieval times.

There are two senses in which these findings shed light on our understanding of grammatical knowledge. Firstly, there is the relatively direct and important empirical contribution a theory of memory makes to our understanding of linguistic performance: by embedding theories of language comprehension (or production) in a well-justified theory of human memory, we obtain more precise models of real-time linguistic events. Likewise, confronting accounts of human memory with well-justified descriptions of linguistic structure provides a means of clarifying those theories as well. Secondly, there are the ways in which the primitives of linguistic theory – the kinds of representations used, the functions that relate those representations – might be adapted to the kind of memory system into which they are embedded.

[Figure 12.4](#) highlights two conceptions of how a phrase structure could be chunked into multiple encodings: i.e. two “focal strategies.” The capacity-limited focus of attention suggests that chunking occurs, and that it occurs below the clausal level. Panel (a) shows one natural way of accomplishing such chunking: by exhaustively parsing the constituent structure into non-overlapping substructures. In this case, the scope of focal attention is assumed to be roughly X-bar-sized. Exhaustive, non-overlapping chunking of this sort is explicitly assumed in some accounts (e.g., the ACT-R model, Lewis and Vasishth 2005; Lewis, van Dyke, and Vasishth 2006) and more implicitly in others (e.g., the Sausage Machine theory, Frazier and Fodor [1978](#)). We call this focal strategy a “local” focus of attention, because it only concurrently activates a head and its neighbors. Panel (b) illustrates a different focal strategy, one which concurrently activates a more global set of syntactic features.

In the local strategy, dependencies that cannot be satisfied strictly locally must be constructed via a retrieval mechanism. Two principal concerns then arise. Firstly, in the absence of any information about the non-local dependent, retrieval cues are likely to be grammatically general. For example, they may specify the non-local dependents’ required category and other relevant formal features, like its phi-feature composition. Such general cues constitute precisely the kind of retrieval structure which engenders interference.

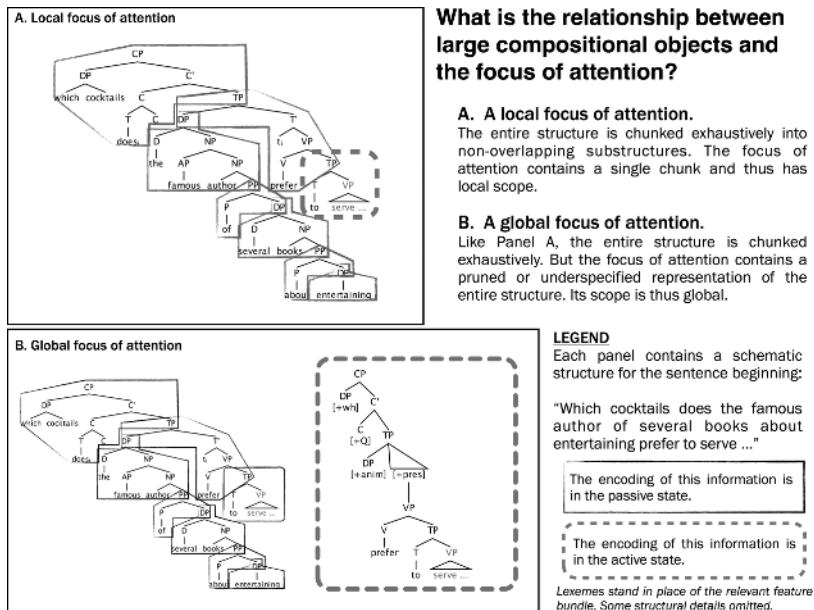


Figure 12.4 Two different conceptions of the capacity of focal attention in language processing.

Secondly, the content-addressable retrieval system can only clumsily handle relational cues like c-commands or is-in-the-same-clause, through means of category-tagging strategies and activation-order heuristics (see note 5). On the one hand, the increased reliance on retrieval means that dependency completion will sometimes be swamped by irrelevant information. On the other hand, because representations occupy focal attention fully with the local context, they can do so in much greater detail. In other words, though the representations are restricted to a small domain, they are fine-grained.

In contrast, the global focus of attention is forced to underspecify. There is limited capacity to bind features into structured representation, and so we conjecture those features are relatively sparse. Firstly, this seems compatible with available evidence on depth of processing in real-time comprehension (Sanford 2002). Secondly, by maintaining a more global context, it is possible to form what one might think of as “first-draft” of non-local dependencies without retrieval. For example, it may be possible to link a verb and its filler without retrieving the specific contents of the filler, but simply by pointing to the small, underspecified representation in focal attention. Interpretation will likely require more details from the lexical representation of the filler and these can be supplied by retrieval. Crucially, the retrieval structure can be built around any item-specific features that may persist in the focus of attention and which are used in the first-draft representation (cf. Ericsson and Kintsch 1995; Nairne 2002).

Such retrieval structures are likely to have greater precision than those constructed from the general grammatical features which a parsing rule or licensing head might propose.

Finally, considering whether the focus of attention maintains more local or more global contexts leads to a more subtle question. When lexical information must be restored to the focus of attention, it is possible that the information comes from previously bound tokens of a lexeme; but it is also possible that it comes (again) from the lexicon. Thus future progress is likely to be made by elucidating the relationship between the retrieval of partial syntactic descriptions during on line comprehension and lexical access itself. Richards's (1999) proposal for left-to-right tree building anticipates this concern, and leads him to support a Move-over-Merge preference: it is better to manipulate light and nimble syntactic representations in the workspace, than to draw from the lexicon. Moreover, there is some tantalizing evidence that it is lexical representations, not syntactic representations, that engender interference from outside the focus of attention. In particular, it has been observed in several languages that only syncretic, case-ambiguous DPs are liable to cause agreement attraction (see Badecker and Kumiak 2007, for discussion). This observation is surprising if the interference stemmed from syntactic representation per se, on the assumption that the syntactic representation is disambiguated for case.

The research described in this chapter has only scratched the surface of the “memory-syntax” interface. But it seems fair to say the greatest progress has been made when both theory and experiment are mutually well informed of both linguistic theory and the theory of memory. Given their common biological foundations, this should come as no surprise.

13

Computational primitives in phonology and their neural correlates

Philip J. Monahan, Ellen F. Lau, and William J. Idsardi

13.1 Introduction

Understanding the representational and algorithmic systems that underlie language has been the primary goal for linguists (e.g., Chomsky 1959a). As it stands, however, the neural bases of linguistic systems remain woefully underspecified. Moving toward an understanding of how such systems are encoded in the brain demands a linking between the basic ontological primitives that underlie linguistic systems on the one hand with neurophysiology on the other (Poeppel and Embick 2005). The goal of the current chapter is to identify what we believe to be the core computational primitives that underlie phonological knowledge and present evidence from the domain of cognitive neuroscience that attempts to investigate the nature of the neural correlates of these primitives.

We take the goal of biolinguistics to be to understand the biological bases of human language with a strong emphasis on its evolutionary origins (Jenkins 2000). In practice, the focus has been placed on investigating the biological underpinnings of syntactic knowledge, e.g., the evolutionary origins of the syntactic operation MERGE (Hauser, Chomsky, and Fitch 2002). Although this work has been enlightening, it is perhaps surprising that relatively less effort has been paid to the biolinguistic foundations of phonological systems (Samuels 2011) and their interface with speech perception and production. This is surprising because we believe that there are a number of reasons why biolinguistic inquiry into this domain should be more tractable. First, fewer levels of abstraction separate the fundamental representations of phonology from the basic sensory input representations. This means that knowledge about how basic auditory information is represented and processed in both humans and animals is more likely to provide important insights into how phonological information could be represented and processed. For this type of

investigation, we can use animal models that allow a fuller range of invasive measures such as recording from individual neurons. As we will show, there is also evidence that representations that are closer to the sensory input are more likely to be neurally implemented with a spatial-mapping coding scheme that is relatively easy to detect with current neuroimaging measures (see below). Second, there already exists an extensive literature to build on from cognitive psychology that has investigated the extent to which “speech is special” (Liberman 1996). The Motor Theory of speech perception is essentially a biolinguistic perspective: whether or not humans have specific biological adaptations that support the processing of auditory linguistic input or whether speech is processed and represented with purely domain-general machinery (Holt and Lotto 2008). Third, on most linguistic theories, phonological representations are the basic unit that connects sensory input and motor output. Therefore, by investigating the biological basis of phonological knowledge, we can benefit from existing evidence from other cognitive domains on the biological basis for sensory-motor translation, such as is needed for visually guided teaching.

Technological advances in the last 30 years have led to exceptionally powerful recording devices, allowing us to not only ask questions about when or where in the brain particular cognitive processes occur but, in theory, to provide answers with millisecond temporal resolution and/or millimeter spatial resolution (see Hämäläinen 1992; Luck 2005; Cabeza and Kingstone 2006 for reviews of some current techniques). Previously, much of what we knew about the brain with respect to language was inferred from neurological impairments, e.g., aphasias. The link between brain region and linguistic process required behavioral observation/testing and an often much later postmortem assessment of the lesions (Broca 1861; Wernicke 1874; see Geschwind 1970 for a review). The advent of hemodynamic and electrophysiological measures allows us to observe cortical and sub-cortical neural activity as it occurs in healthy adults and children. Yet, despite the popularity of these technologies in psycholinguistics and the cognitive neurosciences, our understanding of how the brain encodes speech sounds, words and higher-order linguistic representations has so far remained limited.

In this chapter, our aim is not to outline a complete program for a biolinguistic investigation of phonology, but rather to provide a summary of the existing “tools” for such investigation: the representational primitives from which we believe it will be the easiest to begin, a representative selection of previous cognitive neuroimaging findings that may serve as a guide to promising entry points for further research, and in an appendix, the neuroimaging measures currently available. Although the primary concern of biolinguists may be to determine how phonological systems and grammars are encoded in cortical structures, we believe that phonological representations are likely to depend on how they are

deployed during speech perception and production, and thus that a better understanding of these processes is also required. Issues that must be addressed as we move forward in this line of research therefore include understanding what mechanisms the brain uses to parse acoustic input into words made up of abstract phonological pieces (e.g., Hickok and Poeppel 2007; Poeppel, Idsardi, and van Wassenhove 2008) and on the output side, what mechanisms are used by the brain to arrive at a set of articulation instructions governed by phonological rules of the language from a conceptualized message (see Levelt 1989; Levelt, Roelofs, and Meyer 1999 for psycholinguistic models). Our own research is primarily concerned with perception, and consequently, we will have nothing substantial to report with respect to the question of the neural bases of speech-motor planning.

13.2 Background

Phonological grammars provide a framework for how speech sounds are represented and the nature of the various combinatorial operations they undergo in mapping between *lexical representations* and their *surface-forms* (Chomsky and Halle 1968; Prince and Smolensky 2004; see Kenstowicz, 1994 for textbook discussion). These grammars have explicitly concerned themselves with the *competence* of an ideal speaker/hearer (Chomsky 1965; cf. Bybee 2001) and are generally unconcerned with psychological or neurobiological implications or what has been termed a speaker's *performance*. Consequently, phonologists are interested in developing theories of the knowledge of phonological primitives and rules that operate over them within the language. However, we may want to go beyond a theory of the knowledge that people possess to understand how that knowledge is actually instantiated in the brain. The question may be as simple as: in what brain region are phonological representations stored? Or, more complex, how do networks of neurons represent phonological categories? Because phonology is intimately tied to lexical representation, we might ask how *words* are represented neurally. And from here, we may go on to ask questions of evolutionary biology, e.g., why might the brain have gravitated to this particular way of representing phonology and the lexicon? Unfortunately, formal approaches to phonological grammars and developments in the cognitive neuroscience of language and audition tend to proceed in parallel, and only rarely do advances in one discipline frame discussion in the other. Moreover, the lack of linking hypotheses between representational and procedural primitives in phonology and those in the neurosciences potentially impedes progress in understanding how phonological grammars are neurally encoded or the range of plausible phonological computations that can be carried out in the nervous system (Poeppel and Embick 2005).

We can infer the nature of the knowledge of phonology that people possess from many sources of evidence: acceptability judgments, historical sources, production data, and reaction times on comprehension or production tasks. These measures are also, of course, indirect measurements of neural representations. One avenue of cognitive neuroscience research on phonology is devoted to identifying correlated evidence from slightly more direct measurements of brain activity, such as ERP, MEG, or fMRI (discussed in more detail in the appendix). The data gained from these more sophisticated measures are of a similar ilk to behavioral measurements in that they are aggregated measures whose response properties cannot be directly tied to biological events. Brain measures can have advantages over behavioral measures. For example, it is often not necessary to require an overt response on the part of the participants, and as such, the notion of the “task,” in principle, can be foregone with these measures. We provide a brief review of the tools that are currently used and some of their strengths and weaknesses in the appendix.

13.3 Representational primitives

Traditionally, generative phonology has asked two questions: (1) what representations subserve phonology (e.g., features, segments, syllables) and (2) what procedures map between surface forms and memory representations. The representational units that constitute part of the *knowledge* or *competence* of sound systems have been thought to be discrete and invariant in their most abstract form. The exact form of these representations, however, has been long debated within the field – though, we believe – there is a consensus that sub-segmental units, such as distinctive features, appear to be the appropriate level of description for the purpose of biolinguistic investigation. The specific features themselves continue to be revised, and while these revisions will bring important implications for how we investigate the biological instantiation of phonological knowledge and representation, the general point remains that we believe that electrophysiological measures will reveal a featural organization for phonology. This does not preclude investigating other aspects of phonological representations, e.g., syllables.

A hallmark proposal of phonological inquiry has been that the relevant representational primitives are not single segments, e.g., phonemes, but rather smaller units of which segments are composed (Jakobson, Fant, and Halle 1952). Initially, segments were thought to be bundles of divalent distinctive features, and in the mapping from lexical representation to surface forms, phonological rules would adjust their valence (Chomsky and Halle 1968). Evidence in support of distinctive features arises from extensive cross-linguistic observations that speech sounds pattern phonologically in natural classes. That is, phonological rules

rarely target individual speech sounds, but rather entire natural classes that share some common characteristic or feature, e.g., high-vowels ([+vocalic], [+high]) or voiced consonants ([+consonantal], [+voiced]). For example, the feature specifications [+syllabic, -consonant] designated all vowels in a language and all vowels shared this particular valuation (Halle, 1972) without an internal organization to the features. These features are arranged into a hierarchical organization (Clements and Hume 1995; McCarthy 1988)

For the purposes of perception, distinctive features are divided into two broad classes: articulator-free (e.g., [vowel], [consonant], [continuant]) and articulator-bound (e.g., [spread glottis], [high], [lateral]). Articulator-free features provide a “first-pass” segmentation of incoming speech by identifying “landmarks” in the signal for the perceptual system (Stevens 2002). In Stevens’s “Landmarks” model of speech perception and spoken word recognition, acoustic analysis around landmarks permits hypothesis generation for what the articulators were doing during production, i.e. recovery of the articulator-bound features. This latter class of articulator-bound features provides instructions to the articulators during production. For example, the specification of the feature [\pm nasal] provides instructions as to whether or not the velum, the soft tissue located at the roof of the mouth, should be elevated to prevent air passage through the nasal cavity, while [\pm labial] provides instructions to the lips as to whether or not a constriction should be formed. During natural articulation, these features must be orchestrated in time in a gestural score to produce a meaningful speech signal (Browman and Goldstein 1989).

Despite major differences in phonological theories over the past 50 years, phonologists continue to express generalizations in terms of distinctive features. In generative phonology, phonemes are nothing more than constellations of simultaneous distinctive features. Other theories (Mielke 2008), however, give segments a more primitive status, while still retaining features within the system. Much of the electrophysiological work, thus far, fails to distinguish between segmental and featural interpretations of the experiments. Well-designed experiments to tease apart the interpretations should be a priority as we move forward.

13.4 Where to look for phonological primitives

Now with over a century of work with aphasia and more recently, neuro-imaging, we have gained significant insights into the most basic neural implementation question: where the neurons that code the stored phonetic and phonological primitives are located. Today, there is a fairly broad consensus that these neurons are located in the superior temporal gyrus (STG) bilaterally, a region neighboring primary auditory cortex (Binder 2000; see Hickok and Poeppel 2004 for a review).

Early neuroimaging studies using PET and fMRI universally showed more activity in STG (bilaterally) for listening to speech compared to rest (e.g., Petersen *et al.* 1988; Mazoyer *et al.* 1993; see Binder, 2000 for review). However, STG is a very large region that is likely to be involved in a huge number of different computations. The use of the basic speech-rest contrast did not allow researchers to distinguish between areas involved in non-specific auditory processing from areas involved in speech processing, or between areas involved in phonetic/phonological processing from areas involved in semantic and syntactic analysis. Subsequent imaging studies have contrasted speech with a variety of sophisticated conditions designed to isolate different aspects of auditory processing. For example, reversed speech is usually largely uninterpretable, but it maintains the same level of spectrotemporal complexity as speech, in that all the transitions are present but reversed. By comparing transformed conditions like reversed speech to auditory stimuli with less complexity (strings of tones) or less structure (noise), researchers have been able to show that bilateral regions of STG and the superior temporal sulcus (STS) bordering it are preferentially active for spectrotemporally complex stimuli like speech (Scott *et al.* 2000; Binder *et al.* 2000). Because these regions show increased activity even for speech-like stimuli that is semantically uninterpretable, it seems that they must be involved in a pre-lexical stage of processing.

The neuroimaging results fit in with the results of a long line of aphasia research showing that bilateral damage to STG results in a syndrome known as phonological word deafness (Barrett 1910; Henschel 1918). In this syndrome, early auditory processing is preserved – for example, patients can discriminate between tones of different frequencies and can recognize familiar non-speech sounds and music – but speech perception is severely damaged. Patients report that speech sounds like “meaningless noise” (Poeppel 2001) and frequently display phonemic paraphasias (errors in production) as well (Buchman *et al.* 1986). This pattern of deficits follows if STG supports both processing of speech and storage of the underlying representations.

So far this evidence is consistent with phonetic/phonological primitives being coded in STG/STS regions, but an alternative possibility is that these regions are involved simply in non-linguistic analysis of complex auditory signals. What evidence do we have that linguistic information is coded here? First, there is an argument from null results; phonetic/phonological representations presumably must be accessed in processing speech and speech-like stimuli, yet no other region consistently shows increased activity for speech and speech-like vs. non-speech stimuli. However, there are many reasons that neuroimaging studies might fail to show real brain activity – lack of power in neuroimaging studies is a constant concern – so this is a weak argument at best. Two lines of evidence are more compelling: work showing that STG is differentially active to

phonological contrasts as opposed to auditory contrasts matched for complexity, and work showing that this region is required for speech production as well.

First, many studies show that regions of STG are differentially sensitive to acoustic contrasts that are phonologically meaningful in the listener's native language. For example, a seminal fMRI study by Jacquemot *et al.* (2003) tested French speakers and Japanese speakers on sets of stimuli that differed acoustically along a dimension that was linguistically available in Japanese only (vowel length), or in French only (simple versus complex consonant strings). Behavioral evidence showed that French speakers had difficulty distinguishing stimuli differing on vowel length (e.g. *tokei* versus *tookei*) and Japanese speakers had difficulty distinguishing stimuli such as *ebza* versus *ebuza*, presumably because they perceive an epenthetic vowel in the former due to constraints on consonant form in Japanese. The fMRI data showed that activity in STG was associated with phonologically meaningful distinctions rather than acoustic differences; French speakers showed a significant increase in STG when the syllable form changed, and Japanese speakers showed a significant increase in STG when the vowel length changed. Similarly, the mismatch negativity in MEG, a response shown to be sensitive to phonological variables discussed in more detail below, consistently localizes to STG (Hickok and Poeppel 2004).

Second, there is significant evidence that STG is also accessed during production (see Indefrey and Levelt 2004 for a review). Conduction aphasia, a disorder characterized by phonemic paraphasias and naming difficulty, is associated with damage to left posterior STG (e.g., Damasio and Damasio 1980); and as alluded to above, subtle production difficulties are also often observed in pure word deafness. Neuroimaging studies have shown that regions of left STG are consistently activated in speech production tasks; this is the case even when production is covert (e.g. Hickok *et al.* 2000). The fact that part of this region is involved in both perception and production argues against an interpretation in which STG is only involved in processing complex sounds.

Therefore, there is sufficient evidence at this point to conclude that regions of STS/STG encode phonetic/phonological primitives, in addition to implementing numerous other computations required for processing complex spatiotemporal auditory signals. Although a larger discussion of those computations is beyond the scope of this chapter, it is interesting to note that left and right STG are sometimes thought to be specialized in speech perception at different levels of temporal analysis: left STG for phoneme-sized time-windows (20–40 ms) and right STG for syllable-sized time-windows (200–300 ms) (Poeppel 2001, 2003).

Finally, we note that so far there is not good evidence that any particular sub-region of STG is “speech-specific” in the sense that it only processes speech and not other similar input. In fact, it is not clear that we should expect to find such a sub-region, at least based on linguistic theory.

The only language-specific circuits motivated by classic phonetic and phonological theory are those needed to represent language-specific phonetic and phonological primitives in long-term memory. But these circuits may be very simple and non-centralized, and thus may encompass only a fraction of the machinery needed to process speech in real time. In the next section, we turn to existing evidence about the sub-organization of these long-term representations. While hemodynamic measures provide us with information about where to look, their temporal response properties are poor, especially given the quickly changing temporal dynamics of the speech signal. Electrophysiological data offer much better temporal resolution, and are known to robustly respond to activity in STS/STG.

13.5 How to look for phonological primitives

Electrophysiology (EEG, MEG) has proven to be an exceptionally useful tool for understanding the nature of auditory and speech representations. The early electrophysiological evoked components commonly associated with auditory and speech perception (N1/N1m/M100, N1-P2, MMN/MMNm/MMF) are pre-attentive and do not require a task, providing researchers with a task-independent probe into the early stages of processing. The automaticity of these components, combined with excellent, millisecond temporal resolution, makes the use of such methods extremely powerful in understanding the nature of linguistic and auditory representations and processes employed and entertained prior to contact with a lexical level of representation. An enormous amount of work has been done on not only understanding the response properties and the neurophysiological and cognitive sources of the MMN (Näätänen 1992; Näätänen, Jacobsen, and Winkler 2005; Näätänen *et al.* 2007), but also the nature of auditory and linguistic representations as indexed by the MMN (Näätänen 2001). In particular, MMN studies of speech perception have provided neurophysiological evidence for the existence of representations at the level of abstract phonology (e.g., phonemes, distinctive features) and shown sensitivity of native language phonological inventories (Näätänen *et al.* 1997; Winkler *et al.* 1999) and syllabic constraints (Dehaene-Lambertz, Dupoux, and Gout 2000).

The Mismatch Negativity (MMN; Mismatch Magnetic Field (MMF)/MMNm) in MEG is an electrophysiological component observed when there is a *discriminable physical change* within a series of auditorily presented standards that can be grouped based on some physical or psychological basis into a single category. In a typical MMN/MMF paradigm, participants are presented with a series of standard tokens interrupted by a deviant, which differs from the standard along some physical (or linguistic) dimension. The probability of hearing a deviant within a given experimental block is usually approximately 15%. If the deviant is perceived as being

perceptually distinct from the standards (the requisite physical difference between the standard and deviant is typically commensurate with behavioral discrimination thresholds for particular stimulus attributes (Näätänen *et al.* 2007)), then a large, negative-going waveform (in EEG; a larger magnetic field strength in MEG) in comparison with the electrophysiological response to the standard is observed approximately 150–300 ms post-onset of the deviant stimulus. The magnitude of the MMN elicited by the deviant is determined by subtracting the grand average waveform of the electrophysiological response to the standard from the grand average waveform of the electrophysiological response to the deviant (Näätänen 1992, 2001; Näätänen *et al.* 2007). Modulations of spectral properties of an auditory stimulus, such as the frequency, intensity, and duration have all been reported to reliably elicit an MMN/MMF (Gomes, Ritter, and Vaughan 1995; Sams *et al.* 1985; Winkler *et al.* 1990). Based on intracranial electrode recordings from monkeys (Javitt *et al.* 1992; Javitt *et al.* 1994) and cats (Csépe, Karmos, and Molnár 1987), and MEG and EEG source modeling in humans (Hari *et al.* 1984; Alain, Cortese, and Picton 1998; Scherg, Vajsar, and Picton 1989; see Näätänen and Alho 1995 for a review), the neural generators of the MMN/MMF component are located in the superior temporal plane in primary (or immediately adjacent to primary) auditory cortex, roughly 3–10 mm more anterior than the source of the N1m (M100; Näätänen *et al.* 2007). Moreover, its elicitation does not require attention on the part of the participant or active control modules. Instead, it can be elicited, for example, during sleep (Näätänen *et al.*, 2007), and the magnitude and presence of the response are generally unaffected by attentional or task demands required of participants during the experimental procedure (Alho *et al.* 1998; Näätänen 1992; Ritter *et al.* 1992; Ritter and Ruchkin 1992). The Mismatch Negativity electrophysiological component has also been exploited extensively to probe the nature of speech representations and the impact of native language phonology on the perception of speech (see Näätänen 2001; Phillips 2001 for reviews). Research using the MMN in speech perception has demonstrated that this component reflects much higher levels of representation and processes than what can be inferred from the physical/acoustic attributes of the signal alone.

13.5.1 Vowels

Näätänen *et al.* (1997) assessed the extent to which one's native language vowel inventory affects elicitation of the MMN, and more generally, the early stages of perceptual processing of speech. They tested native speakers of Finnish and Estonian, two closely related languages with nearly identical vowel inventories. The primary difference between the two vowel inventories is that Estonian contains the vowel /ö/, while Finnish does not. Näätänen *et al.* (1997) synthesized vowel tokens corresponding to /e/, /ö/, /ö/, and /o/, whose fundamental acoustic difference is the frequency of their second

formant (F2). The semi-synthetic tokens were matched on their fundamental frequency (F0) and first (F1), third (F3), and fourth (F4) formants. In the MMN paradigm, there were two primary comparisons between the groups: (1) the response to the deviant synthetic vowel tokens (i.e. /ö/, /õ/, /o/) from the prototype standard /e/ synthetic vowel token and (2) the response to deviant sinusoids of the same frequencies of the prototype F2 values for the vowels /ö/, /õ/, and /o/ from the F2 value of the standard /e/. Given the findings from Tiitinen *et al.* (1994), who found monotonic relationships between properties of the MMN and the distance of difference between the standard and deviant tokens, Näätänen *et al.* (1997) expected a monotonic rise in the magnitude of the MMN the further away the F2 value of the deviant was from the F2 value of the standard. Therefore, /o/ should elicit the largest MMN (because its F2 value is furthest from the standard's F2 value), while /ö/ should elicit the smallest MMN compared with the electrophysiological response to the standard /e/. The critical comparison is the response to the deviant /õ/. An MMN magnitude mid-way between /ö/ and /o/ should be elicited in the Estonians and not in the Finnish participants if the MMN is reflecting phonetic/phonemic processing. They found a monotonic increase across the three vowel tokens in the magnitude of the MMN for the Estonian participants but not for the Finnish, while both groups showed a monotonic rise to pure sinusoids whose frequencies matched the F2 formant frequencies of the vowels, suggesting that this difference is not solely attributable to the physical properties of the stimuli, but must also be a function of the phonemic vowel inventories (and therefore, presumably, the distinctive features) of the two groups of participants. Winkler and colleagues (1999) followed up on these results comparing Finnish and Hungarian participants with contrasts that were within-category in one language and across-category in the other. The range of the Finnish vowel /e/ occupies portions of the four-dimensional (F1-F4) vowel space occupied by /é/ and /ɛ/ in Hungarian. Meanwhile, the Finnish vowels /e/ and /æ/ are located in the region of vowel space occupied by /ɛ/ in Hungarian. Consequently, they synthesized a pair of vowel tokens that would be perceived as /é/ and /ɛ/ by Hungarian participants, but only as /e/ by Finnish participants. They also synthesized a pair of vowel tokens that would be perceived as /e/ and /æ/ by Finnish participants, but only as /e/ by Hungarian participants. Winkler and colleagues (1999) report an MMN in all comparisons across groups. The MMNs in the across-category conditions, however, were significantly larger than the within-category condition in both groups of participants. While these findings are consistent with distinctive feature theories, they do not preclude purely segmental explanations.

13.5.2 Consonants

Sharma and Dorman (1999) used consonant-vowel (CV; /ta/-/da/) sequences to better understand the influence of phonetic categories on the MMN.

They found a minimal MMN in the within-category condition, but a significantly larger MMN in the across-category condition, despite the fact that the tokens were equally acoustically distant (20 ms VOT difference between each token in both the within- and across-category conditions). Subsequently, Sharma and Dorman (2000) compared the MMN responses of Hindi and American English speakers on a VOT contrast native to Hindi but absent in English. They manipulated the amount of pre-voicing duration to create a /pa/-/ba/ continuum between 0 and -90 ms VOT. In the MMN experiment, the standard was the -10 ms VOT stimulus and the deviant was the -50 ms VOT stimulus. As predicted, a large and reliable MMN beginning roughly 175 ms post-onset of the deviant stimulus was found in the Hindi participants, but absent in the English participants. Consistent with the findings from Näätänen *et al.* (1997) and Winkler *et al.* (1999), these results suggest that the MMN is sensitive to phonetic category distributions of the native language of the participants. The interpretation of these results is complicated by the fact that the stimuli were words in Hindi, allowing the Hindi participants to map the sounds onto lexical entries, but not for the English speakers. While it is clear that these studies report electrophysiological sensitivity to properties of the stimulus not reflected in their physical attributes (e.g., native language inventory, differential effects contingent upon category boundaries, etc.), it remains to be seen whether or not listeners are constructing phonological representations of the standards in these cases, or whether they are reflecting phonetic category-level distributions.

The experiments conducted up to that point could not distinguish between a phonological account from a purely acoustic or phonetic one. Using MEG in an oddball paradigm, Phillips and colleagues (2000) employed a many-to-one design (e.g., Aulanko *et al.* 1993; Gomes, Ritter, and Vaughan 1995; Winkler *et al.* 1990) in the discrimination of /dæ/ and /tæ/, which differ in the duration of voice onset time (VOT; the duration between the release of the stop closure on /t/ and /d/ and the onset of voicing in the vowel). This time instead of varying pitch (cf. Aulanko *et al.* 1993), which is not the primary acoustic/phonetic contrast between /b/ and /g/, Phillips *et al.* modulated the duration of voice onset time, which is a primary cue in the distinction between /d/ and /t/ (Liberman, Delattre, and Cooper 1958). The VOT duration for /d/ is of the range 0 ms to 25 ms and /t/ is of the range 30 ms to 105 ms (Lisker and Abramson 1964). In a many-to-one oddball design, there is a many-to-one relationship at the phonological but not acoustic level of representation (cf. Sharma and Dorman 1999). Phillips *et al.* synthesized a series of stimuli along the /dæ/-/tæ/ continuum which varied in the duration of the VOT in 8 ms increments. In the first half of the experiment, 87.5% of the acoustic tokens were randomly sampled from one side of the category boundary continuum (12.5% from the continuum of the other side of the category boundary), and in the second half of the experiment, the majority of acoustic tokens (87.5% again)

were randomly sampled from the other side of the category boundary. Consequently, at the level of acoustic representation, there was no standard, as each successive stimulus presentation was acoustically distinct from the token that either preceded or followed. Phillips and colleagues reported a reliable MMN to the deviant, suggesting that listeners were able to construct a category representation at the phonological level for the standard despite the acoustic variation in the individual tokens. That is, listeners seemed to be able to perceptually group these acoustically distinct tokens together to form a category, and when they perceived a token from the other side of the category boundary, they were able to detect the change (as indexed by the MMN).

To demonstrate that listeners were constructing representations consistent with their linguistic representations and not simply grouping the tokens based on “long” or “short” VOTs, Phillips *et al.* (2000) conducted a follow-up whereby 20 ms of VOT were added to all the VOT values, such that now all the tokens had “long” VOTs, and there was no longer a many-to-one relationship at the phonological category level. They found no MMN, suggesting that in the earlier experiment, listeners were in fact grouping the standards together in a manner consistent with their linguistic categories. These results do not, however, necessarily point to a phonological explanation over a phonetic category explanation. In exemplar models (e.g. Pierrehumbert 2002), phonetic representations reflect Gaussian distributions along a number of acoustic phonetic parameters. Consequently, perception of a token that is sampled from a distinct phonetic category could give rise to the MMN reported in Phillips *et al.* (2000). An additional, alternative explanation is one based entirely on neurophysiology. The categorical boundary in English stop consonants is roughly 30 ms VOT. The idea is that this VOT boundary is a consequence of auditory neuron response properties to independent acoustic events occurring in quick succession (Sinex and McDonald 1988, 1989; Steinschneider *et al.* 1995). Certain groups of auditory neurons respond to both the noise burst of the stop consonant and the onset of the voicing of the vowel. The refractory period for some of these neurons is roughly 30 ms, which lines up well with the typically cross-linguistically observed VOT durations.

One of the core definitional properties of being phonological in nature is the direct relationship to meaning (Halle 2002). The phoneme, a unit of representation undeniably phonological, is traditionally defined as the smallest unit of linguistic representation that can serve to distinguish lexical meaning. Consequently, it is important to compare contrasts that do and do not serve to distinguish lexical meanings in different languages. Kazanina *et al.* (2006) compared speakers from two languages in a mismatch experiment nearly identical to Phillips *et al.* (2000). The novelty of this experiment is that Kazanina *et al.* tested both Russian and Korean speakers on the /d/-/t/ continuum, speakers of languages which differ in

the phonemic status assigned to /t/ and /d/. In Russian, both /t/ and /d/ have phonemic status: [tom] “volume” and [dom] “house” are two distinct lexical entries. In Korean, however, /t/ and /d/ share an allophonic relationship and appear in complementary distribution. The voiced allophone [d] occurs intervocally (e.g., /paTa/ → [pada] “sea”), whereas the voiceless unaspirated counterpart occurs word-initially (e.g., /Tarimi/ → [tarimi] “iron”). Thus, both languages share a bimodal distribution of /d/ and /t/ at the phonetic level. The expectation, then, is that in a many-to-one oddball design, if participants are constructing a purely phonological representation of the standard, we expect to find an MMN in the Russian participants but not in the Korean participants, because these sounds are represented independently at the level of phonology in Russian but not in Korean. They reported a reliable MMN for the Russian participants, and no reliable difference in the RMS of the MEG temporal waveform for the Korean participants in any time window between 20 ms and 340 ms. Unlike the findings from Phillips *et al.* (2000), the results in Kazanina *et al.* (2006) are considerably more difficult to explain by appealing to phonetic category distributions alone. Given that both [t] and [d] occur phonetically in Korean and Russian, if participants were simply constructing a phonetic representation of the standard, then both the Korean and Russian participants should show an MMF to deviant stimuli. One caveat, however, is that given that Korean listeners rarely, if ever, hear [d] word-initially, their failure to elicit an MMF to the deviant tokens in the experiment could be explained by the fact that [d] never occurs word-initially in Korean, and therefore, word-initial [d] does not exist in the phonetic distribution in the language (see Silverman 2006 for the idea that allophonic variants are stored together with their contextual licensing environments). The fact that allophones occur in complementary distributions makes it difficult to design a completely balanced experiment.

Gomes *et al.* (1995) suggest that listeners can perceptually group standards in an oddball MMN design along one particular physical dimension that all the standards share. If this interpretation is correct, then the MMN is an excellent tool for investigating the representative nature of distinctive features. In the only experiment of which we are aware, Yeung and Phillips (2004) asked if participants would be able to perceptually group relatively disparate standards sharing one distinctive feature (i.e. [+voice]). In 37.5% of the trials, they heard /baɪ/, and in another 37.5% of the trials they heard /gæ/. In 12.5% of the trials they heard the pseudo-deviant /dæ/, and in the final 12.5% of the trials they heard the deviant /tæ/. The consonants /b/, /d/ and /g/ all form a natural class: voiced stop consonants. While the consonant /t/ is also a stop, it is produced without vibration of the vocal folds in the glottis. Therefore, in an oddball paradigm, if listeners can construct representations of the standard at the level of the distinctive feature, then they predict to find an MMF to the /tæ/ syllable and not the /dæ/ syllable even though their

likelihood of occurrence is identical. However, this design is heavily dependent on the exact details of the feature theory. For example, /b/ and /g/ can form a natural class to the exclusion of /d/ if assuming a feature such as [peripheral] or [grave] (Jakobson, Fant, and Halle 1952). If the standard is then [+voice]; [+grave], then both /d/ and /t/ would be appropriate deviants. Ultimately, Yeung and Phillips found significant effects in the mismatch region between standards and deviants and also failed to find a difference between the standard and pseudo-deviants in any region. Curiously, however, they also found an effect in the mismatch time window for the acoustic condition (cf. Phillips *et al.* 2000). This result makes these findings difficult to interpret, but highlight quite nicely the power of the MMN/MMF paradigm in trying to assess the representational nature of speech sounds. More studies using this design are encouraged.

13.5.3 Underspecification

Investigating a perhaps more nuanced linguistically motivated hypothesis, Eulitz and Lahiri (2004) used the MMN to test whether phonemic representations in the lexicon are underspecified for non-contrastive distinctive feature values in the language. They used the German vowels /e/, /ø/ and /o/ in an oddball paradigm with German participants. It has been assumed that the feature [coronal] is not specified in the phonological lexicon (Archangeli 1988; Lahiri and Reetz 2002). Under this hypothesis, then, the vowel /e/ is underspecified for its place of articulation in the phonological lexicon, while /o/ is specified for both [dorsal] and [labial], since it is both round (i.e. [labial]) and back (i.e. [dorsal]). Given that /ø/ is both front and round, it is specified for [labial] but underspecified for [coronal]. The comparison of interest lies in the /o/-/ø/ pair. When /o/ is the standard and /ø/ is the deviant, a conflict at the level of phonological representation occurs. This is because the [coronal] feature extracted from the auditory signal of /ø/ mismatches with the stored representation of [dorsal] for the standard /o/. A contrast should not occur in the opposite direction because [coronal] is omitted from the lexical representation. If /ø/ is underspecified for its place of articulation, then the constructed representation of the standard does not contain a specification for place, and therefore, the specified [dorsal] feature on /o/ would not conflict. For the /e/-/ø/ pair, since neither is specified for place of articulation, no conflict should exist at the level of phonological representation. Therefore, they predict a larger MMN when /o/ is the standard and /ø/ is the deviant compared to when /ø/ is the standard and /o/ is the deviant. Moreover, they predicted no difference in the /e/-/ø/ pair. They found a clear MMN component in the grand average waveform for all conditions. There was no difference in the latency or amplitude of the MMN for the /e/-/ø/ pair. That is, an approximately equivalent MMN was elicited irrespective of

which phoneme was the standard and which phoneme was the deviant. They did, however, find a differential MMN in the /ø/-/o/ pair: a larger and earlier MMN when /o/ was the standard and /ø/ was the deviant than in the opposite configuration. That is, despite the fact that the acoustic difference is identical, a larger and earlier MMN is elicited in one standard/deviant configuration than the other, suggesting that the MMN is indexing more than just the physical properties of the stimulus. Eulitz and Lahiri (2004) suggest that these findings support the predictions of a featurally underspecified lexicon model (Lahiri and Reetz 2002, 2010), whereby some features, those that do not play a contrastive role in lexical representation, are not phonologically specified.

More recently, Hacquard, Walter, and Marantz (2007) exploited the MMF to investigate the role of vowel inventory and size on the perception of vowels. The size of a vowel inventory within a given language influences the acoustic consequences of articulation. In particular, it has been reported that languages with larger vowel inventories also tend to have a larger acoustic vowel space relative to languages with smaller vowel inventories (Bradlow 1995). To understand the influence of vowel inventory size and organization on perception, Hacquard *et al.* (2007) tested native speakers of Spanish and French in an oddball MMF paradigm. The vowel spaces of Spanish and French differ on both their size and organization: (1) Spanish is a five-vowel system, while French has 12 vowels (including the five vowels found in Spanish) and (2) French has a series of vowels intervening in F2/F1 space between the vowels of Spanish (e.g., /ɛ/ intervenes between /e/ and /a/; /ɔ/ intervenes between /o/ and /a/). They hypothesized that if inventory organization had an effect on perception, then the effect of the MMF should be roughly equivalent for the /o/-/u/ pair as the /o/-/a/ pair in Spanish. That is because these two pairs are equally distant in terms of intervening vowels. In French, however, since /ɔ/ intervenes between /o/ and /a/, they predicted the MMF to be larger in the /o/-/a/ pair than in the /o/-/u/ pair if inventory size played a role. Across languages, if inventory organization was the primary factor in perception, no difference would be predicted in the magnitude of the MMF for the /o/-/u/ pair between the French and Spanish pair, while a difference would be predicted between the /o/-/a/ pair, since French has an intervening vowel category and Spanish does not. If, on the other hand, inventory size was the primary factor driving the MMF response, Hacquard *et al.* expected the French participants to show a larger MMF across the board, since French has the larger vowel inventory. What they found was that a larger MMF was elicited in French participants compared to Spanish participants for all vowel comparisons except the /ɛ/-/e/ pair, suggesting that vowel inventory size (the number of vowel categories in the language) and not inventory organization affects the perception of vowels. Hacquard *et al.* took this particular pattern of results to support a model of expanding vowel space whereby point vowels (e.g., vowels on the edge of the vowel space) are

produced more distinctly from one another than vowels more centrally located in the perceptual space.

In sum, the MMN/MMF has proven to be an extremely powerful tool in assessing the types of auditory and linguistic representations supported by auditory cortex. While the ultimate focus of these studies was to investigate the available representations supported by auditory cortex, as well as properties of phonetic category distributions and native language inventories, they also serve to demonstrate that the MMN indexes abstract properties of the stimulus. It should also be noted that the MMN oddball paradigm has been used to investigate the role of phonological constraints on syllable structure in native and non-native speech perception (Dehaene-Lambertz, Dupoux, and Gout 2000), the nature of lexical access (Assadollahi and Pulvermüller, 2003; Shtyrov and Pulvermüller 2002a, 2002b) and certain aspects of syntactic processing (Pulvermüller and Shtyrov 2006; Shtyrov *et al.* 2003). Designing experiments geared toward directly testing the status of phonological features, *per se*, remains challenging.

13.6 Phonotopy

The most intuitive neural coding scheme in any domain of cognition is a topographical one, the most famous example being retinotopy in vision (Palmer 1999). The neurons in primary visual cortex are mapped in such a way that brain space corresponds to retinal space. For example, two neurons in neighboring areas of cortex each represent neighboring parts of the retina. This kind of coding is known as place coding, because the significance of a particular neuron firing can be straightforwardly related to the region of cortex in which it is located. Place coding is probably the easiest neural coding scheme to detect with non-invasive neuroimaging techniques, and we can look for place coding in any domain in which we believe the representations to be ordered along some kind of semi-continuous stimulus dimension. In the auditory domain, we know that receptors in the cochlea are organized “tonotopically,” i.e. along a frequency gradient (Schnupp, Nelkin, and King 2010). Tonotopic mapping is preserved in the projection to auditory cortex (Formisano *et al.* 2003; Talavage *et al.* 2004; Langers *et al.* 2007; Humphries *et al.* 2010) both in individual neurons and in aggregations of neurons. Using MEG it has been shown that the estimated location of the source of the M100 auditory response to a tone depends on the tone’s pitch along a medial-lateral dimension (Pantev *et al.* 1989; Huotilainen *et al.* 1995; Langner *et al.* 1997).

Since the neural representation of basic acoustic properties appears to use place coding, a natural question is whether this coding continues to be used as the representations to be encoded become more complex and speech-specific. Phonemic representations have often been defined by virtue of their position along a number of proposed featural dimensions,

most notably the vertical and horizontal position of articulators within the mouth. Might phonemic representations therefore be subject to place-coding as well? Many studies have examined this question in recent years, mainly by using MEG to localize the M100 response to vowel or syllable presentation. The most consistent finding across these studies is that early MEG responses localize along an anterior-posterior gradient in STG according to the place of articulation on the front-back dimension (e.g., Diesch *et al.* 1996; 2000; Obleser *et al.* 2003; 2004; Scharinger *et al.* 2010). For example, Obleser *et al.* (2004) examined the M100 response to German vowels [a], [e], [i], [o], [u], [ø], and [y]. They showed that the M100 response to front/coronal vowels such as [i], [y], [ø] and [e] localized to a more anterior position than the response to back/dorsal vowels such as [u] and [o]. Convergent results have been observed in fMRI (Obleser *et al.* 2006).

Although these kinds of results show that the location of neural responses tracks featural dimensions that have been proposed by phonological theory, a similar pattern of results could arise from a place code that is purely acoustic in nature. Speech sounds are spectrally complex, of course, and featural dimensions proposed on articulatory grounds have acoustic correlates in the form of frequency shifts across primary formants like F1 and F2. In this way, what looks like “phonotopy” may simply be multidimensional “tonotopy.” In order to show that the spatial selectivity observed reflects the encoding of linguistic representations, one must demonstrate that the topographical pattern of activity cannot be explained by acoustic parameters alone. Several of these studies have made progress toward this by showing that a model including phonological features achieves a better fit to the observed localizations than a purely acoustic model (Diesch *et al.* 1996; Obleser *et al.* 2004). For example, Scharinger *et al.* (2011) mapped the M100 response for the entire vowel space of Turkish and observed spatial gradients that could capture the featural dimensions of height, place of articulation, and roundedness. However, the map for front vowels was oriented orthogonally to the map for back vowels. The authors show with statistical modeling that while the cortical maps reflect acoustic properties of the signal, the map is warped toward linguistically relevant categories. This suggests that place-based encoding of language-specific phonemic representations may develop on the basis of experience from “tonotopic” encodings that are originally purely acoustic. More practically, this study indicates that future experiments in other languages may need to more exhaustively probe their vowel spaces in order to correctly interpret how the topographical pattern maps onto the linguistically relevant feature dimensions.

13.7 Phonological processes

To this point, we have primarily focused on the nature of phonological representations, the primitives that have been investigated to date in

the cognitive neuroscience literature. The other half of phonology has concerned itself with the processes that operate over these representations. Far less work has been conducted on the cognitive neuroscience of phonological processes, and consequently, we have less to say about these issues. They do, however, remain ripe for further investigation. Electrophysiological techniques have been used to investigate the neural time course of violations of phonological rules/phonotactic restrictions (Flagg, Oram Cardy, and Roberts 2006; Mitterer and Blomert, 2003; Tavabi *et al.* 2009). For example, Flagg and colleagues (2006) exploited the fact that pre-nasal vowels are nasalized in English and measured the electrophysiological latencies of the response peaks in MEG to congruent (i.e. [aba], [ãma]) and incongruent (i.e. [äba], [ama]) VCV sequences. Overall, latencies in the time-window of 50–100 ms post-onset of the consonant were shorter for the congruent as opposed to incongruent sound sequences.

In an oddball detection experiment, Mitterer and Blomert (2003) found in Dutch listeners that violations of expected nasal place assimilation patterns between words elicited a larger mismatch negativity response (MMN, approximately 100–200 ms after onset of the consonant) than did adherence to nasal place assimilation patterns. These results suggest that although cortical responses that reflect phonological processing are early, the types of responses measured (latency versus amplitude) and the time-window in which differences were found varied.

In recent MEG work (Monahan, Hwang, and Idsardi 2008) based on previous behavioral findings (Hwang, Monahan, and Idsardi 2010), we found a reliable difference between consonant clusters that were congruent in their voicing specification (i.e. [utz]) with those that were incongruent (i.e. [uds]) as early as 150 ms post-onset of the violating segment, in this case, the fricative (i.e. [s]). We interpreted these differences to suggest that listeners can exploit their knowledge of phonological processes and representation to constrain early perceptual parses of the sensory input, as well as using this detailed knowledge to serve as the basis for generating hypotheses and predictions about the nature of the upcoming speech signal (see Poeppel and Monahan 2011 for more discussion on how we believe these results fit into a larger, analysis-by-synthesis architecture).

13.8 Conclusions

Although a vast amount of work has examined the biological basis of speech perception and speech production, a critical missing piece has been our lack of knowledge about the biological basis of the phonological representations that map between lower-level sensory and motor representations to more abstract lexical representations. We believe that

linguists have an important role to play in the effort to better understand the neural implementation of phonological knowledge, and that phonology may in fact be a more fruitful starting place than syntax for the general biolinguistic enterprise. Here, we have reviewed some recent work that makes some beginning steps forward in this direction and hope that in doing so we have encouraged others to continue to expand this effort.

Appendix: Cognitive neuroscience methodologies

Acceptability judgments and various fieldwork methods have been the preferred methods for the field of phonology over the past several decades. In the domain of behavioral psycholinguistics, a variety of tasks and methodologies have been employed to understand the psychological processes that underlie spoken word recognition (see Grosjean and Frauenfelder 1996 for a collection of papers on the various techniques in the field), and by and large, the primary dependent measures in those tasks are reaction time data, accuracy, or normalized discriminability scores (e.g., d' ; see Macmillan and Creelman 1991 for review). While we advocate the position that no type of measure or technique is inherently better than any other and that an interpretable answer to any problem requires a combination of the right question with the appropriate technique, the techniques of cognitive neuroscience may remain less familiar than others to some readers of this volume. Therefore in the following we briefly introduce the primary techniques used in this field.

A.1 Aphasias and patient-work

Overview

Neuropsychological studies of patients with language deficits are one important source of evidence. In these studies, investigators develop tasks to carefully probe those aspects of linguistic knowledge or processing that are relatively preserved and those that are not. If one observes that performance on two seemingly independent tasks are typically correlated in patients, one can conclude that the same brain area is likely responsible for task performance, even before determining which brain area is recruited; conversely, a dissociation in performance implicates a dissociation in regions responsible for performing those tasks. Much of the early work proceeded in this way, with investigators identifying deficits that tended to cluster together or dissociate. Given the current prevalence of MRI, most neuropsychological studies additionally include measures that attempt to determine exactly which brain area is responsible for the deficits. Structural MRIs of numerous patients presenting with various patterns of linguistic deficits can be conducted, lesions can be identified

in each, and the number of lesions in a particular area can be correlated with behavioral measures in a method known as voxel-based lesion mapping (e.g., Dronkers *et al.* 2004). Perhaps the key benefit of neuropsychological studies in understanding language in the brain is that they permit at least limited conclusions about causality. With measures of brain activity, the absence of observed activity in an area could always be due to the insensitivity of the technique; the presence of observed activity could reflect some process correlated with the process of interest. However, with patient studies, one can conclude that a particular brain area is in some way crucial for performing a task because when that area is damaged or not functioning, the patient simply cannot do the task.

Practical issues

Neuropsychological studies require a very good theory of the processes and knowledge required for performing the tasks that compose the dependent measure (see Hickok and Poeppel 2004 for a discussion of how misunderstanding of the task may have led to incorrect conclusions about the brain areas involved in phonological processing). The small sample sizes and the heterogeneity of the brain areas damaged and the deficits observed across patients frequently make it difficult to extrapolate findings to the general population.

A.2 Electroencephalography (EEG)

Overview

Perhaps the most widely used measure of brain activity, EEG is typically conducted with an array of electrodes attached to a cap that is placed on the scalp. Salt water or water-soluble gel is used to improve the connection between the scalp and the electrodes, and then the voltage between each electrode and a reference electrode is recorded. Momentary neural events like action potentials (~1 ms) or extremely local changes in activity (across a few neurons) are unlikely to sum to a change in potential large enough to be observed at the scalp. Therefore, changes in the EEG are thought to mainly reflect modulatory activity across entire neuronal populations in rough synchrony (Nunez 1981; Luck 2005). In cognitive studies, one is often interested in the brain response to stimuli (events) presented at particular points in time, so one might present many events and average the EEG in the time-window following each event; the resulting waveform is referred to as an event-related potential (ERP), and studies that focus on changes in the average EEG response to a stimulus are therefore often referred to as ERP studies. As a measure for probing phonological processing, EEG has a number of benefits. In contrast to behavioral measures like reaction times, an ERP study does not require an explicit task, which can alleviate concerns of invoking unnatural processing strategies. Moreover, the time-sensitivity of the technique is excellent. With sufficient statistical

power, one can detect differences on the order of a few milliseconds, or more commonly, a few tens of milliseconds, and therefore, it is one of the best techniques available for investigating questions about the time course of processing.

Practical issues

Setting up an EEG lab is relatively inexpensive (~ \$30,000), and the only continuing costs are minor supplies for set-up and replacing the caps (\$500–\$1000) that hold the electrodes and are worn by participants after several hundred uses. The analysis methods, while requiring some training, are easier to grasp and are less computationally intensive than those for fMRI, and are more standardized (and therefore easier to build on previous results and to get published) than those for MEG. Therefore, EEG can be a good choice for new investigators. A well-known weakness of EEG is that it does not provide reliable information about the location of changes in neural activity. This is because the conductive layers of skull and tissue between the brain and the electrodes tend to smear electrical activity. Some degree of source localization can be achieved if one records from a greater number of electrodes, but these methods are still under development and replication of results has been challenging.

A.3 Magnetoencephalography (MEG)

Overview

MEG measures fluctuations in magnetic field strength recorded externally from the scalp. Like all electrical signals, the changing electrical current measured by EEG is accompanied by magnetic fields. Thus, MEG captures the same kind of synchronous modulatory neural activity, and as a consequence, it has the same excellent temporal resolution; however, the relationship of the relative orientation of the source of the electromagnetic potentials/fields to the scalp with the optimal orientation for the sensitivity of the techniques is slightly different (Hansen, Kringelbach, and Salmelin 2010). While EEG records electrical activity oriented in any direction, MEG is blind to activity in certain orientations, in particular magnetic dipoles that are radial to the surface of the scalp. For the same reason, MEG is less sensitive to activity deeper in the brain, in subcortical areas. Although this seems to suggest that EEG “sees more” than MEG, the fact that more activity is visible to EEG also means that EEG is more susceptible to cancellation between sources (activity in two areas simultaneously with opposite orientation will sum to zero at the scalp). The most well-known benefit of MEG compared to EEG is that it provides better information about the location of neural activity because, unlike electrical fields, magnetic fields are not distorted by intervening tissue between the electric activity and sensors/electrodes. For example, even without

sophisticated source localization methods, in MEG it is often possible to reasonably conclude from observing greater activity in left hemisphere sensors that there is greater neural activity in the left hemisphere.

Practical issues

MEG setup is typically much faster and more comfortable for participants, and requires less training for the experimenters. In MEG the magnetic sensors are arranged in a fixed helmet, because they must be constantly cooled by liquid helium. Unlike EEG, they do not need to be physically attached to the participant, and there is no need to create a conductive bridge between the sensor and the scalp. A major downside of MEG is its cost; start-up can be as much as \$4,000,000, in large part due to the costs associated with the magnetically shielded room, and the liquid helium required to maintain the machine is approximately \$50,000 annually. Analysis is much less standardized than in EEG, and therefore often requires a much greater time investment for the beginning investigator who may need to develop individualized analysis procedures. Source localization, while more accurate than for EEG, is still a computationally ill-formed problem with an infinite number of solutions, and it is still largely unknown how well current methods do in cases in which multiple sources are active simultaneously. Collecting a separate structural MRI is also necessary for accurate source localization in EEG or MEG, which further increases cost and complexity. However, it is important to remember that although MEG is often touted on the basis of its improved localization, MEG measurements can still be very informative about the time course of processing even without precise source localization analyses.

A.4 Functional magnetic resonance imaging (fMRI)

Overview

fMRI is the most widely available tool for localizing neural activity to specific brain regions. Different types of biological tissue and substances differ in magnetic susceptibility, and MRI can use these differences to create maps that highlight the location of the tissue or substance of interest. Functional MRI typically measures the blood-oxygen-level dependent (BOLD) signal, the change in magnetic susceptibility that arises when the blood oxygen level changes. It is known that local neural activity results in systematic changes in blood oxygenation, and therefore changes in BOLD in a given brain region are often taken as a proxy for changes in neural activity in that region. However, one limitation of fMRI is that the exact nature of the relationship between BOLD and neural firing is still not completely understood (see Huettel *et al.* 2004 for an introduction to this issue), and many factors such as the proximity of large blood vessels and excitatory versus inhibitory firing may complicate the interpretation of

BOLD changes. Because blood flow is relatively slow, the peak of a stimulus-evoked increase in BOLD occurs about 6 seconds after the item is presented, and temporal precision is poor for the same reason, so that fMRI is generally not a good method for estimating the time-course of processing. However, no other non-invasive technique can currently match fMRI in spatial precision. Unlike MEG source localization, which is only as accurate as the model, fMRI delivers an unambiguous measurement of magnetic susceptibility for each unit of space in the measurement. This means that fMRI is often still the best choice for addressing “where” questions.

Practical issues

In the past, one particular challenge for fMRI work on phonology has been that the shifting magnetic gradients that are required for imaging are very loud, thus inducing noise and possible confounds in studies using auditory stimuli. Studies of phonological production are similarly challenging because changes in articulator position can result in time-locked artifacts. However, clever imaging sequences are now available in which acquisition is delayed until several seconds after stimulus presentation or production, thus mitigating some of these problems. Because of both cost and the safety issues involved in working in an environment with a powerful magnet, most MRI machines are maintained by large institutions rather than individual laboratories. At the same time, there are now so many MRI machines for medical use that researchers in developed countries can usually find one nearby that they can arrange to use during off-hours. Although the cost per hour for MRI use is often high (~\$500), many institutions will allow researchers with a well-developed proposal to collect pilot data at little or no cost, which can be used to motivate grant proposals that would allow completion of the project. fMRI data analysis is computationally intensive and less intuitive than ERP analysis, but it is currently more standardized and better documented than MEG analysis.

A.5 Alternative methods

Several other techniques have also been used to probe brain activity associated with phonological processing. Positron Emission Tomography (PET) can measure brain metabolism of different compounds through the use of radioactive tracers; for example, one could measure the rate of glucose metabolism in different brain areas while participants perform cognitive tasks in order to determine which areas become more active for which tasks. Some benefits of PET for phonological studies are that it provides fairly good spatial precision but is not noisy to acquire like fMRI and is not disrupted by articulator movement. Some drawbacks are that only block designs can be used with PET (as it takes a number of minutes for the tracer to leave the system), and high cost, technical difficulty in

working with short-lived radioactive tracers, and radiation exposure to participants. Near Infrared Spectroscopy (NIRS) uses infrared light to measure changes in blood oxygenation associated with neural activity. It is less widely available than fMRI but is beginning to be used more frequently for studies of processing in infants, because of safety concerns in exposing infants to strong magnetic fields, the reduced acoustic noise compared to fMRI, and less susceptibility to movement artifact. Event-related optical signaling (EROS) is an even newer method that also uses infrared light but measures changes in the optical properties of active neurons themselves, thus increasing temporal resolution.

14

Computational primitives in syntax and possible brain correlates

Matthias Schlesewsky and
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14.1 Introduction

In spite of the many debates that continue to revolve around language, two assumptions appear relatively uncontroversial: (a) language is structured; and (b) it is ultimately a product of neural processes, i.e. the result of interactions between neurons and neuronal assemblies. The first point is supported by the observation that communication does not seem to proceed arbitrarily in any human language. In other words, the correspondence between a complex linguistic form (e.g. a sentence) and its respective meaning is both systematic and, at least to a certain degree, compositional (Fodor and Pylyshyn 1988). With regard to the second point, one might, trivially, ask how things could be otherwise. In addition, however, it has been known for thousands of years that pressure applied to certain parts of the brain significantly impacts upon an individual's capacity to produce coherent utterances (see, for example, the Edwin Smith surgical papyrus; Wilkins 1992) and this correlation between certain brain regions/networks and language production or comprehension has, of course, been substantiated by more recent research on the neurobiology of language (e.g. Geschwind 1970). The challenge, then, lies in bringing these two assertions together in order to understand the correspondence between linguistic structure and neuronal mechanisms. Yet as it turns out, this enterprise is far from trivial.

In our view, the problem is (at least) threefold. Firstly, the establishment of correspondences between “computational primitives in syntax” and their “brain correlates” presupposes that we know what these computational primitives are and what might count as a suitable brain correlate. Both issues are controversial – at least to a certain degree. On the one hand, the identification and/or definition of a computational primitive is heavily dependent on the particular syntactic theory which one adopts. On the other hand, since the brain operates at a vast range of topographical and temporal scales (ranging from single neurons to neuronal assemblies in

certain brain regions or neural networks spanning a number of regions; from milliseconds to changes over the lifespan), the identification of what might count as a neural correlate of an assumed computational primitive is impeded by the difficulty of determining an adequate grain size for such a correlate.¹ Secondly, even under the assumption that adequate categories had been established on both sides of the equation, the mapping between them would nevertheless present a challenge. One reason for this lies in what is often, in a computer-based analogy, described as the software vs. hardware distinction: linguistic categories and processes (or, more generally, cognitive categories and processes) likely do not map directly onto basic neural mechanisms, but rather onto a complex interplay between a number of these basic mechanisms. The second difficulty stems from the fact that the neural correlates of language cannot be considered in isolation: language is always encountered or produced within a particular environment and thereby associated with a specific goal or task space. Thus, any measurable correlates of language comprehension or production occur in concert with – or at least alongside – measurable correlates of other cognitive processes that allow us to deal with the task at hand (e.g. the orientation of attention or the inhibition of irrelevant information). Thirdly, while linguistic theories are typically formulated independently of the temporal dimension (“[A] generative system involves no temporal dimension” [Chomsky 2007d: 6]), language production and comprehension necessarily proceed in time. Thus, there is a dimensionality mismatch between the neural correlates of language and theoretical considerations.

Since the focus of this chapter is on linguistic operations, we will begin by discussing possible candidates for primitives of syntactic computation and the state of the art with regard to their possible neurobiological correlates (Sections 14.2 and 14.3). Perhaps not unexpectedly in view of the caveats outlined above, this discussion will reveal that many operations that have been considered in this regard do not explain the overall data pattern in a satisfactory manner. Rather, as described in Section 14.4, many of the existing neuroimaging findings on “syntactic processing” appear to be more parsimoniously explained in terms of more general cognitive mechanisms (specifically: cognitive control). Finally, Section 14.5 discusses Merge (or a similar structure-building operation) as the most promising current candidate for a neurobiologically implemented primitive of syntactic computation and describes approaches that can bridge the gap between grammar and processing.

14.2 Combining elements

Independently of theoretical persuasion, most syntacticians would agree that syntactic structuring involves the combination of elements to form larger constituents, i.e. the combination of A and B to form C, where A and

B can themselves be internally complex. This idea has perhaps been articulated most clearly in the context of the operation Merge in Minimalist syntax:

An elementary fact about the language faculty is that it is a system of discrete infinity. Any such system is based on a primitive operation that takes n objects already constructed, and constructs from them a new object: in the simplest case, the set of these n objects. Call that operation Merge. Either Merge or some equivalent is a minimal requirement. With Merge available, we instantly have an unbounded system of hierarchically structured expressions. Chomsky (2005: 11)

Of course, not all of the properties of Merge carry over to “combining operations” in other theories. On the one hand, the question of whether syntactic objects involve hierarchical structure “all the way down,” i.e. at each level of combination, has long been controversial, especially in view of languages with a considerably freer word order than English or related languages (for discussion, see Van Valin and LaPolla 1997). On the other hand, not all theoretical accounts adopt the assumption that structures are composed derivationally (as is the case with Merge). Rather, they assume that the internal structure of complex syntactic objects is established declaratively via *unification*, which is essentially a case of structure sharing (e.g. Head-driven Phrase Structure Grammar, HPSG; Pollard and Sag 1994). In other approaches (e.g. Role and Reference Grammar, RRG; Van Valin 2005), syntactic structures are established via the (unification-based) combination of precompiled *syntactic templates*.

Abstracting away from the precise details of these different perspectives, the following section will review neuroscientific research on possible brain correlates of a simple structure-building operation. For concreteness’ sake, we will begin by using Merge as a model of such an operation (Section 14.2.1). Indeed, many of the relevant experimental studies also based their designs on the assumption of an operation that is equivalent – or at least very similar – to Merge. Subsequently, we will explore the possibility that different subcases of Merge may be associated with measurably distinct neural correlates (Section 14.2.2). Finally, we will extend the perspective to other possible operations which serve to combine elements and establish structured syntactic representations (Section 14.2.3).

14.2.1 Possible neural correlates of Merge

Perhaps the simplest approach that has been adopted in order to study the neural correlates of sentence structuring has been to compare sentences against word lists. The rationale behind this manipulation is that word lists can serve as a suitable control for sensory and word-level aspects of processing such that a contrast between sentence and word list processing

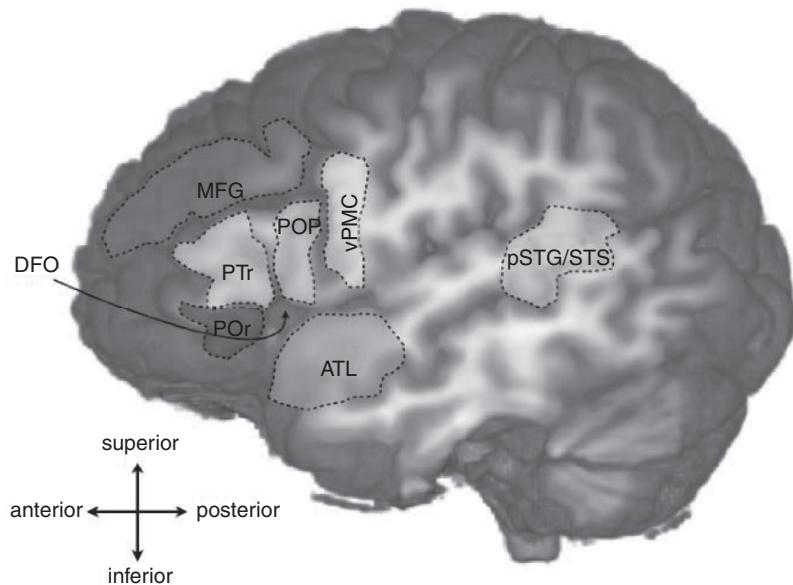


Figure 14.1 Lateral view of the left hemisphere of the human brain including a schematic depiction of the brain regions discussed in this chapter. Abbreviations are explained in the list of abbreviations in the appendix.

should isolate correlates of sentence-level structuring. In a series of neuro-imaging studies, this comparison revealed (primarily left-lateralized) activation in the anterior temporal lobe (ATL) for sentences in comparison to word lists (Mazoyer *et al.* 1993; Bottini *et al.* 1994; Stowe *et al.* 1998; Friederici, Meyer, and von Cramon 2000; Xu *et al.* 2005), thus rendering the ATL a possible candidate for a neural correlate of basic syntactic structuring (see also Dronkers *et al.* 2004, for similar conclusions on the basis of deficit-lesion correlations in a patient study). The location of the ATL – and of the other brain regions discussed in this chapter – is shown in [Figure 14.1](#). However, since sentences not only differ from word lists in that they contain syntactic structure, the ATL activation observed in these studies could potentially reflect various aspects of sentence-level processing (e.g. sentence-level semantic composition). Indeed, on the basis of further studies with more fine-grained manipulations (e.g. of semantic coherence or prosodic contour), several scholars have argued that the sentence-sensitivity of the ATL involves semantic (Vandenbergh, Nobre, and Price 2002; Rogalsky and Hickok 2008) and prosodic contributions (Humphries *et al.* 2005).

One possible explanation for the full range of findings on the ATL in sentence processing could therefore be that this cortical region subserves a range of sentence-level mechanisms, which might be dissociable if more fine-grained neuroanatomical differentiations were undertaken. Alternatively, it may be the case that the function of the ATL in sentence processing is more general, i.e. goes beyond the basic syntactic combination

of elements to form larger constituents or hierarchically structured (and potentially unbounded) expressions.² In spite of these concerns, however, the hypothesis that there may be a specific connection between the ATL and basic processes of syntactic composition continues to attract a considerable degree of attention; it also appears to receive converging support from new paradigms. In a study using magnetoencephalography (MEG), for example, Bemis and Pylkkänen (2011) observed ATL activation only when composition involved syntactic structuring (e.g. when participants read the word “boat” as part of a string such as “red boat” rather than following a nonword as in “xkq boat”). By contrast, as reported by Bemis and Pylkkänen (2010), no such activation is engendered by non-linguistic stimuli involving the same semantic content (e.g. a colored line drawing of a red boat as opposed to a line drawing of a boat without a color fill).

An alternative line of research that can be viewed as relevant with regard to the possible brain bases of Merge has sought to approach the possible neural correlates of “an unbounded system of hierarchically structured expressions” (Chomsky 2005: 11, see above). This approach was sparked by the claim that the “[faculty of language in the narrow sense] only includes recursion and is the only uniquely human component of the faculty of language” (Hauser, Chomsky, and Fitch 2002: 1569). In order to examine the possible neural underpinnings of this proposal, Friederici and colleagues (Friederici *et al.* 2006a; Bahlmann, Schubotz, and Friederici 2008; Makuuchi *et al.* 2009) conducted a series of neuroimaging studies in which they compared recursive structures, i.e. structures involving self-embedding, with structures in which there was no embedding. The first study of this type (Friederici *et al.* 2006a) presented participants with two different types of artificial grammars, one of which involved embedding (recursion) and one of which did not. Following a study by Fitch and Hauser (2004), the recursive grammar was implemented by presenting syllable sequences of the type Aⁿ Bⁿ (e.g. AABB, AAABBB; assumed hierarchical structures: A[AB]B, A[A[AB]B]B etc.). The non-recursive grammar, by contrast, consisted of sequences of the type (AB)ⁿ (e.g. AB, ABAB, ABABAB).³ A functional magnetic resonance imaging (fMRI) study revealed that – when grammatical violations were contrasted with grammatical strings – the processing of both grammar types correlated with activation in the left deep frontal operculum (DFO), a structure deep to the lateral surface of the cortex, while only embedding grammars engendered additional activation in the lateral convexity of the left inferior frontal gyrus (lIFG), specifically within the pars opercularis (POp; Brodmann Area (BA) 44).⁴

In view of these findings, Friederici and her colleagues (Friederici *et al.* 2006a; Friederici 2009) argue for a functional-neuroanatomical subdivision between “hierarchical syntax” and “local syntax.” Specifically, they assume that, in the case of an artificial grammar involving embedding, sequences can only be classified correctly as grammatical (or ungrammatical) if the dependency between the different (“hierarchical”) levels of As

and Bs is recognized, while a non-embedding grammar only requires participants to learn the local transition between As and Bs.⁵ They link this functional-neuroanatomical dissociation to the evolutionary proposal advanced by Hauser *et al.* (2002) by noting that the POp is phylogenetically younger than the DFO, which resembles premotor cortex in terms of its cytoarchitectonic properties (see also Friederici 2004). Thereby, they argue that their data provide evidence for “an evolutionary trajectory with respect to the computation of sequences, from processing simple probabilities to computing hierarchical structures, with the latter recruiting Broca’s area, a cortical region that is phylogenetically younger than the frontal operculum, the brain region dealing with the processing of transitional probabilities” (Friederici *et al.* 2006a: 2461).

With regard to natural language processing, Friederici and colleagues argue that the distinction between hierarchical syntax and local syntax also applies to the dissociation between word order permutations and processes of intra-phasal syntactic structuring (see Friederici 2004, 2009). While word order variations in flexible word order languages such as German consistently correlate with activation in the IIFG and particularly in the POp (e.g. Röder *et al.* 2002; Bornkessel *et al.* 2005; Grewe *et al.* 2005; Friederici *et al.* 2006b; Grewe *et al.* 2006; Bornkessel-Schlesewsky, Schlesewsky, and von Cramon 2009), local phrase structure violations (e.g. an incorrect word category within a phrase) appear to engender activation with the DFO (e.g. Friederici *et al.* 2000; Friederici *et al.* 2003). More specifically, Friederici assumes that the processing of hierarchical syntax draws upon a “dorsal pathway” which connects the POp of the IIFG to the posterior portion of left superior temporal cortex, while local syntax is implemented in a “ventral pathway” which connects the deep frontal operculum to the anterior temporal lobe (Friederici 2009). Crucially, hierarchical syntax is also thought to be dissociable from general cognitive limitations such as working memory resources (Makuuchi *et al.* 2009).

At this point, however, a potential problem becomes apparent: How is the dissociation between local and hierarchical syntax (i.e. between local combinations and hierarchical or long-distance combinations, respectively) compatible with the definition of Merge? This is not at all clear, since, as defined above, Merge is a local operation, the iterative application of which serves to create hierarchical and potentially unbounded syntactic representations. Thus, it essentially encompasses both locality of structure building and the hierarchical aspect of structuring, which are assumed to be neuroanatomically subdivided in Friederici’s approach.⁶ However, if word order variations are taken as a point of departure for the non-local dependencies that form an essential part of Friederici’s “hierarchical syntax,” it appears possible that the neuroanatomical dissociation which she assumes could be attributed to the distinction between external Merge and internal Merge (Move). We will explore this possibility in the next section.

14.2.2 A neuroanatomical distinction between external and internal Merge (Move)? Evidence from the domain of word order permutations

While in older versions of Chomskyan syntactic theory, the base-generation of structure (Merge) was considered a distinct computation from the movement of elements to create non-local dependencies (Chomsky 1965, 1981b, 1995b), the operation Move is now assumed to be a subcase of Merge:

Unless some stipulation is added, there are two subcases of the operation Merge. Given A, we can merge B to it from outside A or from within A; these are external and internal Merge, the latter the operation called “Move,” which therefore also “comes free,” yielding the familiar displacement property of language. (Chomsky 2005: 12)

As is apparent from this definition, the dissociation between external Merge (EM) and internal Merge (IM) indeed appears potentially suited to deriving the neuroanatomical dissociations discussed in the preceding section. Specifically, the IIFG (POp) activation for word order variations could be viewed as a correlate of IM. As an illustration, consider the following sentence conditions, which Friederici *et al.* (2006b) contrasted in an fMRI study.

- (1) Example stimuli from Friederici *et al.* (2006b)
 - a. Heute hat der Opa dem Jungen den Lutscher geschenkt.
today has [the grandpa]_{NOM} [the boy]_{DAT} [the lollipop]_{ACC} given
“Today, the grandpa gave the boy the lollipop.”
 - b. Heute hat dem Jungen der Opa den Lutscher geschenkt.
today has [the boy]_{DAT} [the grandpa]_{NOM} [the lollipop]_{ACC} given
 - c. Heute hat dem Jungen den Lutscher der Opa geschenkt.
today has [the boy]_{DAT} [the lollipop]_{ACC} [the grandpa]_{NOM} given

For the sentence types in (1), Friederici *et al.* (2006b) observed a parametric increase of activation in the POp of the IIFG with increasing deviation from the basic word order: (c) > (b) > (a) (where > denotes “shows higher activation than”). From findings such as these, Friederici and Grodzinsky (2006) conclude that the IIFG (and particularly the POp) supports the processing of syntactic movement operations. In contrast to Friederici’s broader notion of “hierarchical syntax” (see above), Grodzinsky and colleagues indeed assume that this follows from a specialization (of subparts of the IIFG) for movement (Grodzinsky 2000; Ben-Shachar, Palti, and Grodzinsky 2004; Santi and Grodzinsky 2007a; Santi and Grodzinsky 2007b; Grodzinsky and Santi 2008).

Aiming to provide additional empirical support for this proposal, Santi and Grodzinsky (2007a, 2007b) directly compared the processing of syntactic dependencies that either involved movement (wh-questions, relative clauses) or did not (binding). In one study (Santi and Grodzinsky 2007b),

they used a 2×2 factorial design (see example 2) in order to be able to isolate the effects of a movement dependency.⁷ Their results revealed a main effect of movement in the IIFG (Brodmann area, BA 44), the left superior temporal gyrus (STG), and the inferior portion of the left precentral sulcus (i.e. the sulcus separating the POp from ventral premotor cortex, vPMC). By contrast, a main effect of binding was observed in more anterior and inferior portions of the IIFG (BA 45/47), right middle frontal and bilateral middle temporal regions as well as in the cingulate gyrus. Santi and Grodzinsky (2007b) interpreted these findings as evidence for a working memory (WM) module that is specific to syntactic movement.

- (2) Example stimuli from Santi and Grodzinsky (2007b).
 - (a) The girl supposes the cunning man hurt Christopher (~MOV, ~BIND)
 - (b) The girl supposes the cunning man hurt himself (~MOV, +BIND)
 - (c) Which older man does Julia suppose _ hurt the child (+MOV, ~BIND)
 - (d) Which older man does Julia suppose _ hurt himself (+MOV, +BIND)

At a first glance, these studies therefore appear to support the proposal that the IIFG – and particularly the POp (BA 44) – is the neural substrate underlying IM. However, when further results are considered, it becomes apparent that an explanation along these lines faces a number of empirical problems. The first, in fact, stems from a second study by Santi and Grodzinsky (2007a), in which they again compared movement and binding. Using relative clauses rather than wh-questions, they manipulated the distance between filler and gap and between reflexive and antecedent in a parametric design, thus yielding three levels of complexity (distance) for each type of dependency. They again observed differential activation patterns for movement and binding. However, with the exception of the binding effect in the right middle frontal gyrus, the localizations of the previous study did not replicate: an effect of movement was observed in the anterior IIFG (BA 45), which had previously shown an effect of binding, and there was no left inferior frontal effect for binding. While Santi and Grodzinsky (2007a) again conclude “that Broca’s area is specific to the WM needs of syntactic Movement rather than general to dependency relations” (p. 16), the inconsistency of the results across the two studies appears rather problematic for the assumption of a movement-specific neural substrate within (a subregion of) the IIFG.

A second empirical challenge for the proposal that IM correlates with activation in the (POp of the) IIFG stems from a range of further studies on the processing of word order variations in German (Bornkessel *et al.* 2005; Grewe *et al.* 2005; Grewe *et al.* 2006; Bornkessel-Schlesewsky *et al.* 2009). These experiments examined a number of factors which are known to influence argument order in the German middlefield (i.e. the medial portion of the German clause) and found that all of them (see (3) for a list) modulate activation within the left POp.

- (3) Linearization parameters that have been shown to modulate activation of the left POp in German (“>” should be interpreted as “precedes”)
- a. NOM > DAT > ACC
 - b. pronoun > non-pronominal argument
 - c. higher-ranking thematic role > lower-ranking thematic role
 - d. animate > inanimate
 - e. definite/specific > indefinite/unspecific

Crucially, not all of the linearization parameters in (3) (see, for example, Lenerz 1977; Müller 1999) can be derived via movement (IM). Accordingly, many recent theoretical accounts of word order in the German middlefield appeal to Optimality Theory in order to derive the application of these parameters (e.g. Müller 1999; Heck 2000). The clearest case is perhaps that of the animacy scale, which was examined in an fMRI study by Grewe *et al.* (2006) using sentence conditions such as (4).

(4) Example stimuli from Grewe *et al.* (2006)

- a. Dann wurde der Mantel dem Arzt gestohlen.
then was [the coat]_{NOM} [the doctor]_{DAT} stolen
“Then the coat was stolen from the doctor.”
- b. Dann wurde dem Arzt der Mantel gestohlen.
then was [the doctor]_{DAT} [the coat]_{NOM} stolen
- c. Dann wurde der Polizist dem Arzt vorgestellt.
then was [the policeman]_{NOM} [the doctor]_{DAT} introduced
“Then the policeman was introduced to the doctor.”
- d. Dann wurde dem Arzt der Polizist vorgestellt.
then was [the doctor]_{DAT}[the policeman]_{NOM} introduced

Grewe and colleagues observed increased left POp activation for sentences such as (4a) in comparison to (4b), i.e. *increased activation for subject-initial sentences in comparison to their object-initial counterparts*. They attributed this pattern of results to the violation of the animacy-based linearization principle (3d) in (4a) but not (4b). (Crucially, note that all sentences were grammatical. “Violation” in the sense used here is thus to be understood as the violation of a preference rather than of an inviolable syntactic rule.) A control comparison between sentences with two animate arguments (4c/d) did not reveal any inferior frontal activation differences. This result is very difficult to derive via movement: a movement-based explanation of the difference between (4a) and (4b) would need to assume that the dative-initial order in (4b) is the base order, with (4a) derived via an additional movement operation. While such a proposal has indeed been advanced for passivized ditransitive sentences such as those in (4) (e.g. Fanselow, 2000), it entails that (4c) and (4d) differ in the same way, since animacy is not generally considered a determining factor of an argument’s base position. Thus, the fact that (4a) shows increased POp activation in comparison to (4b) while there is no

concomitant difference between (4c) and (4d) calls a movement-based analysis of Grewe *et al.*'s (2006) findings into question.

It is important to note that the findings by Grewe *et al.* (2006) are not an exception. Rather, they form part of a highly systematic overall data pattern, in which the violation of any of the linearization principles in (3) engenders increased activation in the left POp. These findings suggest that the POp engages in aspects of sentence-level sequencing which go beyond those aspects of linear order that can be captured via movement (IM). Thus, by assuming an IM-based specificity of this region, we would miss an important generalization, namely that the POp appears to play a relatively general sequence processing role in language. We shall return to this issue in more detail in [Section 14.4](#) below.

In summary, while a functional-neuroanatomical dissociation based on the distinction between IM and EM may appear appealing at first blush, the full range of neuroimaging findings shows that this proposal is empirically problematic. Rather, it seems to us that the assumption of a single syntactic operation which serves to combine elements to form larger constituents (Merge or some similar operation) is a more promising candidate for a neuroanatomically supported primitive of syntactic computation. The question of how the anatomical differences observed by Friederici and colleagues might be alternatively derived will be discussed in [Section 14.4.2](#).

14.2.3 Unification: establishing syntactic representations via structure sharing and feature matching

Beyond the combination of elements, syntax also involves the matching of features between the elements that are combined. This is apparent in phenomena such as agreement (e.g. between subject and verb or between determiner, adjective, and noun) or a head's designation of the properties of its complement. Indeed, some syntactic operations which serve to combine elements, such as unification, also incorporate aspects of feature matching: unification essentially involves structure sharing between constituents (Pollard and Sag 1994) and this presupposes that the properties of the shared structure match (or, if the shared structure is more highly specified in one of the constituents, that the properties do not conflict). In contrast to Merge and Move (or EM and IM), unification is a declarative rather than a derivational operation, hence the concept of “structure sharing.”

Unification can be used to build up structured syntactic representations (e.g. in HPSG, Pollard and Sag 1994; or in the unification-based parsing model by Vosse and Kempen 2000); it can also serve to establish correspondences between representations from different linguistic subdomains (as assumed, for example, in Lexical Functional Grammar, Bresnan 2001; or in Jackendoff's interface model, Jackendoff 2002; for a

neurolinguistic perspective, see Hagoort 2003). As part of his *Memory, Unification and Control* (MUC) framework, Hagoort (2003, 2005) proposes that the IIFG provides the neural “unification space” for language, i.e. subserves the unification of lexicalist syntactic frames (i.e. precompiled partial tree structures that are attached to individual lexical items rather than established via the application of abstract syntactic rules). As evidence for the link between unification and the IIFG, Hagoort and colleagues (Snijders *et al.* 2009) present findings which show increased posterior IIFG activation for syntactically ambiguous vs. unambiguous sentences (i.e. for sentences with a larger range of unification options; for other reports of a correlation between syntactic ambiguity and the IIFG, see Fiebach, Vos, and Friederici 2004; Stowe *et al.* 2004; Bornkessel *et al.* 2005; Novick, Trueswell, and Thompson-Schill 2005). For ambiguous words (involving a category ambiguity between noun and verb), by contrast, the authors reported an activation increase in posterior temporal cortex, which they interpret as supporting the retrieval of syntactic frames from memory.

Overall, the unification-based perspective, in which syntactic structure-building proceeds via only a single computational operation, provides a clear parallel to the most recent Minimalist proposals (see above), in which Merge is viewed as the core syntactic operation, though the two differ with regard to some of the specific computational details. However, the concept of unification is clearly broader than syntax, since it provides a basis for the association between representations from different linguistic subdomains. In this way, Hagoort’s account views the IIFG as a region that supports general combinatorial processes in language comprehension and production. In addition, however, he proposes that there may be a functional “gradient” across the IIFG which differentiates between the linguistic domains to which unification applies: while the most posterior portion of the IIFG (POp) and adjoining ventral premotor cortex (ventral PMC) are thought to support phonological unification, the central part of the IIFG (PTr and POp) is the locus of syntactic unification, and the most anterior part of the IIFG (POr and PTr) underlies semantic unification (Hagoort 2005).

In summary, the unification-based perspective provides an alternative view on the computational primitives underlying the neurobiology of syntax (and language as a whole) to the Merge-based perspective. In advocating this view, Hagoort and his colleagues have committed very strongly to the position that the IIFG serves as a unification space for language. Thus, while serving a clearly language-related function during language processing, this cortical region is thought to play a role that goes beyond syntax per se, but rather encompasses all linguistic domains. As will be discussed in more detail in Section 14.4, however, this perspective is somewhat at odds with findings which suggest that the functional role of the IIFG in language is broader still, i.e. that it may reflect domain-general

processes of cognitive control rather than mechanisms of linguistic structure-building. For a further and more detailed discussion of the neural correlates of morphosyntactic features such as case marking and agreement, see Bornkessel-Schlesewskey and Schlesewskey (this volume).

14.3 Establishing long-distance dependencies

The phenomenon which Chomsky (2005: 12) described as the “familiar displacement property of language” in the quote cited in [Subsection 14.2.2](#) above has attracted a great deal of attention in the psycholinguistic and neurolinguistic literature – indeed, much more so than the basic combinatorial properties described in the preceding section. Thus, a vast range of studies has sought to determine how the brain deals with long-distance dependencies, i.e. relations between non-adjacent elements. The fascination with such dependencies and our neural language system’s ability to interpret language in spite of them has been fueled further by the fact that, at least under certain circumstances, they are potentially unbounded (see example 5 for a familiar illustration).

- (5) a. Peter likes Mary
- b. Whom does Peter like __?
- c. Whom did Sam say that Peter likes __?

While the sentences (5b/c) involve the same relation between *like* and *Mary* as (5a), the two elements that form part of this relation are no longer adjacent to one another. Moreover, though the argument *whom* is still the grammatical object and the Theme of the verb *like*, it does not occur in the typical syntactic position for direct objects in English, namely right-adjacent to the verb. Rather, it occupies the sentence-initial position, which could also host other types of arguments (e.g. subjects, indirect objects). In short, linear order does not transparently reflect the underlying hierarchical structure in sentences of this type. How, then, does the processing system manage to compute this underlying structure, especially since it cannot “know” in advance at what point in the surface string to expect the argument’s underlying position?⁸

From the perspective of functional neuroanatomy, empirical investigations into the processing of long-distance dependencies have, perhaps unsurprisingly, focused largely on the role of Broca’s region (i.e. the POp and PTr of the IIFG). For example, recall from [Section 14.2.2](#) that Santi and Grodzinsky (2007a, 2007b) reported a correlation between long-distance dependencies and activation within the IIFG (though, somewhat troublingly, the specific localization of this activation within the IIFG was not consistent across studies; see above). Interestingly, their experiments suggest that the IIFG is sensitive to both the presence of an (additional) dependency of this type and to the distance spanned by that dependency.

Indeed, the positive correlation between dependency distance and IIFG activation is one of the best investigated phenomena in the cognitive neuroscience of language. It has been demonstrated, for example, in contrasts between object and subject relative clauses (e.g. *The reporter that the senator attacked admitted the error* vs. *The reporter that attacked the senator admitted the error*, from King and Just 1991), since object relatives involve a longer dependency between the relative pronoun and the base position of the argument to which it refers (for English, e.g. Stromswold et al. 1996; Caplan, Alpert, and Waters 1999; Constable et al. 2004; for German, Fiebach et al. 2004).⁹ Similar results have been obtained in comparisons between sentences with object relative clauses and other control conditions (e.g., in English, conjoined active sentences such as *The reporter attacked the senator and admitted the error*, Just et al. 1996; Keller, Carpenter, and Just 2001; or, in Hebrew, sentences involving a complement clause rather than a relative clause, Ben-Shachar et al. 2003).

In a second class of studies, the processing of long-distance dependencies was examined by means of wh-questions; as in the studies on relative clause processing, these experiments typically manipulated the length of the dependency by comparing subject and object questions and by introducing intervening material (for English: Cooke et al. 2001; for German: Fiebach et al. 2005). Crucially, both of these studies found that left inferior frontal activation was tied to the combination of an object question and a long filler-gap distance (introduced via intervening material). This central role of distance has led a number of researchers to conclude that, rather than playing a central syntactic role, the IIFG supports the working memory mechanisms that are required for the processing of long-distance dependencies (e.g. Caplan et al. 2000; Kaan and Swaab 2002; Fiebach et al. 2004; Müller and Basho 2004; Fiebach et al. 2005). (For an alternative account of the findings by Fiebach et al. 2005, see Bornkessel et al. 2005.)

A structural explanation for the role of the IIFG in the processing of long-distance dependencies also appears unlikely in view of the finding that the increased activation for object vs. subject relative clauses in this region is tied to certain conditions on the animacy of the sentential arguments. More specifically, Chen et al. (2006) found that, when the head noun of the relative clause is inanimate and the relative clause subject is animate (e.g. *The wood that the man chopped heated the cabin*) there is no longer a measurable activation difference between the two relative clause types in the IIFG. This finding can be explained neither in terms of syntactic properties nor with reference to working memory demands. A further problem lies in the fact that the locus of the activation within the IIFG is not consistent across studies (ranging from anterior and inferior portions of the IIFG in Cooke and colleagues' study to posterior portions of the IIFG in Fiebach and colleagues' results). In addition, while manipulations of this type sometimes also engender activation in superior temporal regions (Just et al.

1996; Keller *et al.* 2001; Ben-Shachar *et al.* 2003; Ben-Shachar *et al.* 2004; Constable *et al.* 2004; Fiebach *et al.* 2005), these temporal activations were not observable in several other experiments (Stromswold *et al.* 1996; Caplan *et al.* 1999; Caplan *et al.* 2000; Fiebach *et al.* 2004).

A second type of problem with regard to the neural correlates of non-local dependencies concerns the correspondence between theoretical assumptions and empirical findings. Recall from [Section 14.2.2](#) above that Grodzinsky and colleagues argue for a movement-based specialization of (subregions) of Broca's area (Grodzinsky 2000; Ben-Shachar *et al.* 2003; Ben-Shachar *et al.* 2004; Grodzinsky and Friederici 2006; Santi and Grodzinsky 2007a; Santi and Grodzinsky 2007b; Grodzinsky and Santi 2008). In fact, in several publications, Grodzinsky and colleagues have rendered their proposal even more specific by restricting it to A-bar movement. Empirically, this proposal was motivated by Ben-Shachar *et al.*'s (2004) findings on Hebrew, which showed increased IIFG activation for object topicalization (i.e. the contrast between object-subject-verb and subject-verb-object orders) but not for dative shift (i.e. the contrast between subject-verb-indirect object-direct object and subject-verb-direct object-indirect object orders). Ben-Shachar and colleagues argue that, since dative shift involves A-movement (if it is modelled in terms of movement at all), the correct neurolinguistic generalization is that the IIFG supports the processing of A-bar movement. Conceptually, this makes a lot of sense given the data discussed above: only A-bar movement yields unbounded dependencies, while A-movement does not. There is, however, an empirical problem. Recall from [Section 14.2.2](#) that word order permutations in the German middlefield ("scrambling") reliably engender activation in the POp of the IIFG. For reasons of parsimony, it obviously appears appealing to derive these results by the same mechanisms used to explain the IIFG activation for word order variations in Hebrew. Yet, the theoretical syntactic literature is by no means in agreement as to whether German scrambling should be treated as A-bar movement (e.g. Grewendorf and Sabel 1999), A-movement (e.g. Fanselow 1990; Haider and Rosengren 2003), as displaying properties of both A and A-bar movement (Webelhuth 1989) or as base generated (e.g. Fanselow 2001). Moreover, even if one follows Grewendorf and Sabel in assuming that German scrambling should be analyzed in terms of A-bar movement, another problem arises: these authors assume that clause-bound scrambling in Japanese differs from scrambling in German in that it is best analyzed as A-movement. Yet, like scrambling in German and topicalization in Hebrew, scrambling in Japanese has also been shown to lead to IIFG activation (Kinno, Kawamura, Shioda, and Sakai 2008). Thus, restrictions to particular types of movement do not appear to provide a coherent explanation for the full range of word order findings in the neuroimaging literature. This further corroborates the conclusion drawn in [Section 14.2.2](#) that the assumption of a specialization of (subregions of) the IIFG for movement-based

dependencies (Santi and Grodzinsky 2007a; Santi and Grodzinsky 2007b) is empirically problematic.¹⁰

In summary, as is apparent from the discussion in this section, the neural correlates of long-distance dependency processing are subject to a range of influencing factors, many of which are not syntactic in nature. Furthermore, research focusing specifically on syntactic factors (particularly movement) has revealed inconsistencies in the mapping between theoretical assumptions and empirical results. We thus conclude that, even though long-distance dependencies have played a major role in the literature on the neurocognition of language, it appears unlikely that the findings in question are suited to revealing brain correlates of computational primitives in syntax.

14.4 Sequencing revisited: Consequences for IIFG function

Having discussed the processing of unbounded dependencies, let us now return to the issue of sequencing (i.e. word order) more generally. While word order variations have typically been studied with the aim of examining the neural correlates of movement or unbounded dependencies, the preceding sections have shown that neither of these two notions appears to constitute a suitable computational primitive of syntax that can be consistently tied to a particular neural substrate. Should this, then, be taken to imply that the IIFG activations discussed in the preceding sections are not syntax-related at all? We believe that existing neuroimaging findings on word order permutations in simple declarative sentences (i.e. neither in relative clauses nor in wh-questions) are suited to shedding further light on this question. In the following subsection, we will therefore describe some of these additional findings, before turning to the broader consequences associated with them.

14.4.1 A gradient of sequence processing within IIFG

Firstly, recall from Section 14.2.2 that a range of experiments on word order permutations in the German middlefield has revealed fine-grained word order-related activation changes in the POp (Röder *et al.* 2002; Bornkessel *et al.* 2005; Grewe *et al.* 2005; Friederici *et al.* 2006b; Grewe *et al.* 2006; Grewe *et al.* 2007; Bornkessel-Schlesewsky *et al.* 2009). In this section, we will now describe a second observation, namely that different sequencing demands appear to engender activation in different subparts of the IIFG. Furthermore, we will discuss how a sequencing-based perspective on IIFG function in language comprehension can derive these observations.

While the results discussed in Section 14.2.2 showed a very robust correlation between the processing of word order variations and the

POp, there are some indications that word order-related activation changes in simple sentences may also show activation maxima in the PTr (Bahlmann *et al.* 2007 for German, Ben-Shachar *et al.* 2004 for Hebrew, Kinno *et al.* 2008 for Japanese). Notably, all of these studies (see 6–8 for examples) differed from those described in Section 14.2.2 in that they involved object fronting to the clause-initial position (all three studies contrasted object-before-subject with subject-before-object orders) rather than argument order permutations within a clause-medial region (the German “middlefield”). This observation led Bornkessel-Schlesewsky and Schlesewsky (2009a) to speculate that the different activation maxima may “be somehow related to the positioning of the object within the overall clause” (Bornkessel-Schlesewsky and Schlesewsky 2009a: 152), i.e. to the difference between word order permutations targeting a clause-medial vs. the clause-initial position.

(6) Example stimuli from Bahlmann *et al.* (2007)

a. Object-verb-subject

Den begabten Sänger entdeckte während der Weihnachtsfeier
[the gifted singer]Acc discovered during the christmas.party
der talentierte Gitarrist.

[the talented guitar.player]NOM

“The talented guitar player discovered the gifted singer during the Christmas party.”

b. Subject-verb-object (control)

Der begabte Sänger entdeckte während der Weihnachtsfeier
[the gifted singer]NOM discovered during the christmas.party
den talentierten Gitarristen.

[the talented guitar.player]ACC

“The gifted singer discovered the talented guitar player during the Christmas party.”

(7) Example stimuli from Ben-Shachar *et al.* (2004)

a. Object-subject-verb

‘et ha-sefer ha’adom John natan la-professor me-oxford
[ACC the-book the - red] John gave [to-the-professor from-Oxford]
“John gave the red book to the professor from Oxford.”

b. Subject-verb-object (control)

John natan ‘et ha-sefer ha’adom la-professor me-oxford
John gave [ACC the-book the -red] [to-the-professor from-Oxford]
“John gave the red book to the professor from Oxford.”

(8) Example stimuli from Kinno *et al.* (2008). Note that this study employed a sentence–picture matching task; the geometrical shapes in the sentences thus refer to stick figures with heads depicted as the respective shapes.

- a. Object-subject-verb
 - o □-ga oshiteru
 - ACC □-NOM pushes
 - “□ pushes □”
- b. Subject-object-verb (control)
 - ga □-o oshiteru
 - NOM □-ACC pushes
 - “□ pushes □”

In order to examine this apparent distinction between the clause-initial position and the clause-medial region more closely, Bornkessel-Schlesewsky *et al.* (2012) directly contrasted clause-initial and clause-medial word order permutations in German in a within-participants design. The critical sentence conditions used in this study are illustrated in example (9).

- (9) Example stimuli from Bornkessel-Schlesewsky *et al.* (2012). Only the object-initial conditions are shown; in the subject-initial controls, the position of subject and object was interchanged.
- a. Object-before-subject order - clause-medial

Peter behauptet, dass den Arzt der Lehrer verfolgt hat.
 Peter claims that [the doctor]ACC [the teacher]NOM pursued has
 “Peter claims that the doctor pursued the teacher.”
 - b. Object-before-subject order - clause-initial

Peter behauptet, den Arzt hat der Lehrer verfolgt.
 Peter claims [the doctor]ACC has [the teacher]NOM pursued
 “Peter claims the doctor pursued the teacher.”

Bornkessel-Schlesewsky *et al.* (2012) indeed observed activation differences within different subregions of the 1IFG for the different sentence types employed: whereas posterior portions of the 1IFG (primarily the POp) showed a main effect of argument order (higher activation for object-initial vs. subject-initial word orders), more anterior regions of the 1IFG (including the PTr) showed effects of both argument order and sentence type (higher activation for object-initial vs. subject-initial word orders and for sentences with an argument in the clause-initial position of the embedded clause vs. sentences in which the embedded clause was introduced by a complementizer). Thus, rather than showing a selective response to object-initial orders targeting the clause-initial region, anterior portions of the 1IFG generally responded to the positioning of an argument in the clause-initial position (in addition to the relative positioning of subject and object). Bornkessel-Schlesewsky *et al.* (2012) interpreted this finding with reference to the special information structural status of the clause-initial position: by default, an argument residing in this position is interpreted as the sentence topic, i.e. the entity which the sentence *is about* (see, for example, Frey 2007 and the references cited therein). In the presence of a discourse context, this sentence topic might also be expected to

correspond to the discourse topic, i.e. to the entity under discussion in the discourse. It is also typically the subject of the sentence rather than the object (Keenan 1976). Bornkessel-Schlesewsky and colleagues thus argued for a sequencing gradient within the IIFG, with more anterior regions engaging in “aboutness-based sequencing” (i.e. determining which argument is to reside in the position which correlates with aboutness-based topicality) and more posterior regions engaging in “prominence-based sequencing.” Prominence-based sequencing is based on the inherent properties of the arguments and the prominence scales in (3), which, in addition to influencing argument order in a range of different languages (Tomlin 1986), are known as determinants of morphosyntactic properties in many languages of the world (see, for example, Comrie 1989; Croft 2003).¹¹

Thus, while sequencing options based on prominence features depend on the properties of the arguments themselves and the relation between them; sequencing options based on aboutness depend on what the sentence is construed to be about and are thus not closely linked to individual argument properties. The findings by Bornkessel-Schlesewsky *et al.* (2012) thus suggest that different types of sequencing may be dissociable from the perspective of functional neuroanatomy.

14.4.2 Broader implications: Sequencing and a hierarchy of cognitive control signals

One possible interpretation of the neuroanatomical dissociations between different types of linguistic sequencing is that they reflect distinct linguistic operations. However, Bornkessel-Schlesewsky *et al.* (2012) suggest that there is a more parsimonious – and hence preferable – interpretation of these findings via mechanisms of cognitive control (i.e. domain-general cognitive operations that serve to choose between competing stimuli or task requirements). Specifically, Koechlin and colleagues describe the hierarchy of cognitive control processes and their implementation in frontal cortex summarized in (10) (Koechlin, Ody, and Kouneiher 2003; Koechlin and Summerfield 2007; Kouneiher, Charron, and Koechlin 2009). For an illustration of the assumed functional-neuroanatomical gradient and its potential application to language processing, see Figure 14.2.

- (10) Hierarchy of cognitive control cues in frontal cortex according to Koechlin and colleagues (the interpretation of the different control cues in terms of levels of locality is our own)
 - a. **Stimulus-driven control** draws upon the most basic type of control cue and is thought to be implemented by premotor cortex (i.e. the most posterior portion of the prefrontal control gradient). It consists of simple stimulus-response mappings, such as the tendency to answer the telephone when it rings. This type of

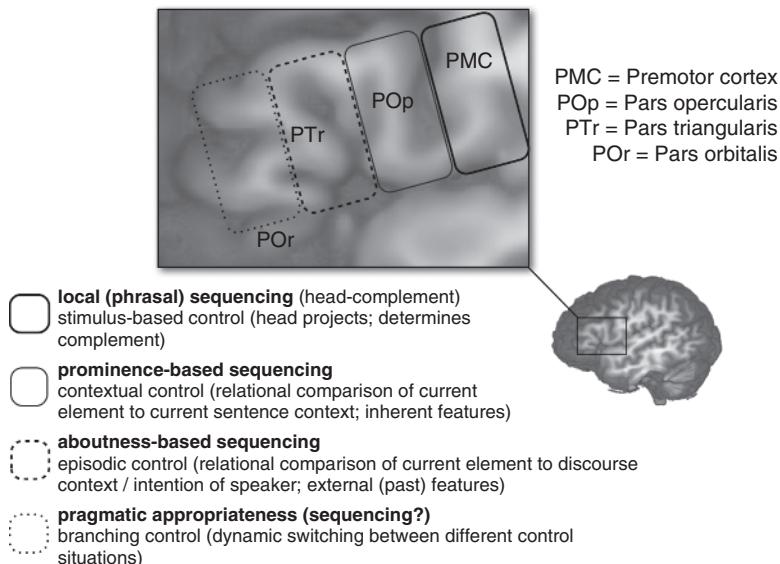


Figure 14.2 Schematic illustration of how the hierarchy of cognitive control processes assumed by Koechlin and colleagues (Koechlin *et al.* 2003; Koechlin and Summerfield 2007) can be applied to different aspects of linguistic sequencing.

control cue is thus closely associated with the critical stimulus itself and is therefore relatively “local.”

- b. **Contextual control** hierarchically dominates stimulus-driven control and implies that stimulus-response mappings can be determined or overridden by the current episode within which an individual finds him/herself. For example, when visiting a friend’s house, we may be less likely to pick up the phone when it rings as opposed to when we are at home. Neuroanatomically, contextual control correlates with more anterior regions of frontal cortex than stimulus-driven control (e.g. with posterior portions of the IIFG such as the POp). This type of control cue is less local than stimulus-driven control, since it requires assessment of the stimulus in relation to the current context in which it occurs.
- c. **Episodic control** involves the possibility of overriding contextual control with reference to past events. For example, if the friend who I am currently visiting asks me to answer the phone because he/she is working but expecting an important call, I will do so in spite of the contextual control cue “at a friend’s house.” This type of control, which is associated with even more anterior portions of frontal cortex (typically BA 46, Koechlin *et al.* 2003; Koechlin and Summerfield 2007), is again less local than contextual control because it involves relating the current stimulus (and context, possibly) to a distal event that took place at some point in the past.

- d. **Branching control** is the most abstract type of control within the hierarchy and correlates with the most anterior portions of pre-frontal cortex (frontopolar cortex corresponding to lateral BA 10). It permits switches between different types of control scenarios. For example, assuming (as in c above) that my friend asked me to answer his/her phone while staying at his/her house, he/she could temporarily undo this request by stating that he/she would like to take the next call him/herself because it is going to be particularly important. I would then refrain from answering the phone the next time it rings (effectively disenabling the currently active episodic control cue), but would return to “answering mode” with the next phone call (thereby reenabling the episodic control cue).

Bornkessel-Schlesewsky *et al.* (2012) argue that the linguistic sequencing gradient along the anterior-posterior dimension of the lIFG precisely correlates with this hierarchy of cognitive control. Prominence-based sequencing can be described as requiring contextual control, because the prominence properties of the current stimulus element must be assessed in relation to the (sentence-internal) context in which they occur, i.e. with respect to the prominence status of the coarguments. Aboutness-based sequencing, by contrast, is more closely associated with episodic control, because it requires a link to a distal (sentence-external) event: the broader discourse and the referents under discussion in it or the speaker’s intention with regard to what the sentence is about. Accordingly, aboutness-based sequencing correlates with activation in more anterior portions of the lIFG than prominence-based sequencing. This proposal thus shows how the notion that the lIFG engages in linguistic sequencing can be extended to encompass different types of sequencing cues and how it can be linked to broader cognitive processing mechanisms. In this regard, it shows clear parallels to Thompson-Schill and colleagues’ cognitive control approach to lIFG function in language (e.g. Thompson-Schill *et al.* 1997; Novick *et al.* 2005; January, Trueswell, and Thompson-Schill 2009), but makes the additional claim that the abstractness (or locality) of the currently relevant control cue may play a crucial role in determining localization within frontal cortex.

Crucially for present purposes, the proposal advanced in Bornkessel-Schlesewsky *et al.* (2012) can be extended even further. Recall from [Section 14.2](#) that the processes of local phrase structure building which comprise “local syntax” in Friederici’s account can be described (in processing terms) as the analysis of a sequence A B into an overarching constituent C, with the characteristics of C determined either by A or B depending on their lexical properties (in terms of Merge, either A or B “projects”). This type of lexically determined projection appears compatible with the notion of stimulus-driven control: whether an element projects or not is essentially a stimulus-inherent property and can thus be

described in terms of an (abstract) stimulus-response mapping (the response being a linguistic analysis rather than a behavioral reaction in this case). This assumption fits perfectly with Friederici and colleagues' empirical observations, which suggest that local structure building of this type correlates with activation in the DFO, a region that is cytoarchitecturally comparable to premotor cortex rather than to the lateral surface of the IFG (Friederici 2004; Friederici *et al.* 2006a; Friederici 2009). From this perspective, the local aspect of Merge (i.e. the single combination of two elements to form a larger constituent) amounts to the use of linguistic sequence information under the most local conditions of cognitive control (stimulus-driven control) and thereby correlates with activation in the most posterior portions of prefrontal cortex.¹² By contrast, word order variations in simple sentences (prominence-based sequencing) call for a relational comparison between the properties of the current stimulus and the (sentence-internal) context in which it is encountered and thereby require a less local form of control, namely contextual control. They thus correlate with activation in a slightly more anterior region of frontal cortex, typically the POp.

How does embedding, which, according to Friederici and colleagues' studies (e.g. Friederici *et al.* 2006a; Bahlmann *et al.* 2008; Makuuchi *et al.* 2009) shows similar neuroanatomical correlates to word order variations, fit into this picture? Clearly, embedding requires a less local form of control than phrase-internal structure building since it is not simply dependent on the inherent properties of the current stimulus. Rather, it resembles prominence-based sequencing in that it requires a comparison between the properties of the embedded clause and that of the main clause. This relational comparison is required, on the one hand, because only certain types of phrases can be embedded recursively (sentences and noun phrases, but not, for example, prepositional phrases). On the other hand, embedding at the sentence level also involves sequencing, since many languages allow embedded clauses (complement clauses, relative clauses) to be "extraposed" to a sentence-final position, i.e. to a position following the main clause. Whether an embedded clause is extraposed or not depends, for example, on its "heaviness," namely on the relative length of the embedded clause in comparison to the main clause (e.g. Hawkins 1994). Taken together, embedding shows very similar properties to prominence-based sequencing in that both require relational comparisons between the properties of multiple elements *within* a sentence. As discussed above, this process can be described in terms of contextual control.

An explanation along these lines thus derives the activation differences between sentences with and without embedding (Makuuchi *et al.* 2009) without requiring any reference to the notion of recursion. It also explains why embedding and (prominence-based) word order variations in simple sentences cluster together in neuroanatomical terms – as noted above,

Friederici's explanation in terms of hierarchical syntax is problematic in this regard, since "local structure building" also establishes a hierarchical syntactic structure via the iterative application of Merge. Moreover, our account has advantages in terms of parsimony, since it draws upon mechanisms which also apply to non-linguistic information processing and shows how these can be applied to the processing of and extraction of information from linguistic sequences.

14.4.3 The IIFG as an evaluative component

In essence, the sequencing-related information types that were discussed in the previous sections could be viewed as *constraints on the combination of elements*. For example, once again using Merge as an illustration for concreteness' sake, all of the information types under discussion can be described as constraining which elements are merged with one another. Depending on the nature of the information source (e.g. element-inherent information such as the fact that two heads should not be merged with one another or prominence-/aboutness-based information on the order of constituents within a clause etc.), these constraints appear to correlate with differing degrees of cognitive control. However, does this imply that sequence-related IIFG activation should also be viewed as a correlate of an operation such as Merge, i.e. a primitive of syntactic computation that serves to combine elements to form larger constituents? Here, we suggest an alternative interpretation, namely that the frontal activations associated with sequence processing correlate with the evaluation of linguistic sequences (i.e. essentially of the output of Merge or some equivalent operation) rather than with incremental syntactic structuring per se.¹³

The proposal that IIFG activation may not directly reflect syntactic (or other linguistic) computations has been put forward by a number of scholars. As already noted in section 14.2.1, the conspicuous absence of increased IIFG activation for sentences as opposed to word lists has cast some doubt on whether the IIFG is indeed central to syntactic processing (e.g. Stowe, Haverkort, and Zwarts 2005). Indeed, several studies even reported increased IIFG activation for word lists in contrast to sentences (Mazoyer *et al.* 1993; Stowe *et al.* 1998) or for sentences involving pseudowords in comparison to normal sentences (Friederici *et al.* 2000). This appears to suggest that, rather than supporting "normal sentence processing," the IIFG only steps in when processing demands increase in some way.¹⁴ A somewhat similar line of argumentation has been advanced by proponents of a cognitive control-based account of IIFG function in language (e.g. Thompson-Schill *et al.* 1997; Novick *et al.* 2005; Thompson-Schill, Bedny, and Goldberg 2005; January *et al.* 2009). These studies demonstrated that IIFG activation is modulated by demands on the "selection" of representations from a set of competing alternatives as is required, for

example, in ambiguous sentences or in the naming of objects with a number of possible labels. Findings such as these suggest that the activation of this cortical region is not crucially tied to specific linguistic structuring operations. Finally, on the basis of results showing that IIFG activation can also be observed in tasks related to action observation (Buccino *et al.* 2001) or motor imagery (Binkofski *et al.* 2000), it has been suggested that the IIFG provides a supramodal basis for the hierarchical combination of elements (e.g. in language or music) that was co-opted onto or extended from the combination of motor commands (Fadiga, Craighero, and D'Ausilio 2009).

14.5 Syntactic primitives and the brain? Consequences and perspectives

The neuroimaging evidence reviewed in the preceding sections suggests that, at present, the only syntactic operation which appears to have possible neurobiological correlates is Merge or something akin to it. However, what does it actually mean to say that Merge, as an abstract linguistic operation, is implemented in the brain? As already noted in the introduction, Chomsky envisages Merge as an operation that is time-independent and similar considerations hold with regard to structure-building operations in other theories of grammar. By contrast, all brain correlates of linguistic structuring necessarily reflect how this takes place in time, since communication can never be time-independent. How can this discrepancy be resolved, assuming that Merge is *the* syntactic primitive with the highest degree of neurobiological plausibility?

Essentially, this problem takes us back to the classic distinction between a competence system and its application during performance or, in other words, the relation between the grammar and the parser. Several approaches in the literature have concerned themselves with precisely this issue. Most generally, two perspectives can be distinguished: one which assumes a close link of some kind between the grammar and the parser (Hawkins 1994; Phillips 1996; Hawkins 2004) and one which maintains that the two are independent but that grammars have adapted to be “parser friendly” (Haider 2012). Perhaps the most widely known position is that adopted by Hawkins (1994, 2004), who assumes that grammars are shaped by ease of processing. From this perspective, languages tend to grammaticalize those structures that are less complex and more efficient to process than their competing counterparts according to a variety of metrics (e.g. the number of elements within which constituents can be recognized). However, since Hawkins's metrics apply at the level of constituents and above or refer more generally to dependency relations between particular types of elements, they do not straightforwardly apply to basic combinatorial

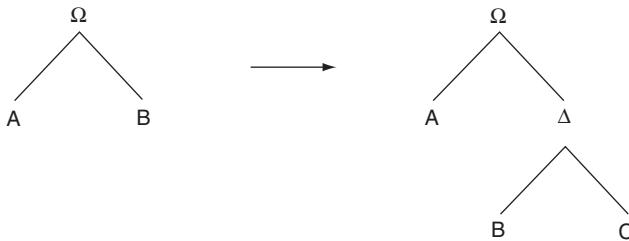


Figure 14.3 Illustration of the incremental application of Merge in the approach by Phillips (1996, 2003).

operations such as Merge. Phillips (1996), by contrast, offers an interesting possible solution to the “time problem” applied to Merge: he proposes that the parser and the grammar are one and the same, the only difference being that the parser utilizes grammatical principles under the restriction of a particular amount of resources. Specifically, he posits that: “Apart from the idealization of unbounded resources, grammaticality and parsability are just the same” (Phillips 1996: 14). In order to make this possible, grammatical structure-building is assumed to apply from left to right (“Merge right”), as is illustrated schematically in Figure 14.3. As is apparent from Figure 14.3, new elements are always merged at the right edge of the structure in Phillips’s approach, thereby leading to a preference for right-branching structures. This implies that a sequence of input elements A–B is initially merged to create an overarching constituent, but that this sisterhood relation is destroyed again when the next input element C is integrated into the structure. Phillips (1996, 2003) shows that, in addition to allowing for the parser to be equated with the grammar, this assumption also accounts for a range of grammatical facts such as apparently contradictory results of different types of constituency tests.

An even stronger grammatical preference for right-branching structures is assumed by Haider (1992, 2000, 2012). He proposes that Universal Grammar (UG) incorporates a restriction termed the *Basic Branching Condition* (BBC): “Each branching node on a projection line is to the *right* of its sister node” (Haider 2012: Chapter 2, italics in original). This restriction is assumed to have been implemented as a product of cognitive evolution, as UG adapted to become parser-friendly. Given the BBC, linear sequences of elements can be unambiguously mapped onto hierarchical structures, which is not possible given the standard (non-directional) version of Merge alone. This approach contrasts with Phillips’s assumptions in that grammar and parser are considered separate and that the branching requirement is a strict condition rather than a mere preference or ambiguity resolution metric. However: “The grammar guarantees easily processable data structures because ease of processing is one of the selection properties in the history of the

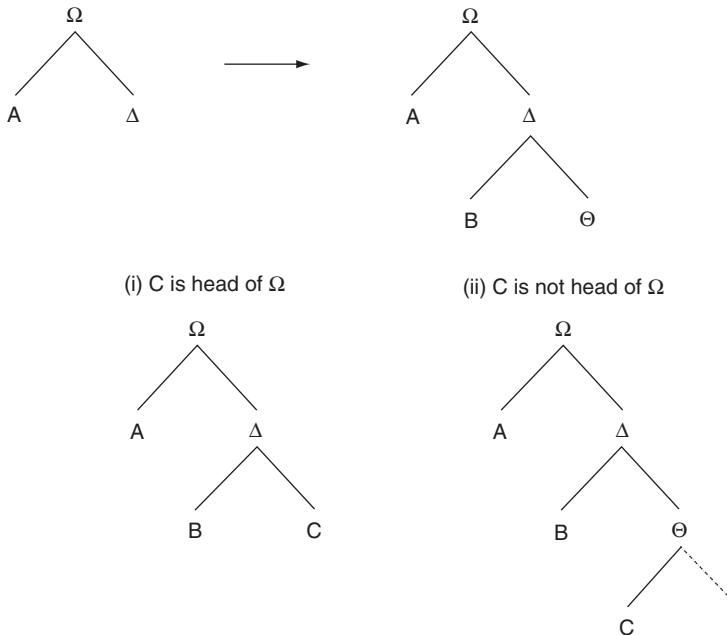


Figure 14.4 Illustration of the incremental application of Merge in the approach by Haider (1992, 2000, 2012).

cognitive evolution of the human language faculty” (Haider 2012). Incremental structure-building in Haider’s approach is illustrated in Figure 14.4.

The central contrast between the structure-building steps in Figure 14.4 (Haider’s approach) as opposed to Figure 14.3 (Phillips’s approach) is that an incoming terminal element need not be merged in the rightmost position. This only happens when the element (C in Figure 14.4) is the head of the currently open projection (scenario i). When C is not the head, an intermediate category (a variable, Θ in the figure) is introduced and merged with the preceding terminal node (B ; scenario ii). Thus, in contrast to Phillips, Haider does not assume that structure-building necessarily involves structure creation and destruction.

In summary, the approaches by Phillips (1996, 2003) and Haider (1992, 2000, 2012) demonstrate that, under certain assumptions, Merge can indeed be compatible with the requirements of online processing. This provides us with the necessary link between theoretical proposals and neurobiological findings that was previously missing. As argued by Haider, one does not even have to give up the distinction between grammar and parser to allow for a (potentially) psychologically and thereby biologically real Merge operation. This strengthens the assumption that Merge is a promising candidate for a primitive of syntactic computation that is implemented in the human brain.

Appendix:	List of abbreviations
ATL	anterior temporal lobe
BA	Brodmann area
EM	external merge
DFO	deep frontal operculum
IFG	inferior frontal gyrus
IM	internal merge
l	left
MFG	middle frontal gyrus
POp	pars opercularis (of the inferior frontal gyrus)
POr	pars orbitalis (of the inferior frontal gyrus)
pSTG	posterior portion of the superior temporal gyrus
pSTS	posterior portion of the superior temporal sulcus
PTr	pars triangularis (of the left inferior frontal gyrus)
r	right
vPMC	ventral premotor cortex
WM	working memory

15

Computational primitives in morphology and possible brain correlates

Ina Bornkessel-Schlesewsky and Matthias Schlesewsky

15.1 Introduction

With regard to questions of computational primitives, morphology is what might be termed a “special case.” One of the main reasons for this is that it is a paradigmatic example of an interface domain in linguistics. As Spencer (1991) puts it in a classic textbook on morphology: “Morphology is unusual amongst the subdisciplines of linguistics, in that much of the interest in the subject derives not so much from the facts of morphology themselves, but from the way that morphology interacts with and relates to other branches of linguistics, such as phonology and syntax” (Spencer 1991: xii). This raises the question of whether morphology in fact has any specific computational primitives of its own or whether it is rather the convergence of several domains that is most crucial for the explanation of morphological structure. Conversely, however, one might argue that there is actually a long list of potential candidates for such computational primitives – or at least for the way in which morphology is structured and represented. Thus, morphological theorizing is characterized by a range of binary contrasts: Whatever the theoretical persuasion of the author, an arbitrarily chosen textbook on morphology is likely to discuss the difference between derivation and inflection, between syntagmatic and paradigmatic properties and between regular and irregular morphological forms. All of these distinctions might thus be considered possible, and plausible, candidates for the way in which morphological knowledge is organized in the human brain. In this chapter, by contrast, we will argue that none of the above-mentioned distinctions appears to offer a satisfactory account of morphological processing from a neurobiological perspective. Rather, we propose that morphological representations and computations require both a broader and a narrower (more fine-grained) perspective than these macroscopic distinctions suggest. In *broader* terms,

morphology is characterized by the fact that it is inherently a *relational* domain, i.e. one in which the establishment of relations between different linguistic units is of primary importance. In this way, morphology contrasts with the primarily combinatorial nature of syntax (see Schlesewsky and Bornkessel-Schlesewsky, this volume) – a position that, as we shall see below, is supported by neurophysiological and neuroanatomical results. In *narrower* terms, neuroscientific studies of morphology have revealed a very rich data pattern that emphasizes the importance of taking fine-grained distinctions into consideration. For example, though both would be considered to lie within the domain of inflection, interpretively “empty” phenomena such as agreement show different brain correlates to interpretively rich ones such as case marking. In this way, a neuroscientific perspective on morphology suggests that the search for neurobiologically plausible computational primitives in this domain should take place at multiple levels or grain-sizes and should look beyond traditional linguistic oppositions. In this sense, the overall data pattern on the brain bases of morphological processing at present appears to favor an interface-based perspective on morphology, in which a range of interacting information types is of primary importance. By contrast, there appears to be little compelling evidence that the brain implements specifically morphological computational primitives.

The remainder of this chapter is organized as follows. In [Section 15.2](#), we discuss whether there are, in fact, computational primitives in morphology that appear plausible from a neurocognitive perspective and conclude that there is presently no unequivocal evidence for this assumption. Subsequently, [Section 15.3](#) reviews research on the two morphological distinctions that have attracted the most attention in the literature on the neural bases of morphological processing: regular versus irregular morphology and inflection versus derivation. Again, we argue that neither of these two oppositions appears suited to explaining how morphology is organized in the brain. Having worked through those factors that, in our view, do not seem to account for the neurobiology of morphological processing, [Section 15.4](#) attempts to offer some more “positive” suggestions regarding the neural representation and processing of morphology. Here, we argue for the primarily relational role of morphology, which serves to distinguish it from the combinatorial nature of syntax, and for the importance of distinguishing between purely formal and interpretively relevant relations in this regard. Finally, [Section 15.5](#) offers some conclusions. Note that, since an introduction to the relevant experimental methods in the neuroscience of language is beyond the scope of this chapter, we assume that readers have a basic knowledge of these methods in what follows. For a recent introduction aimed at a linguistic audience, see Bornkessel-Schlesewsky and Schlesewsky ([2009a, Chapter 2](#)).

15.2 Are there computational primitives in morphology?

This chapter is concerned with computational primitives in morphology and their possible brain correlates. This would seem to presuppose that there indeed *are* computational primitives in this domain and that, consequently, our task is simply to identify them and how they are implemented in the brain. However, as already outlined briefly above, this is by no means an uncontested assumption: In cognitive neuroscience, one of the dominating debates with regard to morphological processing and the role of morphology within the overall language system has focused on whether morphology should be considered a separable domain or whether it is rather emergent from the convergence of form (phonology, orthography) and meaning (semantics) (Seidenberg and Gonnerman 2000). From an empirical point of view, this discussion essentially came forth from the broader debate regarding the modularity of language (e.g. Fodor 1983; Rumelhart and McClelland 1986, amongst many others). However, it is also worth considering from a theoretical perspective in view of the interface character of morphology (see above), which raises the question of whether the search for morphology-specific neural signatures in fact constitutes a realistic goal. This problem is aggravated by the observation that, in contrast to domains such as syntax for which all existing theoretical approaches would at least agree on a basic set of component features (primarily the ability to combine elements into larger structures), no such consensus exists in morphology. Thus, in spite of the undisputed status of morphology in linguistic theory (compounding is clearly a morphological operation in any linguistic theory), the neurobiological status of purported computational primitives in this domain appears well worth examining.

15.2.1 Priming studies

Studies seeking to address the question of whether morphological structure can be isolated in word processing have often used priming paradigms, i.e. a task in which participants are required to perform a lexical decision and critical stimuli are preceded by a prime word that is either unrelated, morphologically related, orthographically related, or semantically related.¹ Results have been mixed. On the one hand, Devlin *et al.* (2004) contrasted orthographic (passive – PASS), semantic (sofa – COUCH), and morphological priming (hunter – HUNT) with a non-related control (award – MUNCH) in English and concluded that morphological priming effects in brain activation could be fully explained by the effects of semantic and orthographic priming: morphological priming effects overlapped with those due to orthography in occipitotemporal (OT) cortex and with those due to semantics in the middle temporal gyrus (MTG). By contrast, Bozic *et al.* (2007) reported priming effects for morphologically related transparent (bravely – brave) and opaque (archer – arch) words in the left

inferior frontal gyrus (IIFG). A similar finding, albeit showing activation modulations in left OT regions, was reported by Gold and Rastle (2007). This neuroanatomical discrepancy, which may have resulted from the use of different priming paradigms (delayed vs. masked), is rather disconcerting from the perspective that studies of this type might be suited to revealing possible computational primitives in morphology. However, results from Hebrew, a language which – in contrast to English – allows for clearly separable manipulations of semantic and morphological relatedness, again implicate both the IIFG and left OT cortex in morphological processing using masked priming (Bick, Frost, and Goelman 2010; Bick, Goelman, and Frost 2011). Here too, however, task-dependence appears to be an important issue: a similar manipulation involving an overt morphological relatedness judgment did not reveal activation in the IIFG (Bick, Goelman, and Frost 2008). Rather, only the left middle frontal gyrus (IMFG) showed activation in both tasks and in all three studies by Bick and colleagues (Bick *et al.* 2008, 2010, 2011), while left OT cortex was implicated in morphological processing only in Bick *et al.* (2008, 2011). From these findings, Bick and colleagues conclude that the IIFG, the IMFG, and left OT cortex are important for the processing of morphological information, though with varying degrees of automaticity. They argue that the IIFG “may be involved in early and automatic morphological processing” (Bick *et al.* 2010: 1965).² The IMFG, by contrast, they regard as being involved in morphological processing regardless of task and argue for a connection with IMFG involvement in other grammatical tasks in European languages, e.g. for the processing of verbs vs. nouns (Shapiro *et al.* 2001; Shapiro, Moo, and Caramazza 2006; Cappelletti *et al.* 2008) and in artificial grammar processing (Forkstam *et al.* 2006). Finally, in connection with the known association between OT cortex and visual word processing (e.g. Cohen *et al.* 2000; Price and Devlin 2003; Dehaene *et al.* 2005), they argue that “this region is involved in the structural level of morphological processing that is mainly morpho-orthographic” (Bick *et al.* 2011: 2288).

However, though the IIFG and IMFG may show repetition suppression for morphological relatedness but not for semantic or orthographic relatedness in Hebrew, it appears difficult to claim that either of these regions is specific to morphological processing. While the IIFG has been implicated in the processing of a wealth of grammatical information, this activation is notoriously non-specific for morphosyntax (see Bornkessel-Schlesewsy and Schlesewsky 2009a, for a recent review). In fact, it has even been argued that this region’s involvement in language processing is extra-linguistic and should rather be explained with reference to more general cognitive functions such as working memory (Caplan *et al.* 2000; Müller, Kleinhans, and Courchesne 2003) or cognitive control (Thompson-Schill *et al.* 1997; Thompson-Schill, Bedny, and Goldberg 2005; January, Trueswell, and Thompson-Schill 2009; Bornkessel-Schlesewsky, Grewe, and Schlesewsky 2012). It is also known to be activated during action

observation and motor imagery (Pulvermüller and Fadiga 2010, for a recent review) and in the processing of music (e.g. Maess *et al.* 2001; Koelsch *et al.* 2002), thus leading some researchers to propose that the IIFG, together with the ventral premotor cortex, is “tuned to detect and represent complex hierarchical dependencies, regardless of modality and use” and “that this capacity evolved from motor and premotor functions associated with action execution and understanding” (Fadiga, Craighero, and D’Ausilio 2009: 448). Thus, in view of its candidate role as a region that is involved in the controlled processing of various types of linguistic and extra-linguistic stimuli, the proposal that the IIFG is crucial for *implicit* morphological processing appears rather unlikely. Likewise, lMFG activation can be observed in a range of linguistic tasks and for a variety of linguistic and extra-linguistic stimuli and tasks: recent reviews on language and the brain note this region’s involvement in semantic processing (Binder *et al.* 2009) and word retrieval (Price, 2010), while its potential role in executive functions was already highlighted in a number of early reviews on memory and the prefrontal cortex (Owen 1997; D’Esposito *et al.* 1998; Smith and Jonides 1999).

These observations raise the question of what the brain activation measured during the most popular task used to assess morphological processing, the priming paradigm, actually reflects. Masked priming in particular is often described as a means of accessing automatic or implicit aspects of word processing. However, what is rarely discussed in this context is that priming paradigms in fact involve an extremely unnatural setting for language processing, namely one in which a single word presentation mode is combined with a meta-linguistic judgment (namely a lexical decision). This appears particularly problematic in view of the interface-property of morphology that was already described above.³ Thus, rather than illuminating how computational primitives of morphological processing are implemented by the brain, it is likely that results of this type are more suited to shedding light on more general cognitive mechanisms related to task performance. While these may, of course, rely in part on morphological information, they should not be confused with aspects of morphological computation proper.

Irrespective of these caveats, however, the set of studies discussed in this section has produced an interesting result that appears somewhat orthogonal to questions of task. While we have argued that the precise functional interpretation of the brain activity observed in priming tasks may not be straightforward, it is nevertheless interesting to note that morphologically induced activity changes in a particular brain region are only sometimes accompanied by semantically and/or orthographically induced activity changes. On the basis of a study with Hebrew-English bilinguals, Bick and colleagues (2011) raise the intriguing hypothesis that this may be subject to cross-linguistic variation: while both Hebrew and English showed morphological priming (repetition suppression) in IIFG and lMFG, this effect was

modulated by semantic relatedness only in English. They thus argue that dedicated (i.e. semantically independent) morphological processes may exist only in some languages (e.g. Hebrew, a language with a rich morphology and in which morphological and semantic relatedness of words is orthogonal) but not in others (e.g. English, in which semantic information is needed in order for morphological relatedness to be assessed). Similar conclusions were drawn earlier on the basis of behavioral (priming) results (for a review, see Plaut and Gonnerman 2000). In view of the more general functional-neuroanatomical considerations advanced above, this suggests that whether morphology may independently serve to modulate meta-linguistic processes such as those required for a lexical-decision task is language-dependent. However, it has been argued that this pattern of results does not necessarily implicate a dedicated and separable morphological processing component even in morphologically rich languages, since connectionist simulations suggest that both types of systems (i.e. morphologically impoverished and morphologically rich languages) can be processed with a (single) computational system that does not include morphology as an independent domain (Plaut and Gonnerman 2000). Nevertheless, as Plaut and Gonnerman themselves point out, their simulations do not provide a realistic account of morphological processing in English-type and Hebrew-type languages and therefore serve more as a “proof of concept” than as a model. It will thus be interesting to see whether future neuroimaging experiments using more naturalistic tasks to examine Hebrew-type languages will reveal neural correlates of morphological processing that have not been observed in languages such as English or whether the present data pattern – the finding of similar neural regions, though with differences regarding the degree of coactivation or interaction between morphology and semantics – will be shown to generalize.

15.2.2 Morphological decomposition

In an alternative approach to the priming paradigm, a range of studies has attempted to find evidence for (automatic) morphological decomposition. The logic behind this type of approach is as follows: if complex words can be shown to be decomposed into morphemes during language processing, this would seem to provide compelling evidence for the existence of morphemes as linguistic elements and for combinatory mechanisms that operate upon them. The earliest studies of this type date back several decades. In Taft and Forster’s (1975) classic study, for example, participants took longer to classify pseudowords which were stems of existing words (e.g. *juvenate*, from *rejuvenate*) or composed using existing stems (e.g. *dejuvenile*) than pseudowords that were not stems (e.g. *pertoire*, from *repertoire*) or incorporated a non-existing stem (e.g. *depertoire*). The authors interpreted this result as evidence for automatic morphological decomposition during word recognition.

In a similar spirit to Taft and Forster's original study, McKinnon, Allen, and Osterhout (2003) conducted an ERP study in which they compared real words with prefixes and bound stems (*sub-mit*, *re-ceive*, *dis-cern*), pseudowords with prefixes and bound stems (*pro-mit*, *ex-ceive*, *in-cern*), morphologically complex real words (*muffler*, *bookmark*) and pseudowords containing no morphemes (*flermuf*). Since phonologically legal pseudowords are known to elicit larger N400 effects than existing words (e.g. Bentin, McCarthy, and Wood 1985; Holcomb and Neville 1990; Bentin *et al.* 1999), they expected to observe an increased N400 effect for pseudowords consisting of real morphemes in comparison to real words only if these are accessed as a single unit, but not if morphemes are represented separately in the mental lexicon. Indeed, only morphologically empty pseudowords showed an increased N400 in comparison to all other stimulus types, which did not differ from one another, a result which seems to speak in favor of morphological decomposition.

However, there also seems to be an alternative explanation. Proponents of the convergence theory approach to morphology have highlighted the issue that what counts as a morpheme in classical approaches (i.e. those assuming that morphemes are separable units, namely the smallest meaning-bearing units in language, which are combined compositionally into larger units) is debatable. Thus, *grocer* has the same apparent morphological structure as *baker*, *runner*, and *writer*, but the relationship between *groce* and *grocer* does not resemble that in *bake* - *baker*, *run* - *runner*, or *write* - *writer* (Seidenberg and Gonnerman 2000). They also point out that different studies have adopted different analyses with regard to the morphological status of words such as *grocer*, which of course poses problems for the comparability and interpretability of results across experiments. In their alternative view, morphological structure is viewed as *quasi-regular* (see Seidenberg and McClelland 1989, for the coining of this term with regard to orthographical structure): this means that, while the correspondence between form and meaning can be described as rule-governed in some cases and appears fully irregular in others, there are also many intermediate cases. Convergence theory assumes that morphological structure emerges from the correspondence between form (phonology/orthography) and meaning (semantics) and that this mapping involves a graded set of regularities. In Seidenberg and Gonnerman's words:

The attempt to impose a discrete notion of morphemic compositionality misses the broader generalization about the quasiregular character of the lexicon. The word *turn* is very related to *turned*, somewhat less related to *return* and unrelated (any longer) to the name *Turner*. It would be desirable to have a knowledge representation that reflects these different degrees of relatedness. (Seidenberg and Gonnerman 2000: 357)

They propose that connectionist networks provide a natural means of encoding this type of knowledge representation: networks learn to map

the domains onto one another and, in doing so, acquire the graded degrees of regularity evident in this mapping. Crucially, in this approach, there are no units corresponding to morphemes. How could this approach explain the findings by McKinnon *et al.* (2003)? In convergence theory, strings with a particular orthography and/or phonology which make similar semantic contributions to a number of existing words will activate those semantic features whenever they occur in the input. This assumption should clearly also apply for the existing morphemes in the morphologically structured pseudowords in McKinnon and colleagues' study (e.g. *pro-mit*, *ex-ceive*, *in-cern*). Thus, these pseudowords should be expected to activate semantic features in a similar way to pseudohomophones such as *sute* (see Harm and Seidenberg 2004, for detailed discussion and connectionist simulations of pseudohomophone processing). Assuming that the ease of activating semantic features may be related to N400 amplitude (see Lau, Phillips, and Poeppel 2008, for an account of the N400 based on lexical activation, though assuming traditional lexical entries), it appears plausible that those pseudowords that McKinnon *et al.* (2003) analyzed as having morphological structure should engender a reduced N400 effect in comparison to their morphologically empty counterparts, since the overlap between phonology/orthography and meaning is larger in these cases. Of course, this assumption would need to be tested with explicit simulations. Nevertheless, the case of the pseudohomophones, which were shown to activate semantic representations in connectionist simulations (Harm and Seidenberg 2004) appears a promising precursor in this regard. Clearly, as for the pseudohomophones, one remaining question is how the network would recognize whether a "morphologically structured" pseudoword is not an existing word. This, however, must also be addressed in a decomposition-based approach, presumably by means of an additional processing mechanism (see, for example, Schreuder and Baayen's (1995) "licensing and composition" stage).

In summary, while existing studies on morphological decomposition strongly suggest that morphological structure is processed at some level, they do not provide compelling evidence for morphemes as independent symbol-like units which are combined via compositional principles. Rather, they leave open the possibility that their findings could also be explained by the convergence of codes.⁴

15.2.3 Frequency effects

A third approach to studying the question of morphological composition capitalizes upon word frequency effects to investigate decomposition vs. full-form storage. It is well known that frequency affects measures of word recognition, with low versus high frequency words engendering longer fixation times during reading (see Rayner 1998, for an overview) and longer reaction times in lexical decision (Whaley 1978) or naming tasks

(Forster and Chambers 1973). Building on these basic observations, many studies have examined frequency effects in morphologically complex words. The basic idea here is that frequency values are associated with lexical entries (e.g. serving to modulate their activation) and that frequency effects for a given unit can be viewed as evidence for the storage of that unit. Alegre and Gordon (1999) argue, for example:

If regularly inflected forms are not represented as whole words, then the base form must be accessed every time an inflected word is processed. Thus, access speed should depend on the cluster frequency of the base form, defined as the aggregated frequencies of the base and all its inflectional variants (e.g., for *walk*, the summed frequencies of *walk*, *walks*, *walked*, and *walking*). On the other hand, if whole-word representations are available for regularly inflected forms, then the frequency of the inflected form itself should determine access time. (Alegre and Gordon 1999: 42)

Numerous studies using this rationale have been conducted in the behavioral domain (see, for example, Alegre and Gordon, 1999; Seidenberg and Gonnerman 2000, and the references cited therein). In this context, interactions between morphological complexity and frequency have been interpreted as evidence for the full-form storage of some morphologically complex words, e.g. in the case of inflectional ambiguity (Baayen, Dijkstra, and Schreuder, 1997) or with high frequency words (Laine *et al.* 1995).

In the neurocognitive domain, Vartiainen *et al.* (2009) used MEG to examine inflected (case-marked) versus monomorphemic (non-inflected) words in Finnish and crossed this manipulation with the factor word frequency (high vs. low). They observed a stronger activation of left superior temporal cortex for morphologically complex vs. monomorphemic words (200–800 ms post word onset). This effect was also stronger for low versus high frequency words irrespective of morphological complexity, but there was no interaction between the two factors. On the basis of these findings, the authors argue in favor of (general processes of) morphological decomposition, proposing that full-form storage may apply only to inflected forms of the very highest frequency. They further suggest that the timing of their complexity effect is indicative not of early morphological decomposition (which should be expected to occur earlier) but rather of the syntactic and semantic ramifications of processing morphologically complex words.

In a recent fMRI study, Vannest *et al.* (2011) examined the interaction between frequency and morphological complexity in English by contrasting monomorphemic and morphologically complex words with high and low base frequency. Within the morphologically complex words, they additionally manipulated whether these words were assumed to be decomposable or not (based on the assumption that decomposition only applies with certain types of affixes, see Section 15.3.2 for further discussion). Effects of morphological complexity (complex > simplex words) were observed in the

IIFG and ISTG independently of frequency, with the activation increase for non-decomposable words over simple words less pronounced than that for decomposable words over simple words. Base frequency effects were observable only in the thalamus and together with an interaction between frequency and word type: base frequency effects occurred for decomposable and simple words but not for non-decomposable words. The authors suggest that this pattern may be related to the degree of phonological and semantic overlap between the base and the full form, which is higher in the simple and decomposable forms than in the non-decomposable forms (which include forms with a phonological change to the base, e.g. *serene – serenity*, and forms which are semantically less transparent, e.g. *hospital – hospitality*). They argue that issues of transparency may also have played a role in the graded activation patterns in the IIFG and ISTG (decomposable words > non-decomposable words > simple words), in that the effect of the base morpheme on the processing of complex words is stronger for decomposable vs. non-decomposable words.

Perhaps most importantly, however, Vannest *et al.*'s (2011) data show that the relationship between (unidimensional) behavioral findings and multidimensional neuroscientific results is not trivial. While their behavioral results replicated well-known data patterns in showing a base frequency effect for decomposable but not for non-decomposable words, the fMRI data suggest that this result cannot be straightforwardly taken to suggest composition versus full-form storage, respectively: this assumption is not compatible with the graded response within the IIFG and ISTG. In addition, further regions showed a sensitivity to word frequency, but either did not respond to morphological complexity or only showed a frequency effect for simple words. From these findings, the authors conclude that "subtle differences in words' morphological complexity (semantic and phonological transparency, the relationship between its base and surface frequency) affect the neural mechanisms used to recognize them in a more graded fashion than what behavioral measures have suggested so far" (Vannest *et al.* 2011: 152). The implications of this observation are quite far-reaching: if fine-grained neuroscientific findings call for different conclusions on morphological decomposition than behavioral results, this means that conclusions from behavioral experiments cannot be simply transferred to the neural domain. In other words, taking (apparent) behaviorally established candidates for computational primitives in morphology and conducting imaging experiments to isolate their neural correlates is not straightforwardly possible. Rather, it appears that we need to develop new paradigms to address these types of questions directly within the neurocognitive domain.

To summarize, while frequency effects have been very influential in the behavioral literature on morphological processing, recent neuroimaging findings suggest that the behavioral data patterns may be a surface reflection (essentially the unidimensional endpoint) of a complex set of

multidimensional underlying mechanisms (for similar arguments, see also Bornkessel *et al.* 2004; Bornkessel-Schlesewsky and Schlesewsky 2007). Furthermore, it has been demonstrated that connectionist models can produce frequency effects without incorporating word units (Seidenberg and McClelland 1989), thus suggesting that the assumed storage implication arising from such effects may be too strong (Seidenberg and Gonnerman 2000). As in the preceding subsections, then, frequency effects do not at present appear to provide unequivocal evidence for specific computational primitives in morphology.

15.3 Classic oppositions in morphology and their neurobiological (im)plausibility

As already outlined in the introduction, we will argue in this chapter that the way in which morphological computations are implemented by the brain does not appear to follow classic oppositions assumed in morphological theory (e.g. derivation vs. inflection, regular vs. irregular morphology). In this section, we shall justify this position by discussing evidence on the processing of these different domains. We will begin by considering studies on regular vs. irregular morphology, since these have played a major role in shaping debates regarding the cognitive and neural bases of language. Subsequently, we will move on to possible neurocognitive bases for the distinction between inflectional and derivational morphology.

15.3.1 Processing regular vs. irregular morphology: The past-tense debate and beyond

At least since Berko's classic (1958) study on the acquisition of morphology in English,⁵ many scholars assumed morphological rules for the processing (i.e. acquisition, comprehension, and production) of regular inflections to be psychologically real. Berko herself argued:

If a child knows that the plural of *witch* is *witches*, he may simply have memorized the plural form. If, however, he tells us that the plural of *gutch* is *gutches*, we have evidence that he actually knows, albeit unconsciously, one of those rules which the descriptive linguist, too, would set forth in his grammar. (Berko 1958: 150)

This assumption that some (i.e. regular) morphological forms are rule-derived is one of the basic tenets of an influential class of morphological models, namely the so-called *dual-route* models. In addition to the rule-based processing pathway, these models posit a second processing route, namely one which directly accesses the full form of irregular forms. The assumptions of such an approach for the neurocognition of language are aptly summarized by Pinker (1998):

The vast expressive power of language is made possible by two principles: the arbitrary sound-meaning pairing underlying words, and the discrete combinatorial system underlying grammar. These principles *implicate distinct cognitive mechanisms*: associative memory and symbol-manipulating rules. The distinction may be seen in the difference between regular inflection (e.g. walk-walked), which is productive and open-ended and hence implicates a rule, and irregular inflection (e.g. come-came), which is idiosyncratic and closed and hence implicates individually memorized words. (Pinker 1998: 219, our emphasis)

Thus, as the quote by Pinker makes clear, if dual-route models of morphology are to be considered cognitively and neurobiologically plausible, one should expect to find clear brain correlates of the distinction between regular and irregular morphology and the processes associated with each. This is what one might describe as the *weak dual-route assumption*: regular vs. irregular morphology should show clearly distinguishable (neuro-)cognitive processing correlates. A *stronger dual-route assumption* was formulated by Ullman (2001b, 2004) within the scope of his declarative-procedural (DP) model, which posits that the processing of regular morphology (and rule-based processes in language more generally) is accomplished using procedural memory systems, while irregular morphology forms part of declarative memory. Procedural memory stores habits and skills (e.g. riding a bicycle), is implicit (i.e. not consciously accessible), and thought to be associated with a fronto-parietal network of brain regions (including Broca's region) and the basal ganglia. Declarative memory, by contrast, stores facts and events, is explicit (i.e. consciously accessible), and thought to be implemented primarily by medial temporal brain structures (the hippocampus and surrounding regions). With regard to event-related brain potentials (ERPs), Ullman claimed that the distinction between the two memory systems correlated with left anterior negativities (LANs) and P600 effects for procedural and N400 effects for declarative memory processes, respectively.⁶ According to the DP-model and its strong dual-route assumption, then, the processing of regular morphology should be supported by fronto-parietal regions and the basal ganglia in neuroanatomical terms and correlate with LAN and P600 effects in electrophysiological terms. The processing of irregular morphology, on the other hand, should draw upon medial temporal regions and manifest itself in N400 effects.

In short, neither the stronger nor the weak version of dual-route assumptions has stood up to empirical scrutiny. While early EEG and neuroimaging studies in the 1990s appeared to provide support for dual-route theories, the more studies that have been conducted, the more inconsistent the results have become. It is beyond the scope of this chapter to provide a comprehensive survey of all of the relevant results (for a detailed review and discussion, see Bornkessel-Schlesewsky and Schlesewsky 2009a). Rather, we restrict ourselves to providing a summary of key findings – both in

favor of and in contradiction to the assumptions of dual-route approaches – in [Table 15.1](#).

Further complications arise when studies on morphological subregularities are considered. Clearly, and as already noted above in connection with the convergence theory, the assumption that all irregular forms are completely dissimilar to one another and to regular forms is a gross oversimplification. Rather, even in languages such as English, in which the distinction between regular and irregular forms is relatively clear, irregulars fall into a number of classes (e.g. verbs that do not change to form the past tense such as beat and cut, verbs that change a final d to t to form the past tense such as send and build, verbs that form the past tense by means of a vowel change and the addition of a final t or d such as feel, lose and tell, [Bybee and Slobin 1982](#)). In an ERP study on the processing of subregularities in German plural formation, [Bartke et al. \(2005\)](#) found that subregular forms engendered a reduced N400 in comparison to (correct) “fully” irregular forms. Furthermore, morphological violations showed a smaller N400 effect (for the violation in comparison to a well-formed control) when the incorrect form was created by adding a subregular vs. irregular plural ending. This was accompanied by a higher behavioral acceptance rate of the subregular violations, i.e. participants showed a higher tendency to (erroneously) judge these words as well-formed. While dual-route models can accommodate form-based similarities between irregular forms due to the assumption of an associative memory system rather than a simple list of irregular verbs ([Pinker and Prince 1988; Pinker and Prince 1994](#)), the dissociation between different types of irregular verbs observed by [Bartke et al. \(2005\)](#) would seem to require the addition of a third model component. Indeed, Bartke and colleagues argue for a tripartite architecture, in which rules and lexical entries and associative memory are supplemented by a third, intermediary information type which is rule-like but qualitatively different from default rules. Since they also argue that only three model components are required (for rule-based forms, subregular forms, and fully irregular forms, respectively), they do not share the fully graded view of morphological regularity that is inherent in connectionist approaches. However, since only three “grades” were tested in their study, the empirical results don’t seem to rule out a more general, fully graded approach.

At a first glance, the absence of clear neurophysiological and neuro-anatomical evidence for the dual-route perspective might be viewed as support for the opposite assumption, namely that regular and irregular morphology is formed via a *single route*. The most prominent class of single-route approaches in the neurobiological domain is constituted by connectionist models. In a seminal paper, [Rumelhart and McClelland \(1986\)](#) questioned whether the way in which children acquire the English past tense, including the ability to inflect nonce words described above, indeed implicates two distinct cognitive subsystems as noted in the quote by Pinker

Table 15.1 Summary of the key neurophysiological and neuroanatomical evidence for and against dual-route assumptions (for more detailed discussion, see Bornkessel-Schlesewsy and Schlesewsky, 2009a).

Evidence for dual-route assumptions	Representative studies (language in parentheses)
<u>Patient studies</u> Double dissociation between patients with anterior lesions/Parkinson's disease (deficits in the production of regular forms) and patients with posterior lesions/Alzheimer's disease (deficits in the production of irregular forms)	Ullman <i>et al.</i> (1997) (English)
<u>Electrophysiological findings (violation paradigms)</u> N400 for irregularized regular forms LAN for regularized irregular forms	Penke <i>et al.</i> (1997) (German) Weyerts <i>et al.</i> (1997) (German) Gross <i>et al.</i> (1998) (Italian) Rodriguez-Fornells <i>et al.</i> (2001) (Catalan) Lück <i>et al.</i> (2006) (German)
<u>Electrophysiological findings (priming)</u> N400 reduction for primed regular but not for primed irregular forms (priming from stem or infinitive to inflected form)	Weyerts <i>et al.</i> (1996) (German) Münte <i>et al.</i> (1999b) (English) Rodriguez-Fornells <i>et al.</i> (2002) (Spanish)
<u>Neuroimaging findings</u> Neuroanatomical dissociations between regular and irregular forms	Jaeger <i>et al.</i> (1996) (English) Beretta <i>et al.</i> (2003) (German) Tyler <i>et al.</i> (2005) (English) Desai <i>et al.</i> (2006) (English) de Diego Balaguer <i>et al.</i> (2006) (Spanish)
Evidence against dual-route assumptions	Representative studies (language in parentheses)
<u>Patient studies</u> Apparent impairment of regular morphology is no longer observable (in some patients) when phonological structure is controlled	Bird <i>et al.</i> (2003) (English)
<u>Electrophysiological findings (violation paradigms)</u> N400 effects for violations involving productive derivational affixes N400 modulations for subregularities in inflectional morphology More generally: N400 effects for violations of rule-based morphological information	Janssen <i>et al.</i> (2005) (German) Bartke <i>et al.</i> (2005) (German) Choudhary <i>et al.</i> (2009) (Hindi)
<u>Neuroimaging findings</u> Inconsistencies in the neuroanatomical locus of the differences between regular and irregular forms Dependence of IIFG activation on phonological properties	(references as above) Joanisse and Seidenberg (2005) (English)

above and assumed by dual-route models in general. They showed that a relatively simple pattern associator network could simulate the acquisition phases for irregular and regular past tense forms and thus argued that a distinction between two computational mechanisms for regular vs. irregular morphology – one rule-based and one amounting to retrieval in an associative memory system – was not a conceptual necessity. While several aspects of the original Rumelhart and McClelland model turned out to be problematic, these were addressed in later models (e.g. Plunkett and Marchman 1993). Furthermore, since a key property of connectionist networks is that similar input representations are mapped onto similar output representations, models of this type appear, in principle, better equipped to deal with the problem of subregularities that was briefly discussed above. However, as already argued for the dual-route models, the “real” test for single-route connectionist models is, of course, whether they are neurobiologically plausible or not. Here, the data in [Table 15.1](#) reveal several problems: While they do not provide clear evidence for a dual-route perspective, the fact that neurophysiological and neuroanatomical differences have been repeatedly demonstrated for the processing of regular and irregular forms and that findings of this type cannot be reduced to the higher degree of difficulty associated with irregulars (as argued by Seidenberg and Hoeffner 1998; Seidenberg and Arnoldussen 2003) appears difficult to reconcile with a strict single-route proposal.

More recently, advocates of connectionist approaches to morphological processing have therefore posited that regular and irregular forms may draw more strongly upon phonological and semantic processing systems, respectively (Joanisse and Seidenberg 1999). From this perspective, qualitative differences between the two types of forms are to be expected, but these would be due to the reliance upon independently motivated linguistic subsystems rather than to distinct mechanisms for the processing of regular and irregular forms per se. Converging evidence for this proposal stems both from patient studies and from neuroimaging findings: Bird *et al.* (2003) demonstrated that, in some cases, apparent impairments in the processing of regular morphology disappear when phonological structure is controlled; using fMRI, Joanisse and Seidenberg (2005) showed that irregular forms that were phonologically similar to regulars (“pseudo-regulars,” e.g. *slept*, *fled*, *sold*) showed similar activation in the IIFG to regular forms and differed from “true” irregulars (e.g. *took*, *gave*).⁷ However, further recent studies suggest that the assumption of differential phonological vs. semantic contributions to the processing of regular and irregular forms does not appear to provide a completely satisfying account of the existing data, since imaging experiments that have controlled for phonological properties nevertheless found activation differences between regular and irregular forms (de Diego Balaguer *et al.* 2006; Desai *et al.* 2006).⁸

In summary, neither dual-route nor existing connectionist models can explain the full range of existing neurocognitive results on the processing of

regular and irregular morphological forms. In our view, this strongly suggests that the “regularity debate,” i.e. the discussion of whether the distinction between regular and irregular morphological forms should be viewed as a primary determinant of how morphological processing is implemented in the brain, has not provided us with particularly fruitful answers regarding possible computational primitives in morphology and that fresh avenues of investigation should therefore be pursued (for a similar line of argumentation, see Bornkessel-Schlesewsky and Schlesewsky 2009a; de Diego Balaguer *et al.* 2006; Münte, Rodriguez-Fornells, and Kutas 1999a). This observation is further supported by a growing body of results from corpus, computational, and behavioral studies which suggest that the regularity debate has ignored crucial factors that are often confounded with distinctions in regularity. For example, Baayen and Moscoso del Prado (2005) show for English, Dutch, and German that regular and irregular verbs differ in terms of semantic density (i.e. irregular verbs tend to have more meanings than regular verbs and they occur in semantic neighborhoods with larger numbers of other irregular verbs), inflectional entropy (i.e. irregular verbs tend to “make fuller use” of the inflectional possibilities afforded by the language), argument structure alternations, and auxiliary selection. They demonstrate that these properties correlate strongly with a range of behavioral measures (e.g. response latencies in a lexical decision task) and that they were confounded with the regularity distinction in two neuroimaging studies that reported activation differences between regular and irregular forms (Jaeger *et al.* 1996, for English; Beretta *et al.* 2003, for German). It appears very likely that this confound applies to other existing studies as well.

Further results have demonstrated convincingly that both irregular *and* regular past tense formation are influenced by semantic factors (i.e. real-world meaning, Ramscar 2002) and by phonological properties of the stem (Albright and Hayes 2003). This is extremely problematic for dual-route assumptions, since the application of the (single) past tense rule is thought to be guided only by the category of the base to which it applies (unless it is blocked by the presence of a lexically stored irregular past tense form) and to thus be independent of factors such as frequency and semantics. The semantic similarity findings also cannot be explained by Joanisse and Seidenberg’s (1999) connectionist model, since the semantic representations posited as part of this model do not encode semantic similarity between concepts/lexical entries. (In principle, though, it should of course be possible to encode this type of similarity within a network model.) Two different types of conclusions have been drawn from these types of results. Some authors have argued for probabilistic pattern matching or analogy-based effects (Ramscar 2002; Baayen and del Prado Martín 2005). Others have claimed that the data are best explained via a set of probabilistic rules, which are built up from phonologically based lexical generalizations (Albright and Hayes 2003). Further research will be required in order to

arbitrate between these competing views. However, whichever of these approaches ends up providing a better account of the data (for a recent comparison between Albright and Hayes's approach and connectionist modeling, see Mirković, Seidenberg, and Joanisse 2011), they both currently point to the same take-home message: probabilistic information appears to play a crucial role in morphological processing as do phonological and semantic similarity. These conclusions indicate that the overall picture on morphological processing is much richer and more complex than the prominent dual- vs. single-route debate suggests. They also once again point to the interface character of morphology by serving to highlight the crucial role of other linguistic domains in morphological processing. It thus appears likely that future neurocognitive research taking this rich variety of relevant information sources into account will significantly advance our understanding of possible neurobiologically plausible primitives of morphological computation. Some further (initial) ideas in this regard will be described in Section 15.4.

15.3.2 Inflection vs. derivation

The discussion in the preceding section essentially revolved around questions pertaining to inflectional morphology. (And indeed, this was the domain in which the regularity debate – often also termed the “past-tense debate” – originated.) In this subsection, we will extend the discussion to include derivational morphology, examining the hypothesis that the distinction between inflection and derivation may be relevant to defining computational primitives in morphology and their brain correlates.

The possibility that the brain may dissociate between inflectional and derivational morphology was first raised on the basis of a patient study on an Italian patient who made errors in the production and repetition of inflected words, but only few errors with derived words (Miceli *et al.* 1988). On the basis of this finding, Miceli and Caramazza argued that inflection and derivation constitute functionally separable processes within the lexicon. Several later studies appeared to confirm this dissociation (Laine *et al.* 1995; Druks 2006; Hamilton and Branch Coslett 2007), while other patient findings revealed comparably degraded performance for both inflected and derived words (Castles *et al.* 1996; Luzzatti, Mondini, and Semenza 2001). Among the studies reporting a distinction, Laine and colleagues additionally argued for a distinction between highly frequent and less frequent inflected forms, proposing that the former are stored as full forms while the latter are decomposed (see Section 15.2.3). This assumption was based on the observation that the Finnish agrammatic aphasic who they studied was only impaired in reading medium and low frequency inflected forms, but not high frequency inflected forms (but see Vartiainen *et al.* 2009, who argue that full-form storage only applies to inflected forms of extremely high frequency).

Interestingly, a series of more recent patient studies indicates that deficit patterns which, at a first glance, appear highly specific need not necessarily reflect specialized functions. Marangolo *et al.* (2003) reported observations from two Italian patients with a selective deficit for producing nouns derived from verbs (e.g. *fall-imento*, “failure,” from *fallire*, “to fail”). While these initial patients had right-hemispheric temporal and parietal lesions (only one had a frontal lesion), a later group study (Marangolo and Piras 2008) found that this particular impairment appeared to be associated particularly with the right basal ganglia (BG). Similar conclusions were drawn from an fMRI study with healthy participants, which showed right BG involvement for a derivational vs. inflectional task (Marangolo *et al.* 2006). Marangolo and colleagues argue that their findings can be explained in terms of selection processes. They suggest that, due to the fact that Italian has large morphological families for verbs, deriving nouns from verbs requires selecting one case from a large set of alternatives and that the (domain-general) inhibitory mechanisms required by this selection process are supported by the right BG (Marangolo and Piras 2010). In this view, aspects of cognitive control (selection, inhibition) play a crucial role in the computations underlying derivational morphology and their neural underpinnings.

Neural distinctions between inflectional and derivational morphology have also been proposed on the basis of electrophysiological investigations of languages with an extremely rich morphology. For Finnish, Leinonen *et al.* (2008) contrasted inflectional (number agreement), derivational (word-internal selectional restriction; root and suffix combination), and double violations. They argue that their results show qualitatively different responses to the two types of (single) violations: an anterior negativity (AN) for inflectional violations, an N400 for derivational violations, and a P600 for both violation types, with the P600 effect in the combined violation equaling the linear summation of the two single violations. However, visual inspection of the data suggests that all violation conditions in fact engender an AN and that the choice of time windows may have biased towards the different topographies in the effects for inflection and derivation. Thus, while a distinction between inflectional and derivational morphology could still be upheld to some degree if both showed an AN and derivational morphology additionally showed an N400 (as visual inspection suggests), the assumption of a double dissociation appears somewhat questionable.⁹ Moreover, the association between N400 effects and derivational morphology does not appear to generalize, since (in addition to the N400 effects reported for violations of inflectional morphology in the context of the past-tense debate; see Table 15.1), Morris and Holcomb (2005) observed N400 effects for regular and irregular inflectional violations. Nevertheless, the distinction between those aspects of morphological processing that correlate with N400 effects and those that engender anterior negativities does

appear to be of some importance with regard to the brain bases of morphological computation. We shall discuss this issue in more detail in Section 15.4.2 below.

Adopting a different experimental paradigm, Álvarez *et al.* (2011) contrasted inflection and derivation by means of morphologically related and unrelated primes and targets in Spanish word pairs (primes not masked). They reported a reduced N400 in both cases, but different source localizations for the two effects: regions commonly associated with visual word recognition (right cuneus and lingual gyrus) for inflection and left medial frontal areas and anterior cingulate cortex (ACC) for derivation. However, rather than arguing for different computational operations within the two morphological domains, these authors adopt a similar approach to Marangolo and colleagues (e.g. Marangolo and Piras 2010, see above) in claiming that the distinction can be explained in terms of more basic mechanisms or domain-general functions such as cognitive control and aspects of visual discriminability.

The preceding discussion once again shows how questions regarding morphological computation are potentially overshadowed by task-related demands: derivation vs. inflection (or special cases of one or the other) may impose differential demands on general cognitive mechanisms such as selection or visual discriminability in generation or decision tasks, but this does not necessarily imply differing computational primitives in morphology. From this perspective, another experimental paradigm appears potentially more promising, namely the comparison between morphologically complex versus simple words, preferably using an indirect task. This type of experimental setup was used by Laine *et al.* (1999), who used PET to compare case-marked vs. non-case-marked nouns in a recognition memory task on Finnish. They observed increased activation in left BA 44/45 for case-marked vs. non-case-marked words. Inferior frontal activation (though in BA 47) was also observed for this type of contrast in subsequent fMRI studies using a different task (Lehtonen *et al.* 2006 examined Finnish and used a lexical decision task; Yokoyama *et al.* 2006 contrasted inflected verbs of differing morphological complexity (active vs. passive) in Japanese and used an orthography-related judgment task; Inui, Ogawa, and Ohba 2007 examined Japanese and used a particle judgment task).¹⁰ A similar finding has also been reported in the domain of derivational morphology: Vannest, Polk, and Lewis (2005) used a similar task to Laine *et al.* (1999) and contrasted different types of affixed words, monomorphemic words, and inflected words. In line with the assumption that only certain affixes are automatically decomposed (e.g. -able, -ness, but not -ity or -ation, see Bertram, Schreuder, and Baayen 2000, and Section 15.2.3), they observed increased IIFG activation for “decomposable” (e.g. *adaptable*) vs. “whole word” (e.g. *curiosity*) affixed words and a marginal activation increase for inflected vs. “whole words.” Thus, findings from inflectional and derivational morphology appear to converge and, interestingly, results once

again point to a role of the IIFG in morphological processing. As already pointed out in [Section 15.2](#), however, this region is notoriously multifunctional and thus does not appear to be a good candidate for the anatomical locus (should there be such a thing) of computational primitives in morphology as opposed to more general mechanisms.

In summary, the overall pattern of results on the processing of inflectional and derivational morphology does not present compelling evidence for separable brain-based computational primitives between the two domains. Rather, common themes such as the question of decomposition vs. storage recur for both, as do common neural regions (e.g. the IIFG and OT cortex). Finally, the results in this domain once again serve to emphasize the importance of task demands in the interpretation of empirical findings.

15.4 Morphology as a relational information type

In the previous section, we discussed a number of potential candidates for computational primitives in morphology and showed that, in view of the existing data patterns, they do not appear to withstand scrutiny from the perspective of neural plausibility. In this section, we will now adopt a somewhat more “positive” perspective in that we will attempt to discuss some potential solutions for how neurally plausible computational primitives in morphology might be envisaged. At this point, it is important to note that, by computational primitives, we not only mean those computational mechanisms that are unique to morphology (for which we don’t see much evidence at present), but also those mechanisms that might underlie morphological processing, even if they are more general in nature. As we have already seen in the preceding sections, scholars working on the neural correlates of morphology have put forward a number of proposals for such mechanisms, ranging from principles in other linguistic domains (e.g. phonology and semantics) to domain-general cognitive processes such as selection. Here, we will argue for several further important points that, in our view, are of central importance to understanding morphological processing in the brain. Firstly, we will show that the neural correlates of morphological processing differ consistently from those related to the processing of combinatorial structure in syntax (see Schlesewsky and Bornkessel-Schlesewsky, this volume, for a detailed discussion of combinatorial structure in language and its neural underpinnings). We argue that these findings reflect the distinction between the *relational* nature of morphology and the *combinatory* nature of syntax. Secondly, we will discuss several fine-grained distinctions in the neural correlates of morphological processing that appear to be best explained with reference to functional distinctions, specifically with regard to whether a morphologically established relation is interpretively relevant or not.

15.4.1 Neural correlates of morphological processing differ from those related to combinatorial structure

As described in detail in our accompanying chapter on computational primitives in syntax (Schlesewsky and Bornkessel-Schlesewsky, this volume), the most plausible candidate for a neurobiologically plausible syntactic primitive is the combination of elements to form larger structures (i.e. the operation *Merge* in Chomskyan frameworks or corresponding combinatorial operations/representational principles in other approaches). Though the neuroanatomical details clearly require further exploration, since many existing findings are subject to the same task-related influences that we discussed above in the context of morphological processing, the anterior temporal lobe (ATL) may be involved in these computations. From an electrophysiological perspective, two groups have argued prominently that word category-based processes of syntactic structure-building are reflected in the early left-anterior negativity (ELAN) (Neville *et al.* 1991; Friederici, Pfeifer, and Hahne 1993; Friederici 2002; Pakulak and Neville 2010, among others). (But see Steinhauer and Drury 2012, for a recent critical review of the ELAN literature.) Though neither of these potential neural correlates of basic combinatorial processes is unequivocally accepted (Hagoort 2005; Snijders *et al.* 2009), it is quite clear from the literature that neither has been associated with morphological processing (see the preceding sections). Thus, the neural correlates of morphological processing appear separable from those underlying basic combinatorial processes in syntax. This is, in and of itself, an interesting observation, since, from a theoretical perspective, one could also argue for a primitive, *Merge*-like operation at the morphological level (see, for example, the “nanosyntax” approach, in which syntactic structure is thought to extend to the sublexical level, e.g. Starke 2009). The neurocognitive data, by contrast, support a separability of morphological processing from basic combinatorial processing.

This observation could then be taken to reflect the *relational* role of morphology. Morphology essentially serves to relate elements in a string to one another by means of overt marking and without having to rely on properties of the sequence in which they occur. *Merge*, by contrast, combines two elements to form a larger constituent and thus cannot be independent of sequence-based properties. The brain may thus distinguish two distinct types of computations during the processing of linguistic sequences: the combination of elements (combinatorial structure building) and the establishment of relational (sequence-independent) structure (see also Bornkessel-Schlesewsky and Schlesewsky 2009a). While the former falls into the domain of syntax, the latter is crucial from the perspective of morphology. In the next subsection, we will therefore take a closer look at the properties of relational structure building and some of the more fine-grained distinctions underlying it.

15.4.2 Fine-grained distinctions in the neural correlates of morphological processing and their functional underpinnings

Though we argued in the preceding subsection that morphology, as a linguistic domain that is primarily relational in nature, should be viewed as distinct from combinatorial structure, this should not be taken to imply that there is a single unified brain correlate of “relational structure building.” Rather, the brain response engendered by relational structuring crucially depends on the functional significance of the relation that is established; specifically, it appears to reflect whether a particular relation is interpretively relevant or purely formal in nature.

This point is perhaps most clearly illustrated with regard to findings from the electrophysiological domain. Recall from [Section 15.3.1](#) that studies examining morphological processing have found two qualitatively distinct negative potential shifts: LAN and N400 effects. Within the scope of the declarative-procedural model, these were interpreted as reflecting the application of procedural and declarative memory systems, respectively. However, as already discussed above, this interpretation cannot be upheld empirically, since N400 effects are not restricted to morphological phenomena that would be described as “non-rule-based” in a dual-route system. If, then, both the LAN and the N400 can index rule-based (or regular) morphological phenomena, what might be the basis for the qualitative distinction between the two effects? In this regard, Choudhary *et al.* ([2009](#)) proposed that the relevant factor may be whether the rule in question is interpretively relevant or not. In an ERP study on Hindi, they examined the processing of case violations within Hindi’s split-ergative case system, i.e. they contrasted sentences in which the subject case (ergative or nominative) matched or mismatched the aspect of the clause (perfective vs. imperfective), and observed an N400 for the mismatch vs. match between case and aspect.¹¹ This result, which is in line with a range of further findings on case violations in other languages (German: Frisch and Schlesewsky [2001](#); Frisch and Schlesewsky [2005](#); Japanese: Mueller *et al.* [2005](#); Mueller *et al.* [2007](#)), stands in contrast to the processing of agreement violations, which have been found to engender LAN effects (e.g. Kutas and Hillyard [1983](#); Münte, Matzke, and Johannes [1997](#); Coulson, King, and Kutas [1998](#); Hagoort and Brown [2000](#)).¹² Further converging support for this distinction stems from a direct comparison of case and agreement violations (Roehm *et al.* [2005](#)). Importantly, however, it is not the distinction between case and agreement that is central here: with the exception of the Hindi study by Choudhary and colleagues, all of the studies on case violations described above used “double case violations,” i.e. sentences in which the same case marking (illegitimately) occurs on two arguments. Violations of subcategorized case (i.e. object case required by the verb), by contrast, were again shown to yield LAN effects

(Friederici and Frisch 2000). This overall data pattern, which at a first glance might appear somewhat confusing, becomes clear in light of the distinction between interpretively relevant vs. irrelevant rules (or regularities) that was introduced above: subject–verb agreement is a purely formal relation without clear interpretive ramifications and the same holds for subcategorized object case marking (which, though it might be argued to correlate with semantic distinctions, is also grammaticalized and cannot be derived from semantic factors). Double case violations, by contrast, are interpretively relevant because they do not allow the language processing system to construct a semantic role hierarchy between the two arguments, i.e. to establish “who is acting on whom” (Frisch and Schlesewsky 2001, 2005). Strong converging support for this assumption was provided by Frisch and Schlesewsky (2001), who demonstrated that the N400 effect for double case violations vs. controls is eliminated when the two arguments differ in terms of animacy, i.e. when an additional interpretive prominence feature can help to set up an interpretive hierarchy between the two arguments (for a detailed discussion of prominence and argument interpretation including a summary of cross-linguistic findings, see Bornkessel-Schlesewsky and Schlesewsky 2009b). Similarly, the choice of ergative vs. nominative case in Hindi is interpretively relevant, since ergative case is restricted to marking Actor participants (i.e. the more Agent-like participant in a transitive relation) in perfective contexts, while nominative is compatible with a range of possible interpretations (including both Actor and Undergoer interpretations, with the Undergoer the more Patient-like participant in a transitive relation). Butt and King (2005) thus analyze ergative case in Hindi/Urdu as a “semantic case,” i.e. a case that is assigned under more restricted (semantic) circumstances than the default subject case (nominative).

In summary, then, the distinction between LAN and N400 effects for morphological processing at the sentence level appears to correlate with the distinction between formal and interpretively relevant information sources. Note that this dissociation cannot be equated with the distinction between syntactic and semantic information, which was long thought to correlate with LAN (and P600) vs. N400 effects (see Kutas, Van Petten, and Kluender 2006, for a recent overview), since all of the phenomena under consideration here are morphosyntactic in nature. These findings once again point to the interface nature of morphology by emphasizing that it is the type of relation that is important for the type of electrophysiological response rather than the examination of a morphological phenomenon per se. In addition, the robust distinction between (double) case and agreement violations in terms of ERPs further underscores our interpretation of the IIFG activations for morphological processing as not specific to morphology (see Section 15.2.2), since both agreement and double case violations have been shown to correlate with increased IIFG activation (Newman *et al.* 2003; Nieuwland, Martin, and Carreiras, in press). This

discrepancy may reflect the different aspects of neural activity that are captured by fMRI as opposed to electroencephalographic or magnetoencephalographic methods; possibly, the latter two are better suited to capturing the rapid, stimulus-locked signal changes associated with incremental (word-by-word) language processing (for discussion, see Vartiainen *et al.* 2009; Brennan and Pylkkänen 2012). In any case, the ERP distinctions clearly provide further empirical evidence for a more general interpretation of morphology-related IIFG activations. The notion that different aspects of relational processing are associated with qualitatively differing brain responses may also carry over to another of morphology's binary distinctions, namely the difference between syntagmatic and paradigmatic information. Both of these domains are relational in nature, but the type of relations that they involve differ from one another: syntagmatic relations are often described as "horizontal" because they are relations between elements within the same construction (i.e. sentence or word), while paradigmatic relations are "vertical" in that they apply to elements that can be substituted for one another within a construction (i.e. members of the same paradigm). To date, it has not been tested empirically whether this distinction also manifests itself neurally. However, some of the findings discussed above may provide some initial pointers. Recall, for example, that Marangolo and colleagues (Marangolo *et al.* 2003; Marangolo and Piras 2008; Marangolo and Piras 2010) argue that their patients' specific deficit for a particular type of derivational relation can be attributed to a more general deficit in selecting one alternative from a range of competitors. Crucially, the number of competitors is correlated with the complexity of the morphological paradigm. Thus, there may be a natural interrelation between paradigmatic relations in morphology and domain-general mechanisms of selection and cognitive control. Syntagmatic relations, by contrast, require the formation of dependencies and thus may draw upon a different set of cognitive mechanisms, which may once again be domain-general rather than language-specific. Yet this is only an assumption at present and thus clearly requires further corroboration from future empirical studies.

15.5 Summary and conclusions

Overall, the results reviewed in the present chapter indicate that neurobiologically plausible "computational primitives in morphology" are presently very difficult to isolate. This could be viewed as evidence for the position that there are, in fact, no such primitives, a position that would coincide with the "convergence theory" (Seidenberg and Gonnerman 2000), which assumes that morphology results from the convergence of codes between form (phonology/orthography) and meaning (semantics). Indeed, we have discussed many findings pertaining to morphological

processing which appeared to reflect more general distinctions (e.g. formal vs. interpretively relevant relations; task-related processing demands) rather than purely morphological processing aspects. The neurocognitive findings also indicate, however, that morphology differs from syntax in that it is relational rather than purely combinatorial in nature, an observation which appears to speak against the assumption of sublexical syntactic structures. We have argued here that many of the brain correlates of morphological processing are best understood as indexing different aspects of this relational processing, for example whether the relation is interpretive or formal in nature. This finding appears in natural accord with the assumption that morphology is an interface phenomenon, as do the effects of semantic and phonological structure that have recently been shown to be of prime importance for morphological processing. Finally, the observation that morphological effects may not be the same across languages in the sense that only morphologically rich languages show independent effects of morphological structure (i.e. effects that are not correlated with other domains such as semantics) points to yet another level of distinctions that must crucially be taken into account in future research. In our view, these fine-grained differences and the range of dimensions within which they occur indicate a clear way forward for research on morphology and the brain: By examining rich and finely structured sets of distinctions within and across languages, ideally using naturalistic paradigms, and observing how these impact upon brain activity, we may soon be in a position to come up with new empirical generalizations that are firmly grounded in neurobiology.

Appendix List of abbreviations

ACC	anterior cingulate cortex
ATL	anterior temporal lobe
BA	Brodmann area
BG	basal ganglia
l	left
DP	declarative-procedural
ELAN	early left anterior negativity (i.e. a negativity with a maximum at approximately 150 ms post critical stimulus onset with a left-anterior distribution)
ERPs	event-related brain potentials
IFG	inferior frontal gyrus
fMRI	functional magnetic resonance imaging
LAN	left anterior negativity (i.e. a negativity between approximately 300 and 500 ms post critical stimulus onset with a left-anterior distribution)
MFG	Middle frontal gyrus
MTG	Middle temporal gyrus

N400	a negativity between approximately 300 and 500 ms post critical stimulus onset with a centro-parietal distribution
OT	occipitotemporal cortex
P600	a positivity with a maximum at approximately 600 ms post critical stimulus onset with a parietal distribution
PET	positron emission tomography
r	right

16

Grounding the cognitive neuroscience of semantics in linguistic theory

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Jonathan Brennan, and
Douglas Bernis

16.1 Introduction

If a visitor from Mars was told what the Cognitive Neuroscience of Language is and what Linguistics is, they would presumably immediately assume that the two disciplines must be tightly connected – after all, how could one study the neural bases of language without an understanding of what language is? At the present time, however, the connection between the two fields remains rather loose. Linguists have not done a terrific job in making their results accessible to the general public, and perhaps neuroscientists have also not reached out sufficiently to seek theoretical grounding for their research. In our work, we seek to improve the situation for one particular subdomain of language: combinatory semantics.

Of the various core processing levels of language, i.e. phonetics, phonology, syntax, and semantics, the cognitive neuroscience of semantics has been the most divorced from Linguistics. For example, for phonology, although there is no cognitive neuroscience for most questions that phonologists are interested in, there is, for instance, a wide literature studying the neural bases of phonological categories assuming exactly the types of phoneme representations proposed by phonological theory (e.g., Dehaene-Lambertz 1997; Näätänen *et al.* 1997; Hickok and Poeppel 2000). Similarly for syntax, research probing into the function of Broca's area, for example,

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has assumed tree representations of the sort that one would also find in an introductory syntax textbook (e.g., Caplan *et al.* 1999; Embick *et al.* 2000; Grodzinsky and Friederici 2006; Moro *et al.* 2001; Patel 2003; Stromswold *et al.* 1996). Not so for semantics. In fact, the word “semantics” tends to mean rather different things in Cognitive Neuroscience and in Linguistics. In Linguistics, the semantics of an expression refers to the complex meaning that is computed by combining the meanings of the individual lexical items within the expression. The formal rules that are employed in this computation have been studied for about forty years, and many aspects of these rules are now well understood. But until recently, despite the clear necessity of understanding the neurobiology of the combinatory functions that derive sentence meanings, we have had no cognitive neuroscience on the brain bases of these rules. Instead, cognitive neuroscience research with “semantic” in the title most often deals with the representation of conceptual knowledge, investigating questions such as whether living things and artifacts are distinctly represented in the brain (e.g., Mummery *et al.* 1998), or how different types of object concepts are represented (Martin and Chao 2001). The fact that this literature is disconnected from Linguistics is not devastating – the questions are rather different from those that concern linguists. Although linguistic semantics does include a subfield called “lexical semantics” that focuses on word meaning, even this subfield is mostly interested in word meaning that appears to be composed of smaller parts – i.e. it has some type of complexity to it. The representational differences between tools and animals is not a big research question in Linguistics and thus the brain research on conceptual knowledge has less to gain from linguistic theories.

There is another line of brain research on “semantics,” however, that should and needs to connect with the types of theoretical models of meaning representation offered by Linguistics. This is the so-called “N400 literature,” which uses the N400 as an index of “semantic integration.” Although this functional interpretation is assumed by a sizeable research community (see Lau *et al.* 2008 for a recent review), the N400 was not discovered via a systematic search for a neural correlate of a theoretically defined notion of “semantic integration.” In fact, the behavior and the source localization of the N400 are much more compatible with a lexical access based account (Lau *et al.* 2008), making the N400 an unlikely index of semantic integrative processes.

In this article, we outline what we believe a theoretically grounded cognitive neuroscience of semantics should look like. Our focus is on combinatory semantics, i.e. the composition operations that serve to build complex meanings from smaller parts. We take formal syntax and semantics of the generative tradition (e.g., Heim and Kratzer 1998) as the cognitive model that guides this research and defines the operations whose neurobiology is to be investigated. We assume, hopefully uncontroversially, that the right way to uncover the neural bases of semantic

composition is to systematically vary, or otherwise track, this operation during language processing. In our own research, we have aimed to do exactly this. We will first define exactly what we mean by “semantic composition,” then summarize our findings so far, discuss some open questions, and finally, articulate our hopes for the future.

16.2 Semantic composition: Defining the core operations

Theories of formal semantics are models of the possible meanings and semantic combinatorics of natural language (e.g., Montague 1970; Dowty 1979; Parsons 1990; Steedman 1996; Heim and Kratzer 1998). They aim to give a complete account of the representations and computations that yield sentence meanings, including relatively straightforward rules that combine predicates with arguments and adjuncts and extending to more complicated phenomena such as the interpretation of tense, aspect, focus, and so forth. The goal of these models is to understand and formalize the nature of semantic knowledge both within languages and cross-linguistically. Consequently, theories of formal semantics provide an extremely rich and detailed hypothesis space both for psycho- and neuro-linguistic experimentation.

One challenge for rooting the cognitive neuroscience of semantics in formal theories of meaning is deciding what theory to follow; unsurprisingly, different models make different claims about semantic representations and the mechanisms by which they are computed. Theoretical semantics is a lively field with debate at every level of analysis, ranging from foundational questions (such as what is the basic relationship between syntax and semantics?) to issues relating to the formal details of the analysis of a specific construction within a specific language. Such variance of opinion may seem bewildering to a neuroscientist trying to figure out what aspects of natural language meaning semanticists agree on, such that those phenomena could safely be subjected to a brain investigation.

Unfortunately, there is no safe route: like any other branch of cognitive science, semantic theory is ever evolving and virtually every aspect of it has been or at least can be questioned. However, it is still possible to identify certain basic operations as a starting point, while keeping in mind the possibly controversial aspects of the formal treatment. In what follows, we describe two operations that form the core of the combinatorial engine in most semantic theories (for a fuller exposition, see Pylkkänen and McElree 2006). Our notation follows Heim and Kratzer (1998), but again, for our present purposes the details of the rules are not important; rather, what is crucial is that this type of theory distinguishes between two different modes of composition, one that serves to fill in argument positions of predicates and one that modifies the predicate without impacting its arguments. Although mainstream, this distinction

is not a formal necessity nor necessarily empirically correct (e.g., Pietroski 2002, 2004, 2006), but given its central role in most semantic theories, it one of the most basic distinctions that can be subjected to a brain investigation. Since one reason for the disconnection between cognitive neuroscience and formal semantics has likely been the relative impenetrability of semantic theories for the non-linguist, in the following we aim for a maximally informal and accessible description of the basic ideas.

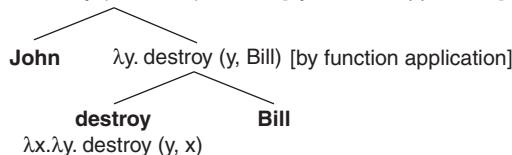
16.2.1 Function application

The driving force behind mainstream theories of semantic composition is the idea that the meanings of most words are in some sense incomplete, or “unsaturated,” unless they are combined with other words with suitable meanings (Frege 1892). For an intuitive example, the semantics of action verbs are thought to have “placeholders,” or variables, that stand for the participants of the actions described by the verbs. In order for the meanings of these verbs to be complete, or saturated, the verb needs to combine with noun phrases that describe those participants. More formally, these verbs are treated as functions that take individual-denoting noun phrases as their arguments. This idea is expressed in lambda calculus for the transitive verb *destroy* in (1) below. The arguments of the function are prefixed with lambdas, and the value, or the output of the function, follows the lambda terms.

- (1) **destroy:** $\lambda x.\lambda y. \text{destroy}(y, x)$

In order to combine the verb *destroy* with its arguments, we apply a rule called *function application*, which essentially replaces the variables in the denotation of the predicate and erases the lambda prefixes. Proper names are the most straightforward individual denoting noun phrases and thus the representation of the sentence *John destroyed Bill* would involve roughly the following representation (here we ignore everything that does not pertain to the argument saturation of the verbal predicate).

- (2) $\text{destroy}(\text{John}, \text{Bill})$ [by function application]

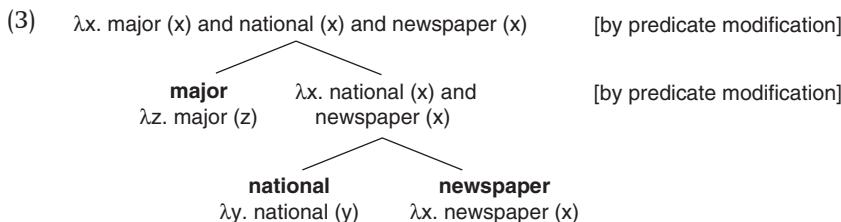


In addition to simple examples such as the one above, function application is the mode of composition for any predicate and its argument. For example, prepositions combine with noun phrases via function application [in [the dark]] as do verbs with clausal complements [knew [that John destroyed Bill]] and verbal ones [John tried [to destroy Bill]]. Thus

function application accounts for a vast amount of semantic composition in natural language and every theory of semantics requires a rule that accomplishes its task. Consequently, it should be regarded as one of the core operations that any theory of the brain bases of language should address.

16.2.2 Predicate modification

The gist of function application is that it satisfies the semantic requirements of a predicate and makes it more complete. But most expressions also have parts that are not formally required, i.e. they could be dropped and the expression would still be grammatical. For instance, although we could describe *The New York Times* simply as a *newspaper*, we could also describe it as a *major national newspaper*. There are no grammatical reasons for why we would ever have to add these adjectives – unlike with *destroy* above, which simply could not function grammatically without, say, its object (**John destroyed*). The adjectives simply provide additional, potentially useful information about the noun. The majority of these types of optional elements are added into expressions via a rule called *predicate modification*. Predicate modification builds complex properties from simpler ones. In formal semantics, adjectives and nouns are both treated as describing properties of individuals, e.g., *newspaper* denotes the property of being a newspaper, and *major* the property of being major. Predicate modification simply concatenates these properties, such that *major national newspaper* ends up meaning “the property of being major, and national, and a newspaper,” as shown in lambda notation in (3).



In addition to building complex properties of individuals, predicate modification can also combine any two predicates that describe properties of the same type of thing. For example, in event-based semantic frameworks (e.g., Davidson 1967; Kratzer 1996; Parsons 1990), both verbs and adverbs are thought of as describing properties of events. Thus the mode of composition between, say, *run* and *quickly* would be a version of predicate modification, such that the resulting verb phrase would describe events that are both quick and events of running.

In sum, while function application is the default mode for adding arguments into a structure, predicate modification is the most basic

way to add optional elements. Although natural language semantics as a whole requires a much more elaborate system than just these two rules, function application and predicate modification constitute the nuts and bolts of semantic composition in most theories, and thus offer clearly defined computations whose neural bases can be characterized within the cognitive neuroscience of language. It is of course an empirical question whether function application and predicate modification dissociate in brain activity. If they do not, this would be a type of null result and not necessarily a motivation for linguists to revise their theories, but a lack of dissociation would obviously call into question the assumption that the operations are distinct. In this type of situation, the brain data would be more compatible with a theory where semantic composition is achieved via a unified operation across expression types, as proposed for example by Pietroski (2002, 2004, 2006). In the long run, this is the type of interaction between brain experimentation and linguistic theorizing that we advocate.

16.3 Semantic representations vs. their plausibility given world knowledge

Theories of formal semantics have a crisp definition of semantic well-formedness: an expression is semantically well formed if the rules of composition yield a meaning for it. For example, in the default case, there is no way for a verb such as *arrive*, i.e. a function that takes a single individual-denoting argument, to combine with two such arguments, given that this predicate only has one argument slot for function application to saturate. Thus an expression such as *I arrived Helsinki* is considered uninterpretable under this type of theory; to make it interpretable, another predicate would need to be added, such as the preposition *in*, which can then take *Helsinki* as its argument.

This definition of well-formedness is very different from a layman's intuition of what "makes sense," i.e. an expression can be semantically well formed in a formal sense even if it means something crazy. For example, *I just ate a cloud* is a nonsensical sentence in that the situation it describes could never happen (unless the world was significantly altered). However, every native speaker of English should have the intuition that they know what this sentence means and, in fact, this meaning is easily computed by the two rules introduced above. In terms of formal semantics, the oddness of the sentence is not revealed until one compares its semantic representation (computed by the rules) to one's knowledge of the world, which dictates that clouds are not edible entities. But crucially, the sentence is not semantically ill-formed, but rather just ill-fitting to our world knowledge.

This conception of semantic well-formedness is dramatically different from what is generally assumed in neuroimaging and especially in event-related potentials (ERP) research on semantics. Such brain research has been overwhelmingly dominated by studies that vary “semantic well-formedness” or “congruity,” but in the majority of these experiments, the semantically ill-formed stimuli are not ill-formed in the above described, formal sense; rather, in terms of linguistic theory, they violate world knowledge instead. The original ERP study on such violations, by Kutas and Hillyard (1980), used sentences such as *he spread the warm bread with socks* to violate what they called “semantic appropriateness.” These violations elicited a negative-going ERP peaking around 400 ms, the N400, a finding that has since been replicated in hundreds of studies. Since the N400 is elicited by semantically surprising elements, it is commonly thought of as a semantics-related ERP. However, this notion of “semantics” is not theoretically grounded, i.e. we are not aware of any theory of linguistic representation according to which typical N400 sentences would count as semantically ill-formed (and even if there is such as a theory, the N400 literature does not reference it). For instance, in the above *he spread the warm bread with socks* example, the final word *socks* composes without a problem with the preposition *with*, to yield a meaning in which socks are used as a spread on bread. Now socks are of course not edible or spreadable, but in terms of linguistic theory, this is a fact about the world, not a fact about language. Given that there is no crisp definition of what “semantics” means in the N400 literature, considering this component a semantics-related ERP is not conducive to building a theoretically grounded model of language in the brain.

It is an empirical question whether N400-type effects could also be obtained for expressions that are semantically ill-formed in the linguistic sense. There are a few studies that have run relevant manipulations, primarily focusing on violations of negative polarity licensing (Drenhaus *et al.* 2006; Saddy, Drenhaus, and Frisch 2004; Steinhauer *et al.* 2010; Xiang, Dillon, and Phillips 2009). Negative polarity items (NPIs) are expressions such as *ever* or *any* that must occur in the scope of negation¹ (e.g., *No tourist ever visited the old harbor* vs. **One tourist ever visited the old harbor*). Violations of this licensing condition count as true semantic violations in terms of linguistic theory, in that if an NPI is not licensed by a suitable operator, no semantic representation can be built for the sentence. Negative polarity violations have indeed been reported to elicit N400 effects (Saddy *et al.* 2004), but also P600 effects (Drenhaus *et al.* 2006; Xiang *et al.* 2009) and late left anterior negativities (Steinhauer *et al.* 2010). Thus the ERP profile of these violations has been quite variable and does not easily lend itself to firm conclusions. Most relevantly for our present purposes, however, negative polarity violations have exhibited rather different ERP profiles from world knowledge violations when the two are examined within the same study (Steinhauer *et al.* 2010).

In sum, most extant research on the cognitive neuroscience of sentence level meaning has operated with only an intuitive notion of “semantics,” not grounded in any particular theoretical model. It should, however, be uncontroversial that technical terminology in any field needs to be clearly defined; otherwise it is unclear what the research is even about. Given the vast amount of work devoted to carefully characterizing crisp, formal semantic theories, disconnecting the cognitive neuroscience of semantics from these models is a missed opportunity. Only by grounding the neurobiology of meaning in theoretical models of meaning can we move forward to a “predictive mode” of research, where the properties of a cognitive model guide the experimentation and make predictions about what phenomena should group together. Without such grounding, one is left with complex data sets and a layman’s intuition about what it all may mean.

16.4 The challenge of compositionality

As soon as one couches the brain investigation of semantics in linguistic theory, a methodological dilemma, however, arises: In most cases, the semantic complexity of an expression tightly correlates with its syntactic complexity, and thus it is not obvious how one could ever vary semantic composition without syntactic confounds. Even worse, linguistic theories differ in the extent to which syntax and semantics are coupled. In some theories, every step of the semantic derivation corresponds to a step in the syntactic derivation (e.g., Heim and Kratzer 1998; Montague 1970). Other theories assume a weaker coupling between syntax and semantics, allowing certain operations to only apply in the semantics without a corresponding syntactic step (Barker 2002; Jacobson 1999; Hendriks 1988; Partee and Rooth 1983). And finally, in Jackendoff’s parallel model, syntactic and semantic representations are built by completely independent mechanisms, creating an architecture that forces no correlation between the number of syntactic and semantic steps (Jackendoff 2002).² The disagreement between these theories has to do with the degree of *compositionality* in natural language, i.e. the extent to which the meanings of expressions are straightforwardly determined by the meanings of their parts and the syntactic combinatorics.

With this type of uncertainty about the fundamental relationship between syntax and semantics, how could a cognitive neuroscientist use these theories to make headway in understanding the brain bases of semantic composition? In our own research, we have adopted what might be called a “bite the bullet” approach. First, we have to accept that there are no expressions that uncontroversially involve semantic computations that do not correspond to any part of the syntax: even if some meaning component of an expression does not appear to map onto the

syntax, one can always postulate a phonologically null syntactic element that carries that piece of meaning. The question though is, whether such an analysis makes the right empirical predictions. As discussed in Pylkkänen (2008), there is great asymmetry in the degree to which this type of solution works for different cases; it sometimes makes the right predictions, but often not. As a starting point in our research, we have aimed to study exactly the expressions that are most likely to involve syntactically unexpressed semantic operations, even if this dissociation cannot be definitively proven. As discussed in the next section, our subsequent research has shown that these types of cases pattern differently from cases where syntax and semantics *are* uncontroversially coupled, lending support to the notion that the initial set of test cases did, in fact, only vary semantics. Thus compositional coupling of syntax and semantics makes the project of “neurosemantics” a difficult one to get off the ground, but once some initial progress is made, we believe it may be possible for brain data to actually contribute to debates about the relationship between syntax and semantics.

16.5 The Anterior Midline Field (AMF) as a correlate of semantic composition

16.5.1 Varying semantic composition: Semantic mismatch

Although much of natural language appears strongly compositional, there are classes of expressions whose meanings appear richer than their syntax. Perhaps the best studied example is so-called *complement coercion*, illustrated in (4) below.

- (4) a. The professor began the article.
b. The boy finished the pizza.
c. The trekkers survived the mountain.

When native speakers are queried about the meanings of these sentences, they typically report for (4a) that the professor began reading the article, for (4b) that the boy finished eating the pizza and for (4c) that the trekkers survived climbing the mountain. Reading, eating, or climbing do not, however, figure in the lexical material of these expressions. Thus where do these implicit activity senses come from? They are typically thought of as arising from a certain semantic mismatch between the verbs and their direct objects. Semantically, each of the verbs in (4) selects a direct object that describes some type of event or activity: one generally begins, finishes, or survives *doing something*. However, none of the direct objects in (4) describe events, rather, they all denote entities. This kind of situation is formally considered a semantic “type mismatch” and in the default case, type mismatch leads to ungrammaticality. However, the sentences in (4) are all grammatical and thus the type mismatch must be somehow

resolved. Descriptively, the resolution appears to involve the semantic insertion of an implicit activity that can mediate between the verb and the otherwise ill-fitting object NP. Formally, the complement of the verb (the object NP) is thought to “coerce” into an event meaning (Jackendoff 1997; Pustejovsky 1995), such that semantic composition can succeed. This type of analysis treats coercion as a purely semantic meaning-adding operation, with no consequences for the syntax.³

Coercion does, however, have consequences for online processing: numerous studies have shown that coercion expressions take longer to process than control expressions involving no coercion (for a review, see Pykkänen and McElree 2006). This psycholinguistic evidence provides empirical support for the hypothesis that coercion expressions do, in fact, involve some type of extra computation, absent in more transparently compositional expressions. Given that the processing of complement coercion had already been well studied behaviorally, we used it as the initial test case in our attempt to identify a neural correlate of purely semantic combinatoric processing.

To measure brain activity, our research uses magnetoencephalography (MEG), which offers the best combination of both spatial and temporal resolution of existing cognitive neuroscience techniques. MEG measures the magnetic fields generated by neuronal currents. The primary difference between MEG and EEG is that in MEG, the locations of the current generators can be estimated reasonably accurately given that magnetic fields pass through the different structures of the brain undistorted, contrary to electric potentials. Typically, the current sources of MEG recordings are modeled either as focal sources (so-called single dipoles) or as distributed patches of activation on the cortex (typically minimum norm estimates) (Hansen, Kringsbach, and Salmelin 2010). The intensities and latencies of these current sources then function as the primary dependent measures of most MEG studies, although an EEG-style analysis of sensor-data without source localization is also always an option.

When we contrasted coercion expressions (*the journalist began the article*) with non-coercive control sentences (*the journalist wrote the article*) during an MEG recording, we observed increased activity for coercion in a prefrontal midline MEG field, dubbed the Anterior Midline Field (AMF) (Figure 16.1). Source localization indicated the ventromedial prefrontal cortex (vmPFC) as the generating brain region of this magnetic field. No such effect was observed for implausible control sentences, suggesting different brain bases for the computation of coercion and the detection of poor real-world fit.

These complement coercion findings established a starting point for our research on the neural bases of semantic composition. Our subsequent studies have then aimed to narrow down the possible functional interpretations of this activity. Figure 16.1 summarizes all of our AMF results so far. First, we examined whether the AMF effect generalizes to other coercion constructions, and found that it is also observed for a different variant of

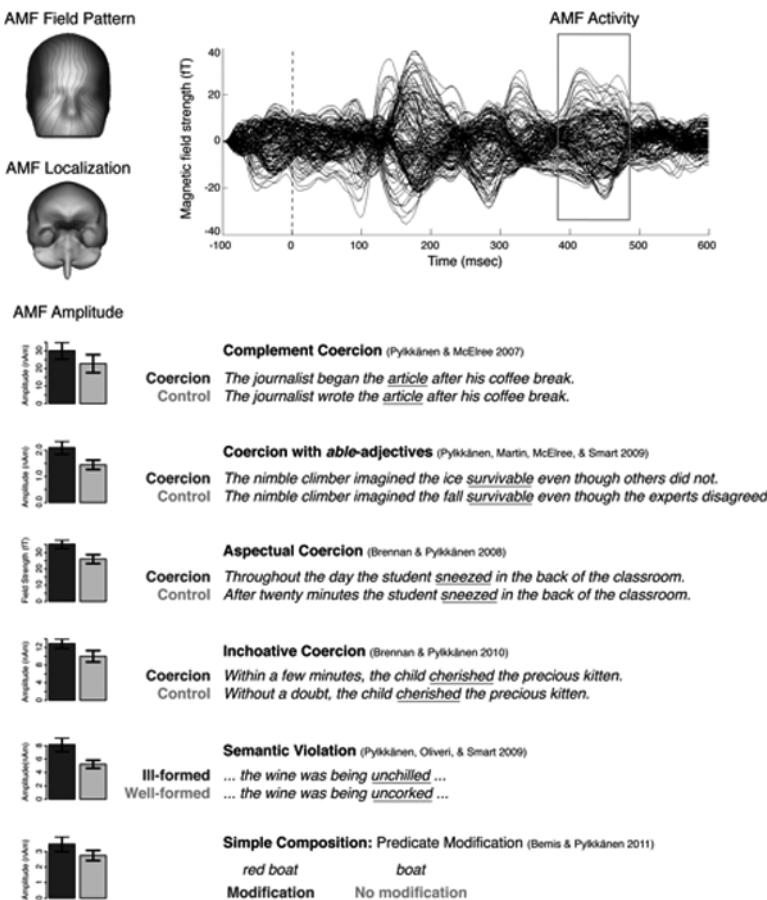


Figure 16.1 Summary of all our AMF results to date, including the typical MEG response to a visual word presentation in a sentential context (top panel). The upper left corner depicts the AMF field pattern and a typical localization. In the stimulus examples, the critical word is underlined.

complement coercion (Pylkkänen *et al.* 2009) as well as for two different types of aspectual coercion (Brennan and Pylkkänen 2008, 2010a). These findings ruled out the hypothesis that the AMF reflects processing specific to complement coercion.

We have also examined the AMF in a violation paradigm (Pylkkänen, Oliveri, and Smart 2009), to better connect our findings to ERP research which has been dominated by this type of design. In this study, we again used semantic mismatch, but of a sort that cannot be resolved by a productive rule, but rather results in semantic ill-formedness (in the linguistic sense, i.e. no well-formed representation can be built). These semantic violations were contrasted both with violations of world knowledge (similar to the “semantic violations” of the N400 literature) and with well-formed control expressions, in order to assess in an ERP-style violation paradigm whether semantic violations but not world knowledge

violations would affect the AMF, as would be predicted if this activity reflected the composition of semantic representations but not the evaluation of their real-world fit. Our results indeed patterned according to this prediction: semantic violations elicited increased AMF amplitudes, while world knowledge violations generated a different type of effect.

Thus our initial set of experiments employed resolvable and unresolvable semantic mismatch in order to vary semantic composition while keeping syntax maximally constant. These studies yielded highly consistent results, implicating the AMF MEG component as potentially relevant for the construction of complex meaning. This effect demonstrated task-independence (Pylkkänen *et al.* 2009) and was insensitive to world knowledge (Pylkkänen, Oliveri, and Smart 2009). But of course these results do not yet show that we have isolated activity reflecting semantic composition in some general sense, as opposed to activity that plays a role in mismatch resolution but does not participate in more ordinary composition. Further, the psychological nature of coercion operations is vastly underdetermined by traditional linguistic data (e.g., judgments of grammaticality and truth-value); in some sense “coercion” and “type-shifting” are descriptive labels for computations that matter for interpretation but do not appear to have a syntactic nature. In other words, although coercion rules are traditionally thought of as operating within the compositional semantic system, it is difficult to demonstrate this empirically, i.e. for example to rule out the possibility that these meaning shifts might be essentially pragmatic in nature. Having discovered that coercion affects a particular brain response, i.e. the AMF, it becomes possible to investigate whether the same response would also be affected by simpler manipulations of semantic composition, involving no mismatch resolution. If ordinary “run-of-the-mill” composition were also to modulate the AMF, this would show that the role of the AMF is not limited to mismatch resolution and would also offer empirical support for the hypothesis that coercion operations involve semantic computations similar to those involved in transparently compositional expressions. The research summarized below aimed to assess this by studying very simple cases of composition, involving only the intersective combination of a noun and an adjective.

16.5.2 Simple composition: Bringing in syntax and the left ATL

Our study on simple composition constitutes, to our knowledge, the first neurolinguistic investigation directly targeting one of the core semantic operations outlined in Section 16.2 above. Specifically, we aimed to measure the MEG activity associated with predicate modification (Bemis and Pylkkänen 2011). Contrary to most extant brain research on syntax and semantics, which has generally employed full sentences involving complex structures such as center-embedding or semantic anomalies (for

reviews, see Kaan and Swaab 2002, and Lau et al. 2008), our study employed minimal phrases invoking exactly one step of semantic composition. The critical stimulus was an object-denoting noun that was either preceded by a semantically composable color adjective (*red boat*) or by consonant string activating no lexical meaning (*xkq boat*). Subjects viewed these phrases (and other control stimuli) and then decided whether or not a subsequent picture matched the verbal stimulus. If the role of the AMF is limited to mismatch resolution, it clearly should not be affected by this maximally simple manipulation. In contrast, if the AMF reflects aspects of semantic composition quite generally, nouns preceded by adjectives should elicit increased AMF activity. The latter prediction was robustly confirmed: a clear AMF amplitude increase was observed for the adjective–noun combinations relative to the isolated nouns, ruling out the hypothesis that the AMF is purely mismatch related.

But predicate modification is of course not the only combinatorial operation varied in this manipulation: each adjective–noun pair also made up a syntactic phrase. Thus the above contrast should also elicit effects related to the syntactic side of the composition, if this is indeed something separable from the semantic combinatorial effects. We did, in fact, also observe a second effect for the complex phrases, and in a location familiar from a series of prior studies. This effect localized in the left anterior temporal lobe (ATL), which has been shown by several imaging studies as eliciting increased activation for sentences as opposed to unstructured lists of words (Mazoyer et al. 1993; Stowe et al. 1998; Vandenberghe et al. 2002; Friederici et al. 2000; Humphries et al. 2006; Rogalsky and Hickok 2008). This body of work has hypothesized that the left ATL is the locus of basic syntactic composition, with Broca's region only participating in more complex operations. This interpretation is further corroborated by imaging results of our own, showing that while subjects listen to a narrative, activity in the left ATL correlates with the number of syntactic constituents completed by each word (Brennan et al. 2012). Thus the combination of the results reviewed so far suggests the following working hypothesis: the AMF generator, i.e. the vmPFC, and the left ATL are the primary loci of basic combinatorial processing, with the vmPFC computing semantic and the left ATL syntactic structure.

The hypothesis just articulated raises a puzzle, though, regarding the relationship between our results on the vmPFC and the just mentioned imaging studies contrasting sentences vs. word lists. Clearly, the sentence vs. word list contrast varies not only syntactic composition but also semantic composition, yet none of the studies using this contrast have reported effects in the vmPFC. This is obviously incompatible with our semantic composition hypothesis regarding this region. But the sentence vs. word list studies have also used a different technique from our research, measuring slowly arising hemodynamic activity as opposed to electromagnetic activity, which can be measured at a

millisecond temporal resolution, matching the speed of language processing. To assess whether a vmPFC effect for sentences over word lists would be observed in MEG, we conducted an MEG version of the traditional imaging design (Brennan and Pylkkänen 2012). Our results indeed revealed a clear amplitude increase in the vmPFC for sentences, conforming to the composition hypothesis. An effect was also seen in the left ATL, replicating the prior imaging results. While questions remain about why hemodynamic methods and MEG should yield different results here, the picture emerging from our MEG findings is encouraging and begins to have the shape of an elementary theory about the neural bases of syntactic and semantic composition, grounded in linguistic theory.

16.5.3 Challenges and open questions

16.5.3.1 Relation to hemodynamic and neuropsychological data

Our MEG research so far suggests a prominent role for the generating region of the AMF in language processing. However, if the role of this midline prefrontal region is as basic as semantic composition, how come this area has not already figured in neuroimaging studies on sentence processing? As just discussed, it is possible that MEG may be better suited for capturing this activity than hemodynamic methods, given our findings on the sentence vs. word list paradigm which yields a vmPFC effect in MEG but not in fMRI or PET (granted that we have not yet conducted a study with the same materials and subjects across the techniques). One possible reason for this difference lies in the better time resolution of MEG: a short-lived time-locked effect lasting less than 100ms should naturally be difficult to capture with techniques whose temporal resolution is on the scale of several seconds. Further, fMRI in particular is limited when it comes to measuring signal from the vmPFC and the orbitofrontal cortex in general. This is due to the proximity of these regions to the sinuses, which leads to so-called susceptibility artifacts, resulting in significant signal loss in orbitofrontal regions (Ojemann *et al.* 1997). Despite these limitations, however, several hemodynamic studies have shown interpretation-related effects in the vmPFC, both in PET (e.g., Maguire, Frith, and Morris 1999; Nathaniel-James and Frith 2002) and in fMRI (Nieuwland, Petersson, and Van Berkum 2007). Thus it is not the case that our findings are entirely without precedent in the neuroimaging literature.

One type of evidence that does not suffer from the artifact issues described above pertains to neuropsychological data. Do patients with ventromedial prefrontal damage suffer severe language processing deficits, of the sort one would expect of a person who has lost their ability to construct complex linguistic meanings? The most simple-minded

prediction of the semantic composition hypothesis might be that such patients should not be able to understand anything or be able to produce any coherent sentences. This prediction is certainly not borne out: language problems are not the predominant issue for this patient population. Instead, their most salient impairments have to do with appropriateness of social behavior, decision making, and emotion regulation (Burgess and Wood 1990; Grafman *et al.* 1996; Barrash *et al.* 2000; Berlin *et al.* 2004; Anderson *et al.* 2006; Koenigs and Tranel 2007). Perhaps because language problems do not present prominently, language skills are typically not even reported, or they may be informally commented on as “normal” (e.g., Anderson *et al.* 2006). There is though one body of research on vmPFC patients that has focused on language processing, specifically on sarcasm and irony. In this work vmPFC damage has been found to lead to deficits in comprehending sarcasm, a finding that has been related to the role of the vmPFC in theory-of-mind processing (Shamay-Tsoory, Tomer, and Aharon-Peretz 2005; Shamay-Tsoory, Tibi-Elhanany, Aharon-Peretz 2006). This finding is obviously related to semantic processing, but not at the very basic level that our most recent simple composition results suggest (Section 16.5.2).

What then, to make of these relatively sparse neurolinguistic data on vmPFC patients in light of the semantic composition hypothesis of the AMF? At least two considerations are important to note here, one methodological and the other theoretical. The methodological consideration is that although MEG systematically localizes the AMF in the vmPFC (with some variation on the anterior-posterior axis), this localization is a source model and not necessarily the absolute truth. For example, so far most of our source localizations have used a smoothed average cortex (provided by BESA, the source localization software), which does not include the medial wall. In this kind of source model, activity in the anterior cingulate, for example, might be projected onto ventromedial prefrontal cortex. Thus it is not yet obvious that vmPFC patients are necessarily the right clinical population to consider. Clearly, the localization of the AMF will need to be further assessed with hemodynamic methods, although as just reviewed, this approach presents challenges of its own.

The theoretical consideration has to do with the precise predictions of the semantic composition hypothesis. What should a person’s linguistic performance look like if they have lost their ability for semantic composition but have intact syntax and lexical-semantic processing? The answer is far from obvious. Such a person would have a combinatory system, i.e. the syntax, and they would understand the meanings of words. Thus it is possible that they might be able to reasonably work out what sentences mean, only failing in subtler situations. Thus perhaps the only clear prediction of the semantic composition hypothesis is that a person without the relevant region should show some semantic deficits. Whether vmPFC patients fit this profile is currently unclear; assessing this would require

sophisticated linguistic testing perhaps focusing on semantic phenomena that are not transparently reflected in the syntax.

16.5.3.2 Domain-generality

One of the most fundamental questions regarding the neural architecture of language is the extent to which the mechanisms employed by language are specific to language or also used in other domains. Localization wise, our findings on the possible brain bases of semantic composition have been quite surprising: the midline prefrontal regions that are modeled as the AMF source are not generally thought of as “language regions.” Instead, these areas, and the vmPFC specifically, are prominently associated with various types of non-linguistic higher cognition, such as emotion (Bechara, Damasio, and Damasio 2000; Damasio 1994), decision making (Wallis 2007), representation of reward value (Schoenbaum, Roesch, and Stalnaker 2006), and social cognition, including theory-of-mind (Amadio and Frith 2006; Baron-Cohen and Ring 1994; Baron-Cohen *et al.* 1994; Gallagher and Frith 2003; Krueger *et al.* 2009; Rowe *et al.* 2001). Thus it seems plausible that the AMF may reflect computations that span across multiple domains, a speculation we already put forth in the first AMF paper (Pylkkänen and McElree 2007) and have later refined (Pylkkänen 2008; Pylkkänen, Oliveri, and Smart 2009). A more specific way to put this hypothesis is that the AMF reflects semantic composition and that semantic composition employs mechanisms also used in other domains. To test this, our on-going research is aimed at assessing whether the AMF composition effects extend to combinatorial phenomena in non-linguistic domains.

16.6 Looking ahead

Moving forward, here is what we would like to *not* see happen in the cognitive neuroscience of semantics: for neuroscientists to “reinvent the wheel” when it comes to cognitive models of semantic representation. We understand that linguistic theory offers a tremendous amount of detail, which may seem daunting to try to relate to neurobiological data. Thus it may be tempting to flatten some of those distinctions when aiming to characterize the brain bases of linguistic functions, as in, for example, Peter Hagoort’s model, where all combinatorial operations of language are treated as examples of a general “unification” function (Hagoort, 2005). We actually think it is quite possible that the brain performs composition in various domains in rather similar ways – this intuition is largely what drives our work on the domain generality of the AMF – but we do not believe that a flat cognitive model is the right kind of theory to guide us when designing experiments. If we do not look for certain (theoretically well-motivated) distinctions in the brain, we certainly will not find them, should they, in fact, be there.

To conclude, natural language meaning exhibits many fascinating phenomena, most of which one is not consciously aware of without engaging in the formal study of semantics. If theoretical semantics and the cognitive neuroscience of semantics are not in communication with each other, we will never understand how the brain achieves the astounding task of comprehending and producing the meanings of human language in all their glory.

17

Modularity and descent-with-modification

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17.1 Introduction

In computer science and engineering, the idea of modular design is axiomatic (Baldwin and Clark 2000). Complex systems are made up of specialized subcomponents designed for particular functions. A modern computer, for instance, includes not just a domain-general central processing unit (itself constructed out of modular subcomponents) but also a wide variety of specialized circuits dedicated to tasks like rendering graphics and producing sound effects; much the same can be said for virtually all of the world’s complex computer programs.

There is ample evidence that many biological structures are similarly modular, at physiological and genetic levels (Beldade and Brakefield 2003; Gerhart and Kirschner 1997; Patthy 2003; Schlosser and Wagner 2004; Schwartz and Olson 1999; Wagner, Pavlicev, and Cheverud 2007). For example, systems such as the vertebrae of the spine consist of serially repeated subcomponents, while the body as a whole consists of specialized systems devoted to tasks such as circulation, digestion, and respiration.

Mental and neural structures,¹ too, might be modular in structure (Fodor 1983; Lenneberg 1967; Marr 1982), and such modularity might be an essential or highly probable aspect of the evolution of complex systems, at both cognitive (Cosmides and Tooby 1994; Pinker 1997) and neural levels (Calabretta *et al.* 2003; Calabretta *et al.* 2000; Redies and Puelles 2001).²

Consistent with this idea, the brain in many ways appears to carry hallmarks of specialization. The adult human neocortex, for example, contains a large number (52 by Brodmann’s count) of “areas” that can be distinguished cytoarchitectonically in terms of connection patterns and

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distributions of cell types. Felleman and van Essen's (1991) justly famous map of the interconnections in the visual part of a macaque brain hints at a staggering degree of highly focused connectivity (Sporns and Kotter 2004). Comparable studies have not been done in infants, but the available evidence strongly suggests that basic cortical structures and pathways are largely established by birth (Bates *et al.* 2002; Shankle *et al.* 1998).

Yet, the twin notions of neural and cognitive modularity have remained deeply controversial within cognitive science and cognitive neuroscience. There are ongoing debates about the definition of modules (Coltheart 1999), and protracted debates about the empirical evidence within psychological domains such as sentence processing (Ferreira and Clifton 1986; Spivey *et al.* 2002; Tanenhaus *et al.* 1995), reading (Coltheart 1999; Shatil and Share 2003), music (Peretz and Coltheart 2003), and navigation (Hermer and Spelke 1996; Learmonth, Nadel, and Newcombe 2002). Similar questions arise concerning the neural substrates for processing of faces (Kanwisher 2000; Tarr and Gauthier 2000). Furthermore, there are considerable debates about the extent to which any modular structure that is found in mature adults is inborn or the product of experience (Pinker 1997; Thomas and Karmiloff-Smith 2002; Tooby and Cosmides 1992).

The purpose of this brief chapter is to consider two competing views about what modularity might consist of, which we will refer to as *sui generis* modularity (from the Latin "of its own kind") and *descent-with-modification* modularity. According to the former view, each cognitive (or neural) domain would be an entity entirely unto itself. A language module (or language acquisition device), for example, would owe nothing to other cognitive devices. Audition and vision might similarly be seen as wholly separate. According to the latter theory, which takes its name from Darwin's (1859) suggestion that "existing forms of life are the descendants by true generation of pre-existing forms," current cognitive (or neural) modules are to be understood as being the product of evolutionary changes from ancestral cognitive (or neural) modules.

17.1.1 Challenges to *sui generis* modularity

If modules were taken to be (say) encapsulated computational devices evolved for specific functions,³ it would be natural to think of them as being separate at every level. Cosmides and Tooby, for instance, once wrote that "There is no more reason to expect any two cognitive mechanisms to be alike than to expect the eye and the spleen, or the pancreas and the pituitary to be alike."

Nonetheless, although any two putatively distinct modules presumably must be functionally distinct in order to count as modules, they need not – and we will suggest should not – be presumed to be evolutionarily or genetically *sui generis*. In what follows, we will suggest that many arguments that seem to militate against modularity per se really argue only

against an extreme, *sui generis* version of modularity, of little force against a more nuanced version of modularity.

Why should one be skeptical about *sui generis* modularity? In the first instance, even in the most “modular” of non-cognitive biological systems (such as the heart), current systems owe much to evolutionary tinkering, in many instances sharing materials (e.g., proteins and cell types) and fabrication techniques (e.g., genetic cascades for establishing positional axes) with other biological systems – and there is no principled reason to expect this to be any different in the brain. Furthermore, in comparison to the period in which (say) eye and spleen have diverged, the time course over which many putative contemporary modules (e.g., for acquiring grammar or for intuitive psychology, the assessing of the beliefs and desires of other people) might plausibly have evolved is relatively short, a time span that perhaps points more to shared lineage than sheer *de novo* innovation.

In keeping with this, a number of empirical facts seems to challenge the extreme *sui generis* view. For example, whatever the neural substrates of proposed modules such as language and intuitive psychology may be, they are likely to emerge from relatively similar bits of six-layered neocortex (Brodmann 1909). They may well occupy (partly) disjoint parts of brain tissue, but it seems likely that the neural tissue that they rely on have at least something in common. (See Kingsbury and Finlay 2001 for a related suggestion.)

Sui generis modularity faces further empirical challenge from the fact that there is considerable overlap in sets of genes that are expressed in different cortical areas (Liu, Dwyer, and O’Leary 2000). Distinct cortical areas are least quantitatively distinguishable, and may well vary both in the combinatorics of their gene expression and in their connectivity at the circuit level, but these differences appear to be superimposed upon a common background that is widely shared across the brain, and not consistent with a picture in which each neurocognitive module would be structured in ways that reflected only adaptive functional constraints that substantially differed from one module to the next.

Likewise, few genetic disorders have been demonstrably and uniquely associated with a single cognitive domain (Hill 2001; Kadesjö and Gillberg 2001; Kaplan *et al.* 1998; Karmiloff-Smith 1998), and few of the cognitive disorders that might be construed as domain-specific can be uniquely associated with a particular genetic anomaly.

Furthermore, it might be argued that there is not enough room in the genome to specify the detailed circuitry of each complex human cognitive domain (e.g., language, intentional understanding, etc.) independently (Bates 1999; Ehrlich 2000), especially given how small the genetic difference between humans and the nearest non-speaking cousins, chimpanzees (less than 2% of genetic material: Ebersberger *et al.* 2002; King and Wilson 1975).

A further challenge comes from neuroimaging results. Despite a tradition of neuroimaging that extends back to the early 1980s (Raichle 1998) it is difficult to uniquely identify particular neural substrates with particular cognitive tasks, and difficult, except in the lower levels of perception, to localize particular tasks to unique areas (Poeppel 1996; Uttal 2001). Complex tasks are often subserved by multiple areas (Cabeza and Nyberg 2000), and individual brain regions often participate in the analysis of more than one type of content. Complex computations might consist of novel configurations of lower-level (modular) structures, rather than wholly proprietary circuitry. Views of the neural substrates of language (discussed in more detail below), are increasingly moving towards views of language as the product of many different physically distinct subsystems, many of which may also contribute to non-linguistic aspects of cognition and motor control (Friederici and Kotz 2003; Lieberman 2002; Poeppel and Hickok 2004; Ullman 2001b).

While none of these facts is devastating to the *sui generis* view that the mind consists of a large number of modules each operating according to entirely different principles, each poses serious challenges.

17.1.2 Descent-with-modification modularity

Considerations like these might lead one to abandon modularity altogether, or to suggest that modules be seen only “as the end product of development rather than its starting point” (Elman *et al.* 1996).⁴ But abandoning modularity altogether would introduce its own set of problems. For instance, a view in which modularity is completely absent would face challenges in explaining why children excel at language at an age (Crain 1991) in which they are otherwise cognitively limited (e.g., well before they can pass classical theory of mind tasks, Baron-Cohen, Leslie, and Frith 1985), and also faces challenges in explaining observations about apparent constraints in neuroanatomy (e.g. the systematicity of Brodmann’s areas and the Felleman–Van Essen wiring diagram); more than that, an entirely non-modular view would seemingly suggest (with little *a priori* motivation) that the brain/mind is the only major physiological system with no internal structure.

Descent-with-modification offers a way out. Although modules are, by definition (Coltheart 1999; Fodor 1983) computationally distinct, they need not be *genetically* unrelated. Although evolutionary pressures can clearly lead distinct physiological structures to diverge and specialize, many putative modules – especially those that have evolved relatively recently (e.g., a language faculty) – might be expected to have diverged fairly recently from common origins.

Consider, by way of analogy, the hand and the foot, two functionally and physically distinct systems that have diverged relatively recently from a common ancestral appendage. The two are known to grow through

largely – though by no means entirely – overlapping sets of genes (Margulies, Kardia, and Innis 2001). This results from mechanisms such as “duplication and divergence” (Duboule and Wilkins 1998; Gerhart and Kirschner 1997; Marcus 2004b), by which evolution creates new systems by making modified copies of old systems, whether at the level of a single gene, or by the force of genetic cascade at the level of complex structures such as limbs or (it appears) brain areas (Kaas 1987; Krubitzer 2000). An informal way to think about this is that the hand and the foot are both products of generative systems for creating vertebrate appendages.

As another example, some of our earliest vertebrate ancestors had only a single type of photoreceptor pigment, which sufficed for monochromatic vision. Around 400 million years ago, early in the history of vertebrates, before the modern classes like mammals, birds, and amphibians emerged, the genetic recipe for that pigment was, by sheer chance, duplicated. When one copy of the randomly duplicated gene diverged (i.e. changed slightly through some process of mutation), a new kind of pigment developed, one that was sensitive to a different part of the light spectrum. With two types, it became possible (given some further machinery to interpret the output of the photoreceptors) to discriminate shorter wavelengths of light (like blue and purple) from longer wavelengths of light (like red and green). About 35 million years ago, one of our primate ancestors branched away from other mammals when there was a second gene duplication, this time of the genetic recipe for the long-wavelength (“red”) color pigment. A third duplication and divergence led to a third type of color pigment, and what is known as trichromatic vision (Bowmaker 1998).

Distinct neural modules, too, may be the product of divergent copies of ancestral cognitive systems. Such a notion, although vague, can help make sense of many of the biological considerations raised in the previous section. It leads, for instance, to the idea that distinct neuro-/cognitive modules could overlap considerably in their genetic substrates. If each module were built according to its own set of genes, it would be unexplained why there is so much overlap between genes in (say) frontal cortex and the genes in (say) occipital cortex. If we instead think of frontal cortex and occipital cortex as diverged variation on a common theme for neural computation, the large degree of genetic overlap no longer seems surprising.

17.2 Biological considerations

Descent-with-modification helps make sense of the considerable phylogenetic continuity that has been documented in recent years, in terms of comparative psychology (Cheney and Seyfarth 1990; Hauser 2000; Tomasello and Herrmann 2010), comparative neuroanatomy (Gannon *et al.* 1998), and comparative genomics (Ruvolo 2004; Waterston *et al.* 2002). Although the linguistic, social and cultural capacities of humans

differ enormously from chimpanzees (Kako 1999; Richerson and Boyd 2004; Terrace *et al.* 1980; Povinelli and Giambrone 2001; Tomasello and Call 1997), these differences complement a shared psychological (Diamond 1992; Tomasello and Herrmann 2010) and biological foundation (Carroll 2003; Gagneux and Varki 2001), suggesting an overall view of evolutionary innovation imposed upon a shared background.

Beyond comparative considerations, we should expect evidence for descent-with-modification even *within a single species*. For example, according to the descent-with-modification view, rather than expecting each cognitive system to be a separate black box, we might expect distinct cognitive (or neural) modules to be evolutionarily and genetically related.⁵

In this way, the descent view helps make sense of the curious conjunction of selective impairment and co-morbidity in both inherited and acquired neurocognitive disorders. On a *sui generis* view, co-morbidity is more or less an accident; on the descent view, it's an expected outcome. At the same time, the descent picture also predicts (correctly) that there should be rare cases of selective impairment, corresponding to impairments in mechanisms that have been modified relative to common ancestry.

Cellular level analyses lend additional support. As mentioned earlier, at the neural level, the neocortex (the area of the brain most strongly associated with higher level cognition) is – to a first approximation – largely similar across its entire extent. With few exceptions, the neocortex is essentially a thin (3–4 mm) six-layered sheet, with each layer having certain characteristic properties that seem largely shared; the 4th layer takes input from the thalamus, layers 5 and 6 send output to subcortical structures. In terms of structure, layer 1 is made up largely of thin white fibers, 5 and 6 largely of pyramidal cells, and so forth (Douglas and Martin 1998). The brain is filled with repeated motifs (Sporns, Tononi, and Edelman 2002) such as topographic maps (essentially sets of parallel neural wiring) that occur throughout the cortex (Kaas 1987; Kaas and Collins 2001). Repeated structural motifs and common cellular components point to modules as different configurations of common computational stock, rather than entirely independent devices with no common components. As Todd Preuss (2000) and others have argued, such broad similarities by no means establish complete uniformity – different cortical areas vary in quantitative factors such as thickness (Ramón y Cajal, DeFelipe, and Jones 1988) and may well have different functions and even different microcircuitry within – but they do anchor the point that there is considerable overlap in the developmental process. Different cortical areas are, it would appear, better seen as variants on a theme than as components generated *sui generis*, independently of the evolutionary and genetic history of their neighbors.

Consistent with this, the expression of genes across cortical areas is remarkably uniform. Contemporary research has identified a unique genetic marker for the limbic system (part of the paleocortex), and

several markers that are distributed in different proportion across the cortex (e.g., FGF8, distributed in greater proportion in the front of the cortex than the back, Fukuchi-Shimogori 2001), but the vast majority of genes, perhaps more than 99%, that are expressed in any given region of cortex are expressed throughout the cortex. There may well be subtle differences in the concentrations of the protein products of those genes, but such differences once again point to parametric variation, not to entirely new suites of genes with developmental programs that are *sui generis*.

One reaches the same conclusion looking a level higher, at the distribution of cell types. Estimates about the exact number of cell types in the brain are notoriously variable, ranging from thousands to hundreds of thousands (Anderson 1992), but to a first approximation there is again relatively little difference across the cortex. Upper bound estimates of the number of cell types in the brain rely on counting parametric variants as distinct – a neuron with a bushy dendrite is counted separately from a neuron with a sparser dendrite, and that difference is multiplied by the differences in neurotransmitters types (e.g., GABA versus glutamate), the differences in channel distributions (sodium versus potassium or both), and so forth. However one counts, the variation among neuronal cell types can be traced to common ancestry (Striedter 2005).

17.3 Language as case study

17.3.1 Language and cognitive processes

Consider language – the canonical putative module – and its relation to cognitive systems. At the cognitive level, at least five considerations suggest that there is overlap between the mechanisms and representations of language and the mechanisms and representations of other cognitive domains: the nature of memory and automaticity, the specificity of sequence learning and abstract hierarchical representations, and the nature of linguistic and non-linguistic representations of space and time.

17.3.1.1 Language and memory

Language, like any aspect of cognition, relies heavily on memory; the physical substrates for the memory that is implicated in language may or may not overlap with the physical substrates for memory in other content domains, but it is clear that there are many shared properties. For example, in contrast to the memory of modern digital computers which is typically addressed by location, human memory, whether for language or other domains, is addressed by content (Martin and McElree 2008; McElree, Foraker, and Dyer 2003). Likewise, there is now considerable evidence that memory in language may well be subject to the same kinds of influences as memory in other domains, including similarity-

based interference effects (Gordon *et al.* 2006; Gordon, Hendrick, and Levine 2002; Van Dyke 2007; Van Dyke and McElree 2006), and effects of representational complexity on retrieval (Hofmeister 2010). As one of us recently argued elsewhere (Marcus 2004a), language may rely on a human-specific organization for memory (i.e. a new *data structure*), but the underlying mechanisms for the encoding, storage, and retrieval of memory in language may overlap considerably with underlying mechanisms for the encoding, storage, and retrieval of memory in other domains that are shared across the vertebrate world. There is also evidence that language may draw on both procedural and declarative memory systems shared with other domains (Ullman 2004).

17.3.1.2 Automatization

Language may, like other domains, rely to an important extent on a process of gradual automatization through repeated practice that is akin to the automatization in other domains (Kawachi 2002). The acquisition of morphological rules – in adults at least – appears to obey classic learning curves characteristic of procedural skill acquisition in other, non-linguistic domains (Ferman *et al.* 2009).

17.3.1.3 Language and sequencing

Language is in no small part a problem of sequencing, and it is possible (though rarely considered within generative linguistics) that the machinery for sequencing linguistic material extends to or adapts other preexisting machinery for sequencing.

Recent imaging work suggests that subjects trained on an artificial grammar show overlapping (but not identical) activation for artificial grammar sequences and syntactic processing (Hoen *et al.* 2006; Petersson, Folia, and Hagoort 2010). Also suggestive of cross-domain overlap in mechanisms for sequencing, Christiansen *et al.* (2010) found that, after training on a visuo-spatial symbolic artificial grammar, agrammatic aphasics were significantly worse at classifying novel strings than age, education, and general fluid intelligence matched controls. Similarly, a tantalizing (albeit preliminary) training study suggests that training in non-cognitive sequencing tasks may to some extent facilitate recovery of syntactic processing in aphasics (Hoen *et al.* 2003),⁶ indicating some sort of common cognitive substrate between these two types of sequencing.

17.3.1.4 Representational underpinnings

Many of language's foundational representational resources – such as the ability to represent abstract variables, distinctions between kinds and individuals, and the capacity to represent structured, hierarchical relationships between elements (Marcus 2001) – play a pivotal role in a wide variety of cognitive domains, ranging from motor planning to music (Jackendoff 2009; Steedman 2002).

17.3.1.5 Representations of space and time

Cognitive mechanisms for spatial and temporal representation seem to run deeply through the structure of our linguistic system (Fauconnier and Turner 2002; Lakoff and Johnson 1980; Landau and Jackendoff 1993; Pinker 1989). Spatial and temporal relations are grammatically marked through prepositions, tense, and aspect, while metaphors for space and time pervade language. For example, spatial terms are often co-opted to describe temporal extent, as with the English word *long*, which was extended from a spatial to temporal description (Lakoff and Johnson 1980). Yet interestingly, spatial and temporal representations in language and cognition are not isomorphic. Tenses and aspects vary across languages, not all languages code the same spatial relations, and our ability to represent certain spatial relations does not seem to depend on the language (Munnich, Landau, and Dosher 2001). This suggests that language makes use of mechanisms that are related to – but not identical to – those used in the representation of space and time.

17.3.1.6 Representations of words

Words themselves may share representation resources with other domains. For example, according to the so-called embodiment hypothesis, the basic elements that underlie lexical representations might be reused sensory-motor representations (Meteyard *et al.* 2010; Zwaan 2003). Consistent with this view, several neuroimaging studies demonstrate overlap in the brain regions activated when processing certain words, and the brain regions activated during matched perceptual or motor tasks. For example, Kable, Lease-Spellmeyer, and Chatterjee (2002) demonstrated that processing action verbs (e.g., *kick*) but not object nouns (e.g., *leaf*) resulted in changes in activity in the human motion areas MT/MST, as well as surrounding areas of the temporal lobe, which some take as evidence that the semantics of action words incorporates the same neural circuits that allow us to represent action and motion in the external world. Although controversy remains (Bedny *et al.* 2008; Meteyard *et al.* 2010), it seems plausible that the representations of at least some words importantly interacts with, and perhaps in some way builds upon, motor and perceptual systems.

17.3.2 Language and its biological substrates

17.3.2.1 The neural substrates of language

If language builds on a variety of non-linguistic abilities, we would expect the neural substrates of language to overlap considerably with other non-linguistic tasks, and this appears to be the case (Blumstein 2009). Of course, to some degree this is trivially true: It would be shocking if a domain-general area like auditory cortex did not subserve aspects of spoken

language comprehension. More interestingly, however, higher-level linguistic processes such as syntactic and semantic comprehension also seem to depend on brain regions that are not specifically tied to language. For instance, recent work indicates that Broca's area, which was long thought to be the seat of syntactic processing, may in fact contribute to our ability to select amongst competing alternative representations (such as competing syntactic structures or word meanings (Novick, Trueswell, and Thompson-Schill 2005; Thompson-Schill 2003). Other work suggests that it contributes similarly to memory retrieval (Badre and Wagner 2007).

For semantics, the case for shared cognitive resources is possibly even stronger. In a number of papers, Pylkkänen and colleagues have provided evidence that compositional and extra-compositional semantic processes occur in the ventromedial prefrontal cortex (Bemis and Pylkkänen 2011; Pylkkänen and McElree 2007), which is not part of the traditional "language" network of brain areas. Instead, this region is associated with high-level cognitive tasks such as decision making, reward-based learning and, in particular, social cognition (Amodio and Frith 2006). The diverse nature of these tasks makes it difficult to suggest a single common function for this region; one possibility is that each is a separately modified descendant of some single common ancestor function.

17.3.2.2 Comorbidity between language and cognition in developmental disorders

Although there has been a handful of reported cases of individuals with impaired language but intact cognition (van der Lely, Rosen, and McClelland 1998) and cases of individuals with (nearly) normal language but significantly impaired cognition (Bellugi *et al.* 1990; Bellugi *et al.* 2000; Johnson and Carey 1998), overall, proposed dissociated cases are exceptions rather than the rule. Developmental disorders with selective effects on language or general cognitive functioning are rare in the population as a whole relative to disorders that impair both domains: while the rate of Williams syndrome (impaired cognition but "preserved" language) is roughly one per 20,000 births (Morris and Mervis 2000), Down syndrome (both impaired language and cognition), for example, is present in one out of 800 births (Nadel 1999).

Further, even within the classically cited exemplars of dissociated disorders, modern work suggests that, often, true selectivity of impairment may not hold up under a finer lens of scrutiny. While it traditionally has been claimed that individuals with Williams Syndrome (WS) have preserved language abilities despite significantly impaired general cognitive skills, a recent meta-analysis suggests that the verbal abilities of people with Williams Syndrome are relatively well predicted by their overall cognitive level of functioning (Brock 2007). On the other end of the spectrum, Specific Language Impairment (SLI), despite its inherently selective behavioral definition of language difficulty combined with normal-range

non-verbal IQ, in fact often shows covariation with more subtle general impairments such as memory and motor control (Hill 2001), or aspects of basic auditory processing (Hill, Hogben, and Bishop, 2005). Contra to the known genetic anomaly associated with WS, SLI – while highly heritable – is still mainly a catch-all label for a purely behavioral profile; SLI is thus a notably heterogeneous disorder, and as such the specific causes are also likely to be various. Although more domain-general deficits may not be able to account for all the profiles of SLI (van der Lely 2005), theories attribute many of the profiles of linguistic difficulty found in SLI to general resources such as procedural memory and temporal processing (Tallal 2003; Ullman and Pierpont 2005).

As selective impairments for language attributable to specific genetic loci go, the speech and language disorder found in the British KE family (Gopnik and Crago 1991; Vargha-Khadem *et al.* 1998) arguably comes closest to a selective impairment in language, since recent evidence directly ties that disorder to a particular gene, FOXP2 (Fisher *et al.* 1998; Lai *et al.* 2001) but the particular disorder in that family also affects non-linguistic aspects of orofacial motor control (for a review, see Marcus and Fisher 2003).

It is worth noting that the comorbidity suggested by these developmental disorders can be seen as extending to variation in the normally developed population as well; while variance in performance of language tasks (such as reading comprehension or complex syntax and lexical item production) is usually most closely related to other language tasks, there is considerable covariation between language and performance in other cognitive domains (for a review of early work in this domain, see J. Carroll 1993; see also Rabagliati and Salthouse 2011). The pattern that emerges from these patterns of comorbidity – relatively subtle and specific areas of potential dissociation couched within largely co-varying deficits between aspects of language and cognition – would pose significant challenges to a strict modularity viewpoint, but is entirely consistent with descent-with-modification.

17.3.3 Summary

None of this is to say that language is *just* a product of preexisting mechanisms, or that language simply is cognition. Just as wings build upon a common plan for vertebrate limbs by adding feathers and adjusting bone and muscle densities, the human linguistic system presumably is *more* than just a preexisting ancestral neurocognitive system. To whatever cognitive machinery may have already been in place, evolution may have added specialization of the vocal tract (Lieberman 1984), an ability to represent hierarchical or recursive structure (Hauser, Chomsky, and Fitch 2002; Marcus 2001, 2004b), a capacity to freely acquire new words (Jackendoff 2002), and/or an interface between systems for

communication and theory of mind (Bloom 2000); understanding such evolutionarily recent modifications is crucial to any complete account of the human capacity for language. At the same time, as important as such new capacities are, the considerations above suggest that language does indeed borrow – sometimes with important modification, sometimes without – cognitive machinery inherited from our non-speaking primate ancestors.

17.4 Modules as the product of descent-with-modification: Implications

Construing contemporary neurocognitive modules as descendants of processes of modification, rather than as *sui generis* devices shaped purely by adaptive advantage (without serious regard for developmental constraints), has a number of implications for the disciplines of cognitive science, including how we assess debates about modularity, and how we investigate putatively modular domains.

17.4.1 Implications for how we identify modular structure

The notion of descent-with-modification, once recognized, has significant implications for how we assess debates about modularity. Although the question of whether any particular computation is performed in modular fashion must inevitably remain an empirical one, ideas such as duplication and divergence undermine many typical arguments against modularity with particular domains.

Consider, for example, developmental disorders that affect multiple cognitive domains. To some opponents of modularity, mental disorders that affect multiple domains are *prima facie* evidence that the mind is without modules. If some disorder affects both language and general intelligence, many assume that language is simply the product of general intelligence, not an independent entity. But a finding that a given disorder affects two behaviors or neural structures doesn't mean that the two are identical – it could just mean that they are built in a similar way. If 95% of the genes involved in the circuitry for building language also participate in the constructing of other mental capacities, the vast majority of genetically originating disorders should be expected to have broad effects. Impairments to memory, for example, ought to impair language, as well as other domains like planning and decision-making. Disruptions to the genes that code for metabolic enzymes might be expected to affect (in some cases) the entire brain, and disruptions to genes that code for receptor proteins might have effects wherever those receptors are found. It would be a rare disorder indeed that would affect only a single aspect of cognition.

To take a concrete example, dyslexics often have a deficit not just in phonology but also in sensory-motor skills, and one might be tempted to doubt that dyslexia is a modular deficit. But the existence (and apparent frequency) of processes such as duplication and divergence make it possible that (say) two physically distinct systems could be genetically related.⁷ A finding that many dyslexics have sensory-motor difficulties doesn't mean that a single neural anomaly underlies both disorders; instead a single gene might impair some shared developmental process. Similarly, the fact that some people with language impairments are also cognitively impaired doesn't necessarily mean that language in such people is impaired *because* cognition is impaired; instead, both language and other aspects of cognition (say executive function) could be impaired by a mutation in genes that participate in the development of both. When a given disorder affects two aspects of cognition, we can't necessarily infer that the two aspects of cognition rely on a common neural or cognitive substrate.

The descent-with-modification perspective suggests caution for inferring the absence of modularity from many studies of "normal" cognition. In particular, it is often assumed that if two systems have shared properties, e.g., common learning curves, they necessarily depend on physically overlapping substrates. For example, Markson and Bloom (1997) argue against domain-specificity for word learning on the grounds that one of word-learning's hallmark traits – rapidity of acquisition – applies in other cognitive domains (e.g., associating an object with information about where it came from). A descent-with-modification view highlights the fact that such results are equally consistent with the possibility that there is a specialized architecture for word-learning that has unique physical substrates that share a common genetic heritage with other neurocognitive systems. (E.g., there could be different memory circuits involved in remembering words and "ordinary facts," but those distinct circuits might be built by common substrate.)⁸ Once the logic of descent-with-modification is recognized, it becomes clear that parceling cognitive processes into underlying components is significantly more difficult than widely anticipated.

Because of the richness of gene regulation (again, see Marcus 2004b, for a non-technical introduction), a single gene may be used multiple times in the service of radically different functions, in each copy of a serially repeated structure (like a finger or a toe), or even in far more diverse circumstances, such as the protein product of the gene *Microphthalmia-associated transcription factor* (MITF), which participates in eye formation, blood cell formation, and pigmentation (Gilbert 2000). If a single brain region is heavily involved in two processes, it is commonly assumed that the two domains involve some of the same computations.⁹ In contrast, a single gene that is involved in two different processes does not necessarily show that the same cognitive functions are produced by the same bit of

neural structure or rely on the same computational mechanisms. Apparently general processes could result from common sets of genes that contribute to multiple physically distinct substrates.

17.4.2 Implications for the study of linguistics

If, as argued here, modules that seem superficially different may share genes and ancestry, descent-with-modification also supplies a novel argument for interdisciplinary collaboration. If two current cognitive mechanisms are, in essence, diverged copies of some single ancestral system, it follows that we may glean insight into both systems by careful comparisons of the two. For example, one might expect that the recently evolved capacity for language would draw heavily on general cognitive (or motor) resources that have been inherited from our ancestors, and hence it stands to reason that we ought to be able to gain insight into language by studying those cognitive resources.

Descent-with-modification suggests that we should expect the hallmarks of ancestry even in the very machinery that makes abstract linguistic representation possible. Although the abstract computational system that allows humans to represent and process language is unique to humans, the circuitry that gives rise to that abstract computational system need not itself be entirely unique. It is possible – maybe even likely – that there is evolutionary precedent for some of the components underlying the abstract computational system.¹⁰ In light of Jacob's (1977) view of evolution as a tinkerer, it is even possible that there are *no* uniquely human components, just uniquely configured components.

If the abstract computational mechanism for language incorporates not just memory and recursion, but also mechanisms for (say) categorization or automating repeating actions, one is led to a whole family of similar empirical questions: to what extent are the mechanisms that support linguistic representation (a) the very same mechanisms used in other cognitive computations (b) divergent copies of ancestral mechanisms that also gave rise to current cognitive mechanisms, or (c) *sui generis* mechanisms with no informative ancestral history. Neuroimaging results that show significant contributions of subcortical regions (e.g., Crosson 1992; Lieberman 2002) and frontal areas not traditionally associated with language (e.g., Poeppel and Hickok 2004) seem to point towards the former two possibilities, and against the latter.

17.5 Afterword

Demonstrating that descent-with-modification modularity is not vulnerable to some of the biologically oriented criticisms that could be leveled against *sui generis* modularity is not the same as showing that the mind (or

brain) consists of modules that descended with modification. Even if the mind (and brain) descended with modification from earlier neural and cognitive architecture, it is still an empirical question whether there is any modular structure at all. Descent-with-modification suggests that there should be shared properties between modules, but if there are no modules at all, there would still be shared properties between underlying neural substrates; at best the current arguments put descent-with-modification modularity on a par with a domain-general view; they don't independently affirm it. Affirming modularity (of any sort) in any domain remains an empirical challenge.

Moreover, descent-with-modification is no panacea. It may answer some puzzles, but there are many more it cannot answer. To the extent that evolution has through descent-with-modification created the entire array of living organisms on the planet earth, from bacteria to worms, spiders, and human beings, descent-with-modification has not constrained biology to any single narrow niche. It is not obvious that our system for communication (say) is the only way that a linguistic system could have evolved, nor that evolution requires all intelligent life forms to reason about things in the same way. To the extent that descent-with-modification provides constraint, it is not by strictly limiting *outcome*, but by limiting process: the question is always where one could go from here (or get here in the first place), not about where one could get starting from scratch. As such, the more we know about ancestral systems the more we will be able to put the notion of descent-with-modification to useful work.

18

The role of Broca's area in language function

Gregory Hickok

18.1 Where is Broca's area?

Broca's area has been a centerpiece of functional anatomic models of language since it was first implicated in speech production by Pierre Paul Broca in the 1860s. Although there is little doubt that the region plays some role in speech production as Broca proposed, it remains unclear exactly what that role is. Further, Broca's region has also been implicated in a dizzying array of additional language and related functions, ranging from various forms of syntactic comprehension to speech perception, cognitive control, action understanding, and music perception. Here we review the primary hypotheses regarding Broca's area function, organized in terms of the historical timeline of the emergence of these ideas.

Broca's area is typically defined as the posterior portion of the left inferior frontal gyrus (IFG) including the pars triangularis and pars opercularis ([Figure 18.1](#)) (Brodmann 1909; Aboitiz and Garcia 1997; Anwander *et al.* 2007). The pars triangularis and opercularis roughly correspond to Brodmann areas 45 and 44, respectively, but the boundaries of BA44 and BA45 can vary considerably (Keller *et al.* 2009; Amunts *et al.* 1999). A third anatomical region, referred to as the frontal operculum, is sometimes noted in discussions of the function of Broca's area (Friederici *et al.* 2000a). The frontal operculum has been described as the inferior-medial portion of the IFG, extending medially to the insular cortex and posteriorly to the pre-central sulcus (Anwander *et al.* 2007).

As noted above, Broca's area is not an anatomically homogeneous region and therefore it is important not to assume that the region as a whole will be functionally homogeneous. Some of the confusion over the role of Broca's area in various functions probably stems from the use of the term "Broca's area" to refer broadly to this heterogeneous region. Progress in understanding the functional organization of Broca's area

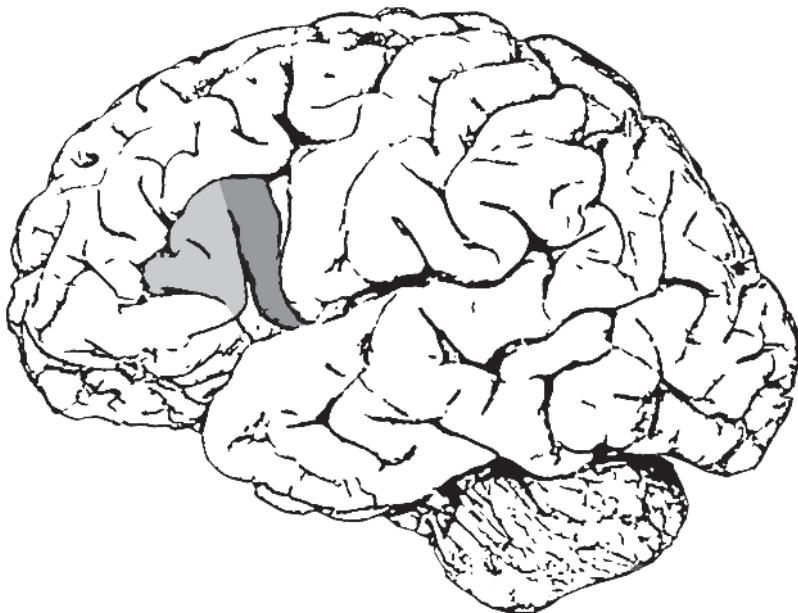


Figure 18.1 Broca's area is traditionally defined as the posterior portion of the inferior frontal gyrus of the left hemisphere, including the pars triangularis (light grey) and pars opercularis (dark grey).

will be facilitated if investigators couch their hypotheses and findings in a higher-resolution anatomical framework. In the present discussion I will try to follow this suggestion where possible.

18.2 Broca's area and speech articulation

Broca's area has long been implicated in articulatory-related speech production and for good reason. Damage to, or disruption of Broca's area has been associated with articulatory/phonetic deficits in speech production. For example, acute damage to Broca's area can cause severe speech articulation deficits and direct cortical stimulation can cause speech arrest. However, the role that region plays in speech production is less clear.

One fact that needs to be considered in this context is that destruction of Broca's area does not lead to chronic, severe speech production deficits (Mohr *et al.* 1978). Severe chronic production deficits, such as those characteristic of Broca's aphasia, require much larger lesions, typically including much of the posterior-lateral frontal lobe and anterior insula (Damasio 1991; Dronkers and Baldo 2009; Hillis 2007). When a lesion is restricted to Broca's area, recovery of motor speech function is near, if not fully complete. For example, (Alexander *et al.* 1990) report that patients with focal lesions that are largely restricted to Broca's area have excellent

recovery of their speech production ability and are left with few language deficits. These findings suggest that Broca's area, while clearly involved in speech production, is not critical for relatively normal speech articulation ability. This observation will have to be taken into account in any model of the role of Broca's area in speech production.

18.3 Broca's area and syntactic computation

The first hints that Broca's area might play a role in syntactic processes came from observations of the speech output of patients with Broca's aphasia. Agrammatism, the tendency to omit grammatical information in speech output, was described as early as 1819 by Deleuze (see Goodglass 1993: 103–104) and received detailed treatment by Arnold Pick in a 1913 volume dedicated to the topic (Pick 1913; Whitaker 1998). But the first half of the twentieth century was characterized by a notable lack of interest in localization of cortical function and so the link between Broca's area and syntactic processes would have to wait until the 1960s and 1970s when localization of function became a topic of intense interest again, largely due to the influence of Norman Geschwind (Geschwind 1965). By then, cognitive (as opposed to behaviorist) approaches dominated psychology, linguistics was making significant headway in understanding the nature of language structure, and technological advances allowed scientists to map the lesion correlates of neurological disorders *in vivo*. These factors led to an explosion of new research on aphasia and on syntactic processing in particular.

A pivotal study in research on the relation between Broca's area and syntactic processes, and one that continues to shape the direction of current research, is the 1976 report by Caramazza and Zurif. Caramazza and Zurif found that Broca's aphasics exhibit not only agrammatic production, but also had a profound deficit in using syntactic knowledge in sentence comprehension. The critical bit of evidence was that Broca's aphasics were able to use semantic information (lexical and real-world knowledge) to derive the meaning of an utterance: they could correctly understand a so-called semantically non-reversible sentence like, *The apple that the boy is eating is red*. But they failed when interpretation required the use of syntactic information, i.e. when the sentence was semantically reversible like, *The boy that the girl is chasing is tall*. This finding suggested that Broca's aphasics had a deficit in syntactic computation generally which had ramifications both in their speech output and sentence comprehension. Broca's area, via its association with Broca's aphasia (a dubious association, see above) then became the major anatomical focus for the localization of syntax, including its role in comprehension of sentences. This obsession with Broca's area and syntax (comprehension in particular) persists today.

It is interesting to note that Caramazza and Zurif tested not only Broca's aphasics, but also conduction aphasics. Conduction aphasics have *posterior* lesions, centered on the temporal-parietal junction (Baldo *et al.* 2008; Buchsbaum *et al.* 2011), and don't have agrammatic speech output. However, Caramazza and Zurif report that their conduction aphasics performed exactly like the Broca's aphasics: both were approximately 90% accurate in comprehending semantically non-reversible sentences, but performed at chance on semantically reversible sentences.

Why then did all subsequent work focus on Broca's aphasics and Broca's area? Why was conduction aphasia and more posterior lesion sites not considered as a possible test case/source for the neural substrate for syntax?

The answer derives from the common interpretation of conduction aphasia at the time, which was that of a disconnection syndrome (Geschwind 1965). According to this view, conduction aphasia was caused not by damage to any computational system, but by a disconnection of computational systems, namely Wernicke's and Broca's area. Caramazza and Zurif write,

Conduction aphasics also were incapable of using syntactic algorithmic processes ... The question arises, therefore, as to whether syntactic operations also rely on cortical regions posterior to Broca's area or whether the conduction deficit should be considered within a disconnection framework, that is, as the severing of a connection to Broca's area ... Given the impressive arguments offered by Geschwind, we are presently satisfied in treating it as a problem of disconnection, but a disconnection from an area that subserves syntactic processes. (Caramazza and Zurif 1976: 581)

But the interpretation of conduction aphasia has evolved since the 1970s. It is no longer considered a disconnection syndrome but rather a deficit caused by cortical dysfunction (Anderson *et al.* 1999; Baldo *et al.* 2008; Fridriksson *et al.* 2010; Hickok *et al.* 2000). Thus, the original motivating factor for focusing on Broca's area no longer holds and a re-examination of the source of the deficit in light of a broader perspective, i.e. one that includes conduction aphasia and posterior cortices, is needed. Once such perspective is outlined below.

Caramazza and Zurif's report caused much excitement over the possibility that the neural correlate of syntax had been identified. However, subsequent work by another group squelched the enthusiasm. Linebarger, Schwartz, and Saffran (1983) tested a group of agrammatic Broca's aphasics on their ability to make grammaticality judgments. If Broca's aphasics indeed had lost their syntactic competence, then the ability to judge the syntactic acceptability of utterances should suffer appreciably. This is not what Linebarger *et al.* found. Instead, they found that agrammatic Broca's aphasics were able to judge grammaticality remarkably well. The idea that Broca's aphasia reflected a global deficit in syntax was replaced by notions

of failures to link syntax and semantics (Linebarger *et al.* 1983) or various online processing deficits (Hagoort 1993; Swinney *et al.* 1989).

But the idea of a link between Broca's aphasia/area and syntax was not completely abandoned. In a series of publications, Grodzinsky articulated a theory in which only a circumscribed component of the syntactic system was compromised in Broca's aphasia, namely that component that links displaced elements with their place of interpretation (Grodzinsky 1986, 1989, 1990). For example, in the "object gap" sentence, *The boy that the girl chased __ is tall*, the noun phrase, *the boy*, is interpreted as the object of the verb *chased*, whereas in the "subject gap" sentence, *The boy that __ chased the girl is tall*, the noun phrase, *the boy* is interpreted as the subject of the verb. In order to interpret these sentences, the syntactic system must correctly link the initial noun phrase with the role it plays relative to the verb. Grodzinsky argued that Broca's aphasics specifically lack the component of syntax that computes this relation. While this syntactic link is thought to be required in both subject and object gap sentences, it has been found that Broca's aphasics only have trouble with the object gap variant. Grodzinsky explains this observation by assuming that Broca's aphasics, in the absence of syntactic linkage information, adopt a "default" comprehension strategy by which they interpret the first noun phrase as the subject. This strategy works fine for subject gaps which follow canonical subject-verb-object word order (in English), but fails for object gaps which follow a non-canonical word order, thus explaining the observed pattern of performance. By (dubious) association between Broca's aphasia and Broca's area, he has also claimed that Broca's area is the seat of this computation (Grodzinsky 2000; Grodzinsky and Santi 2008).

Functional imaging research seems to provide *prima facie* support for Grodzinsky's claim. For example, several functional imaging studies have found that in healthy subjects, portions of Broca's area are more active during the comprehension of object gap sentences compared to subject gap sentences (Cooke *et al.* 2002; Fiebach *et al.* 2005; Rogalsky *et al.* 2008; Santi and Grodzinsky 2007a, b). But on further reflection, it is not clear that Grodzinsky's gap-based theory would necessarily predict this result as both subject and object gap sentences involve a linking computation. This has led some investigators to propose that it is working memory load that may drive the involvement of Broca's area in the comprehension of syntactically complex sentences such as object gap constructions (Just *et al.* 1996; Kaan and Swaab 2002; Rogalsky *et al.* 2008).

A recent functional imaging study (Rogalsky *et al.* 2008) used an articulatory suppression paradigm to assess the role of one form of working memory, phonological short-term memory, in the pattern of activity in Broca's area during sentence comprehension. Articulatory suppression (repeatedly articulating a word or sequence of syllables) interferes with phonological short-term memory by preventing the use of articulatory rehearsal (Baddeley *et al.* 1975). In a functional imaging context, one can

use articulatory suppression to assess the extent to which sentence processing effects in Broca's area reflect articulatory rehearsal: if the object gap sentences yield greater activation in Broca's area than the subject gap sentences even during articulatory suppression, then the residual activation can be attributed to mechanisms other than articulatory rehearsal. In other words, this approach allows one to answer the question, how much of the object gap > subject gap activation pattern that is typically found in Broca's area (when articulatory suppression is not employed) can be accounted for in terms of simple articulatory processes underlying phonological working memory? Subject versus object gap sentences were presented auditorily both with and without articulatory suppression. Without suppression, object gap sentences produced more activation in several Broca's area regions including the pars opercularis and pars triangularis. However, during articulatory suppression only the pars triangularis showed the object > subject effect. A similar result was reported for visually presented sentences by Caplan *et al.* (2000). This finding suggests that the pars opercularis portion of Broca's area contributes to sentence comprehension only via its role in the articulatory rehearsal component of phonological working memory. It is interesting and relevant that the pars opercularis appears to be functionally linked to the temporal-parietal junction (a region known as Spt in particular) (Buchsbaum *et al.* 2001; Buchsbaum *et al.* 2005; Rogalsky and Hickok 2011) that has been implicated in phonological short-term memory (Buchsbaum and D'Esposito 2008; Hickok *et al.* 2003; Hickok *et al.* 2009b) as well as linked to conduction aphasics (Buchsbaum *et al.* 2011), who have been found to have phonological short-term memory deficits (Baldo and Dronkers 2006). This constellation of observations ties together the sentence comprehension performance of Broca's and conduction aphasics as reported by Caramazza and Zurif, among others, with a phonological short-term memory deficit involving a fronto-temporal-parietal circuit. Damage to this circuit appears to account for at least some of the functional activation in healthy subjects and sentence comprehension patterns in aphasics.

But perhaps this isn't the whole story. The pars triangularis, for example, persists in showing a subject-object asymmetry even during articulatory suppression (Caplan *et al.* 2000; Rogalsky *et al.* 2008). Might this be the region supporting a specific syntactic function? It's possible, but there is evidence against this view. One piece of the evidence is that the subject-object asymmetry found in the pars triangularis (even during articulatory suppression) disappears when subjects perform a concurrent finger-tapping task, which was used as a non-speech control for the effects of performing a secondary motor task during sentence comprehension (Rogalsky *et al.* 2008). Other evidence suggests that more anterior sectors of Broca's area support sentence comprehension via some more general cognitive function (Novick *et al.* 2005; Rogalsky and Hickok 2011). We will take up this possibility in the next section.

It is notable that some authors have recently proposed that Broca's area (posterior sectors in particular) is involved in hierarchical processing generally and therefore supports sentence processes via this more general computational function (Fadiga *et al.* 2009; Friederici 2009; Friederici *et al.* 2006a). This is a puzzling claim in light of the lesion evidence noted above that patients with Broca's aphasia – who therefore have lesions that likely include all of Broca's area and surrounding tissue – can judge the grammaticality of sentences quite well (Linebarger *et al.* 1983). In other words, the same reasons that led the field to reject the idea that Broca's aphasics had a global deficit in syntactic processing should apply to these newer claims regarding a supposed role for Broca's area in an even more general hierarchical processing mechanism.

In sum, although there has been much discussion of the role of Broca's area in syntactic computation, especially during comprehension, there is little evidence supporting the claim for any linguistic-specific computation performed in Broca's region (Rogalsky and Hickok 2011).

18.4 Broca's area and cognitive control

It has been suggested that Broca's area supports some aspects of sentence processing via a more general role in "cognitive control," which includes mechanisms involved in resolving conflicts between competing representations or conflicting information (Novick *et al.* 2005). The necessity for conflict resolution arises in a range of language processing situations where ambiguity exists including phonological, lexical, and sentential contexts. Novick *et al.* propose that Broca's area and surrounding tissue support the detection of conflicting analyses and the implementation of re-analyses when needed. Support for this hypothesis comes from the observation that classic cognitive control tasks, such as the Stroop task, have been found to activate regions in the vicinity of Broca's area (see Novick *et al.* for a review), as well as from one study that reported more activity in Broca's region for ambiguous sentences that required reanalysis (Mason *et al.* 2003; Stowe *et al.* 1998) and another (Fiebach *et al.* 2004) that found more activation in the pars opercularis region for sentences that were disambiguated later in the sentence compared to earlier in the sentence (late disambiguation is argued to induce more reanalysis load). Although Novick *et al.* primarily discuss garden-path sentences, one might extend this framework to cases of syntactic movement as these sentences also induce local ambiguities in determining where the displaced item should be interpreted. Additional studies have reported that sentences containing lexically ambiguous words yielded more activation in the pars triangularis than comparable sentences that had fewer word-level ambiguities (Rodd *et al.* 2005) and that this region was more active for sentences containing real world knowledge violations (*Bananas are red and*

delicious) compared to those that did not (Hagoort *et al.* 2004). Such findings might also be construed as evidence for a cognitive control interpretation of the function of Broca's area because both ambiguities and violations of real-world expectations can lead to conflicts that must be resolved.

An examination of the distribution of activation foci for classic cognitive control tasks shows that they cluster in two distinct regions, one slightly dorsal to the pars opercularis (Derrfuss *et al.* 2004) and the other in more anterior portions of Broca's area (Rogalsky and Hickok 2011). The more anterior cluster is in the vicinity of the pars triangularis region that showed a subject-object asymmetry even under conditions of articulatory suppression.

The cognitive control proposal is relatively new and has not been investigated thoroughly across multiple domains and with multiple methods (hemodynamic imaging, lesion). We will therefore have to wait for additional investigation to determine whether it fares any better than the syntax-oriented proposals.

18.5 Broca's area, mirror neurons, and speech perception

The discovery of mirror neurons in the macaque frontal cortex (di Pellegrino *et al.* 1992; Gallese *et al.* 1996; Rizzolatti and Arbib 1998) has sparked renewed interest in the role of the motor system in the perception of speech sounds (D'Ausilio *et al.* 2009; Meister *et al.* 2007; Wilson *et al.* 2004), an idea that is clearly related to the motor theory of speech perception (Liberman *et al.* 1967; Liberman and Mattingly 1985).

The question of the role of the motor system in speech perception has the benefit of decades of research driven by Liberman and colleagues' motor theory of speech perception, which has been evaluated from a range of perspectives including neuropsychology, development, and animal research. The outcome of all this work was that a strong version of the motor theory was rejected as a viable theory by a large majority of speech perception researchers. Some of the most convincing evidence that led to the rejection of the strong view of the motor theory came from studies showing that speech perception could be achieved without motor speech ability. Neither damage to the motor cortex related to speech as in Broca's aphasia (Damasio 1992; Goodglass 1993) nor cases of congenital failure to develop speech production prevent normal receptive speech ability (Lenneberg 1962). Developmentally it was shown that infants could discriminate speech sounds that they could not yet produce (Eimas *et al.* 1971). And research on animals indicated that sophisticated speech perception ability could be achieved even in a model system (e.g., chinchilla) that does not have the capacity to produce any speech (Kuhl and Miller 1975). More recent evidence supports the earlier findings. For example, it has been shown that acute and complete deactivation of left hemisphere

motor regions during the administration of Wada procedures does not preclude impressive speech discrimination performance requiring the resolution of fine phonetic distinctions (Boatman *et al.* 1998; Hickok *et al.* 2008). The fact that motor speech is not necessary for speech perception places a significant constraint on motor-based theories of receptive speech processing and falsifies any claim that the motor system is required for speech perception generally.

At the same time, any theory of speech perception must take account of recent observations that motor systems can affect speech perception to some degree. For example, Galantucci *et al.* review a range of behavioral studies indicating motor influence on speech perception (Galantucci *et al.* 2006), and studies involving the application of transcranial magnetic stimulation (TMS) to motor areas has shown mild changes in performance in the perception of speech presented in noise (D'Ausilio *et al.* 2009; Meister *et al.* 2007) indicating some role for the motor system at least under some conditions (see Hickok *et al.* 2009a; Wilson 2009 for discussion). Findings such as these place constraints on auditory-based theories of speech perception and indicate that there must be a mechanism for the interaction of sensory and motor systems during perception of speech.

An architecture that is fundamentally auditory but which allows for motor modulation is most consistent with the facts (Hickok *et al.* 2011). Specifically, motor effects on perception tend to be small (~10% modulation), have been demonstrated only in partially ambiguous acoustic stimuli (speech in noise), and at least a portion of this effect may be attributable to post-perceptual factors rather than perceptual discrimination (Sato *et al.* 2011). These facts coupled with the observation that speech perception is at or near ceiling levels even with the motor system severely compromised, is evidence for an asymmetry in computational importance between the auditory (driving) and motor (modulatory) speech systems.

If the motor system has only a small modulatory effect, why is it involved in perception at all? The answer, we suggest, is that the perceptual modulation developed to support *motor* function (Hickok *et al.* 2011). Specifically, motor commands generate a corollary discharge which generates an internal forward model predicting the sensory consequences of motor commands. The sensory prediction is realized as an auditory gain modulation (prediction is much like attention). Given the existence of such a mechanism in the motor control system, perhaps it can be co-opted (exapted, in the evolutionary sense) for perception of others' speech under some circumstances.

Lexical retrieval and its breakdown in aphasia and developmental language impairment

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19.1 Introduction

One of the central processes in language is lexical retrieval, the process of getting from a concept to a spoken word. This process became one of the central targets for researchers of biolinguistics, and specifically, for brain research. As we will survey below, lexical retrieval is a multi-staged process, with distinct components. The main challenge of biolinguists in this area is the identification of these components, of their localization in specific brain areas, and the description of the flow of information between them over the course of time. This is done mainly via the exploration of various types of anomia, lexical retrieval impairments after brain damage, and using various brain imaging techniques.

Psycholinguists and biolinguists seek to explain the process that allows speakers to retrieve words rapidly and accurately. In fluent speech, words are retrieved at a rate of 1–3 words per second, from lexicons that include tens of thousands of words, and still, normal speakers make as few as one error in a thousand words or even less (Butterworth, 1989, 1992; Levelt, 1989). Models of lexical retrieval, developed by a group of neuropsychologists of language and cognitive psycholinguists (e.g., John Morton, John Marshall, Merrill Garrett, David Howard, Brian Butterworth, Gary Dell, Willem Levelt, Ardi Roelofs, Max Coltheart, and Lyndsey Nickels), describe the intact word production process and the possible loci of deficits in the model that cause word retrieval difficulties (see [Figure 19.1](#)).

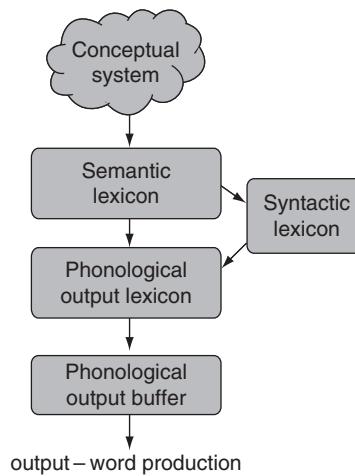


Figure 19.1 Stages in lexical retrieval.

19.2 Stages of lexical retrieval

These models suggest that the first stage of the lexical retrieval process is the formation of a conceptual representation in the *conceptual system*, a representation that is still not formulated in words. This representation includes what the person knows about a concept, probably including its visual image, its semantic properties, its function etc. Such a concept can be created from an idea someone has, or after identifying a visual image such as a picture of an object or the object itself. Many cognitive psychologists and linguists (Collins and Quillian 1969; Rips and Medin 2005; Rosch 1975, 1978; Rosch *et al.* 1976; Rosch and Mervis 1975; Smith and Medin 1981, to name just a few), as well as philosophers (such as Gottlob Frege, Bertrand Russell, Ludwig Wittgenstein, and many others), explored the questions of the nature and representation of concepts, and this question is still under research and debate.

This non-lexical concept then activates a lexical-semantic representation in the *semantic lexicon*. The semantic lexicon is organized semantically and contains words and information about the meaning of words, for example, about the semantic category, function, color, and size of the items the words refer to. According to some conceptualizations, it is actually a “hub” between the conceptual system and the lexical systems, without a content of its own. This lexicon is where semantic priming, the effect of the presentation of a word on the later presentation of a semantically related word, takes place. Semantic priming has a complex pattern – for example, whereas hearing or reading a word facilitates the access to a semantically related word that appears shortly afterwards, studies of word production found that in naming tasks it takes longer to produce a word if a semantically related word was produced shortly beforehand (Howard

et al. 2006; Wheeldon and Monsell 1994). The semantic lexicon is organized also by imageability, with high-imageability (concrete) words being easier to access than low-imageability (abstract) words (Howard and Gatehouse 2006; Nickels 1995; Nickels and Howard 1994).

The selected semantic representation activates the lexical-phonological representation in the *phonological output lexicon*: the representation of the spoken form of the word, which includes the metrical information of the word (number of syllables and stress pattern) and its segmental information (its phonemes – consonants and vowels, and their relative positions, Butterworth 1992; Levelt 1992). A possible reason for the phonological output lexicon to keep the words broken into metric and segmental information, rather than as already-assembled words, is that words may have different phonological forms in different contexts. For example, the words “want” and “to” together do not sound like the juxtaposition of the two separate words, but are rather pronounced “wanna”; the same goes for “come” and “on,” that are pronounced together as “commun,” for “gotta” and many more. The fact that the phonological form of the word is constructed each time we use it makes it easier for the speech production system to be productive and to generate the word in many different ways, depending on the context (Levelt 1992). The exact extent of metric and segmental information in the phonological output lexicon is still debated – for example, whether the metric information includes prosody (Butterworth 1992), or whether there are different representations for vowels and consonants (Caramazza *et al.* 2000).

The phonological output lexicon is organized by word frequency, and hence frequent words are accessed more readily than less frequent ones. As for the representation of morphologically complex words (at least those with regular inflections), it seems that this lexicon only includes the stems of the word, namely, it includes “kangaroo” but not “kangaroos,” “walk” but not “walked.”

Lexical phonology is represented in two separate phonological lexicons: an output lexicon, which is used during speech, and an input lexicon, which is used when hearing words. The phonological output lexicon is also separate from the orthographic lexicon (Coltheart 2004) – we may know how a word sounds, but not how it is spelled. (For example, the reader probably said the word *tomorrow* quite often, but may still wonder whether there are two *m*'s or two *r*'s in the written word; other readers may know the legal word that sounds like /suh-peh-nuh/, but not know how it is spelled.) Conversely, we may know the written form of words that we often read, namely, they may appear in our orthographic lexicon, without knowing exactly how they are pronounced (this could apply to foreign names in books, such as *Yossarian*, names of lexical retrieval researchers, such as *Levelt* or *McCullough*, or city names in signs, such as *Valladolid*, *Llanrwst*, or *Wooloomooloo*, and to irregular words in a

foreign language, such as the words *suffice*, *whole*, or *yacht* in English when read by non-native speakers).

The activation is in turn transferred from the phonological output lexicon to the *phonological output buffer*, a post-lexical, sub-lexical stage that has two functions: maintaining activation and composition. The information that arrives from the phonological output lexicon needs to be held active somewhere until the word is uttered in full. The phonological output buffer is a phonological short-term store, which holds the phonological representation that arrives from the phonological lexicon until the word is produced (e.g., Butterworth 1989, 1992; Dell 1986, 1988; Garrett 1976, 1992; Kempen and Huijbers 1983; Levelt 1989, 1992; Nickels 1997; Patterson and Shewell 1987). It holds units of various sizes: phonemes, morphemes, and number words (Dotan and Friedmann 2007, 2010).

In its other function, the output buffer is a composer. It composes the word by inserting the phonemes into the metrical frame (e.g., Biran and Friedmann 2005; Meyer 1992; Shattuck-Hufnagel 1992). It is also responsible for composing larger phonological units: it composes morphologically complex words from their morphemes (Dotan and Friedmann 2007, 2010; Kohn and Melvold 2000). The buffer is responsible not only for composition at the word level. It seems that the buffer (and/or the phonetic stages immediately following it) is also responsible for the phonological effects of the combination of words, such as the combination of “want to” into “wanna” that we have mentioned earlier, and possibly also for the effects of co-articulation, namely, the obscuration of the boundaries between units in speech production (Kent and Minifie 1977). It might even be, and this is still to be tested and confirmed, that processes at the sentence level that linguists ascribe to the phonological component, the PF, such as verb movement according to some analyses (Chomsky 1995b, 2001), occur in this stage. The output buffer is a component that is separate from the phonological input buffer (Monsell 1987; Franklin, Buerk, and Howard 2002; Nickels, Howard, and Best 1997; Gvion and Friedmann 2008, 2012a; Shallice, Rumiati, and Zadini 2000; Shallice and Warrington 1977). Given that the phonological output buffer is a short-term memory component, it is also affected by the length of the phonemic string it holds (namely, the number of phonemes in a word, or the number of words in a multi-digit number) – longer strings that include more units are harder to maintain and produce, and strings that include more units than its capacity are impossible to maintain and produce in full.

Importantly, the phonological output buffer is involved not only in the process of word retrieval, which we have discussed so far, but also in the production of nonwords. When a person reads a word that is not stored in her orthographic and phonological lexicons, be it a new word or a non-word, she would not be able to use the lexical route. In this case, she will be forced to fall back on a sublexical route of reading, which converts

graphemes to their corresponding phonemes (see [Figure 19.2](#) in the next section). The phonological output buffer holds the products of this conversion until production, and composes the phonemes into a string. Similarly, when one repeats a nonword (or a new or unknown word), it is the phonological output buffer that receives the phoneme string (from the phonological input buffer), holds it, re-composes it and sends it to production.

Eventually, the phonological output buffer sends the phonological representation of the word to *phonetic encoding*, the stage that prepares the phoneme string for articulation and sends it to the motor system. The phonetic encoding stage handles phonemes, and it also uses a mental syllabary, a store of pre-assembled syllables, ready for articulation. For this reason, more frequently used phonemes and syllables are activated more easily (Laganaro [2008](#); Levelt *et al.* [1999](#)). Although the mental store of syllables is located at a phonetic stage, the syllable frequency also affects the phonological output buffer (Laganaro [2008](#)).

19.2.1 The syntactic lexicon

Another component that the semantic lexicon activates, as shown in [Figure 19.1](#), is the *syntactic lexicon*, which stores syntactic information about words. This lexical-syntactic information includes various aspects of words that dictate the syntactic environment in which a word can be inserted. One type of lexical-syntactic information is the information about the argument structure of the verb – the number of arguments it takes, their thematic roles (its thematic grid), and the syntactic types of the verb's complements (its subcategorization frame). For example, for the verb “kiss,” the syntactic lexicon includes the information that it takes two arguments, an agent and a theme, and that the complement of the verb is a noun phrase (*The girl kissed the boy*). For the verb “think,” the complement can be either a sentence (*think that the autumn is a beautiful season*) or a prepositional phrase (*think about the rain*). Namely, the lexical-syntactic information of the verb *think* includes the information that it has two different options for complementation.

Information about nouns is also encoded in the syntactic lexicon. For example, it includes information about the grammatical gender of nouns, a lexical property that determines in many languages the inflection of various constituents in the sentence that agree with the noun (Biran and Friedmann [2012](#); Costa *et al.* [1999](#); Friedmann and Biran [2003](#); Schriefers [1993](#); Vigliocco and Franck [1999](#)). It also includes information about whether a noun is count or mass (Fieder, Nickels, and Biedermann [2011](#); Herbert and Best [2010](#); Semenza, Mondini, and Cappelletti [1997](#)).

The syntactic lexicon includes (only) idiosyncratic properties of lexical items, that is, it only includes properties that do not stem from general principles of Universal Grammar or of a specific language (Chomsky

1995b). For example, the fact that verbs in certain languages are followed by their complements (*I ate pomegranates* and not *I pomegranates ate*) is a general property of those languages (head-first languages) and therefore not part of the lexical knowledge about each verb. In contrast, the set of complements of a particular verb is an idiosyncratic property of that verb, hence part of the verb's lexical entry. Similarly, the grammatical gender of a particular noun is an idiosyncratic property and therefore has to be listed in its lexical entry.

Response-time studies revealed that upon access to a lexical entry of a verb, all its complementation options are activated (Shapiro *et al.* 1987, 1989, 1991, 1993; Shapiro and Levine 1990). Shapiro and his colleagues found that access to the argument structure of verbs is exhaustive, i.e. a verb is always activated together with all the alternative argument structures it has, and hence, accessing verbs with more argument structure options entails longer reaction times to a secondary task. Namely, it takes longer to access and retrieve a verb like "want," which can appear with several possible complements, and hence be part of several verb-phrase structures: *want an apple*, *want to sleep*, *want that the winter will finally come*, than to access a verb like "fix," which has only one possible argument structure, with a noun phrase complement (*fix the radio*). These results are further supported by imaging studies. Shetreet *et al.* (2007, 2010) found brain areas that are more active when a person hears a sentence with a verb that has more complementation options (subcategorization options). Thus, an effect of the number of complementation options on the access to verbs is indicative of a well-functioning syntactic lexicon. A similar pattern of exhaustive access is also reported in the noun domain – the access to ambiguous nouns (such as *letter*, for example) is also exhaustive. When we hear a sentence with an ambiguous word, immediately after the ambiguous word, all of the meanings of that word are activated. According to a line of studies by Dave Swinney and his colleagues, exhaustive access to nouns occurs even when the context strongly points to only one of the meanings (see, for example, Swinney 1979; see also Love and Swinney 1996; Onifer and Swinney 1981).

A short note about the terminology in the lexical retrieval literature is in order here. In early models of lexical retrieval, researchers referred to the lexical entry in the semantic lexicon as *lemma* and to the lexical entry in the phonological lexicon as *lexeme*. With time and development of theories of lexical retrieval, different researchers used the term *lemma* in different ways. Some refer to *lemma* as the entry in a syntactic lexicon (Bock and Levelt 1994; Levelt *et al.* 1998; Roelofs 1992), others refer to it as an entry in the semantic lexicon, yet others use the term for both (Kempen and Hoenkamp 1987; Kempen and Huijbers 1983; Levelt 1989, 1992), namely, according to them the lemma includes a word's semantic representation as well as its syntactic representation. Clearly, the usage of the term relates to the theory one holds with respect to whether or not

there exists a syntactic lexicon that is separate from the semantic lexicon. To avoid confusion, we will not use the term “lemma” here, but rather use the, admittedly longer term, “semantic lexicon entry” and “syntactic lexicon entry.”

19.2.2 Temporary word retrieval failures in normal speakers

Even though lexical retrieval of healthy speakers is usually efficient and rapid, this process sometimes fails. The two main ways in which it fails are cases in which the speaker tries, unsuccessfully, to retrieve a word, and cases in which the speaker retrieves an incorrect word. The temporary inability to retrieve a word is termed “*tip of the tongue*” (TOT) (Biedermann *et al.* 2008; Brown 1991; Brown and McNeil 1966; Levelt *et al.* 1991; Schriefers, Meyer, and Levelt 1990). A person who tries to retrieve the word but gets stuck in a TOT state usually has full semantic information about the word she tries to retrieve, but only partial phonological information. We are pretty sure that every reader has experienced these states, and the feeling that the word is “on the tip of the tongue.” In fact, Brown and McNeil (1966: 326), who were the first to systematically explore TOTs, stated that the signs of TOT are unmistakable, and depicted a person in this situation as being in “mild torment, something like the brink of a sneeze.”

Many studies, starting with the work of Brown and McNeil (1966), inquired into the characteristics of this state. Research provided various characteristics of TOT. For example: people experience TOT universally, TOT states occur about once or twice a week on the average for younger adults, and their frequency increases with age. Speakers often have a feeling of knowing about the target word, and can often provide semantic and phonological information about the target words and judge whether a word is the one they are looking for or not. In about half of the TOT states, the target words are retrieved during the TOT experience, within a minute or two. TOT states frequently occur when trying to retrieve proper names. With respect to the partial phonological information, a bowed serial position effect is often noticed, as speakers in a TOT state are often able to access some of the segments of the target word, usually its first letter, and sometimes also its suffixes. They also tend to be able to recall the number of syllables in the word and the stress position, and often produce words related to the target.

The other way in which retrieval can fail in healthy speakers is in *slips of the tongue*, namely, when an incorrect word or nonword is produced instead of the target word (Fromkin 1971; Garrett 1976; Levelt *et al.* 1991; Schriefers, Meyer, and Levelt 1990). Examination of these speech errors revealed that there are different types of errors, which can be broadly categorized into semantic errors (such as “he sent a fax, ah . . . an email”) and phonological errors (“choung . . . young children,” or “deand end” instead

of “dead end”). TOT states and slips of the tongue provide further support for the separation between semantic and phonological levels in lexical retrieval.

19.2.3 Naming of written words

Reading aloud of a written word is often termed “naming of a written word.” Whereas this is a somewhat confusing term, because we tend to think of naming as a process that starts from a concept, looking at the model for single word reading (Figure 19.2) immediately clarifies in what way reading is intimately related to naming. Firstly, the output of reading aloud uses the same two phonological components we have described in detail in the previous section: the phonological output lexicon and the phonological output buffer. These components play a role not only in the production of a word from a concept but also in the production of a word that has arrived from the orthographic input lexicon. In addition, the conceptual and semantic components we have described before also take part in the reading process – they are responsible for the comprehension of written words that arrived in the orthographic input lexicon.

Figure 19.2 shows the dual-route model for single word reading. This model is the result of the work of cognitive neuropsychologists such as Max Coltheart, John Marshall, Tim Shallice, Karalyn Patterson, Andrew Ellis, Andrew Young, and others. This model describes the stages that a reader passes from seeing a written word until its “naming,” i.e. reading aloud using the phonological output lexicon and the phonological output buffer. It also describes the stages of written word comprehension, via the semantic lexicon and the conceptual system. The first stage of this model includes orthographic-visual analysis that is responsible for the encoding of letter identities and position (Coltheart 1981; Ellis 1993;

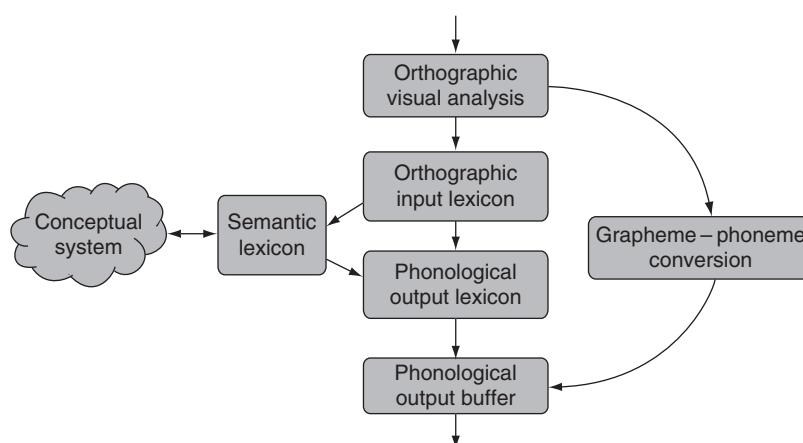


Figure 19.2 The dual-route model for single word reading.

Ellis, Flude, and Young 1987; Ellis and Young 1996; Humphreys, Evett, and Quinlan 1990; Peressotti and Grainger 1995). After this orthographic-visual analysis, the information flows in two routes for reading aloud: the lexical route, which includes the orthographic input lexicon and the phonological output lexicon and buffer, and the sublexical route, in which reading proceeds via grapheme-to-phoneme conversion. The orthographic input lexicon holds the orthographic information about the way the written words we know are written, and is connected to the phonological output lexicon. The direct connection between these lexicons allows for a rapid and accurate conversion from a written word to its phonological form. All words that the reader knows can be read via this lexical route, and it is the only route that enables the accurate reading of irregular words like *talk*, *knight*, or *debt*, which are stored in the two lexicons. The other route for reading aloud is the sublexical route, in which letter strings are converted into sounds via grapheme-to-phoneme conversion. This route enables the reading aloud of new words, which are not yet stored in the orthographic input lexicon, and of nonwords. In addition to these lexical and sublexical routes for reading aloud, the model includes a connection between the orthographic input lexicon and the conceptual-semantic system (through the semantic lexicon to the conceptual system), which allows for the comprehension of written words. Thus, naming of a written word shares components with the oral naming of words that originate in a concept.

19.3 Types of anomia

Anomia is a deficit in lexical retrieval (from *a* = not, *nomn* = name). The process of lexical retrieval can fail due to several different deficits: in the conceptual level, in the semantic lexicon, in the syntactic lexicon, in the phonological output lexicon, in the phonological output buffer, or in the connections between these components. Impairments in these different loci create lexical retrieval problems with different characteristics. Knowing these characteristics can help in identifying the exact locus of the deficit of an individual with anomia. It therefore has not only theoretical but also clinical importance, because different types of anomia often require different kinds of treatment (Nickels 2002). Picture naming is the most commonly used task for the examination of lexical abilities (Best 2005; Kambaranos 2008, 2010; Levelt *et al.* 1998), as it involves all stages of word production and enables us to examine the information the participants have about the target word, both quantitatively and qualitatively.

In the following pages we describe the various types of anomia resulting from deficits in the different possible loci. We describe the characteristics of selective anomias – namely, what happens when an individual has a

single deficit along the lexical retrieval process. On top of these, there exist more complex impairments, of individuals who have a combination of deficits in several components, resulting in the combination of the relevant symptoms.

19.3.1 A deficit in the conceptual system

A deficit in the conceptual system is not a lexical retrieval deficit per se. Indeed, it results in lexical retrieval failure, but it is located in a general cognitive stage, before the lexical stages, and it is bound to cause not only difficulties in lexical retrieval but also impaired comprehension of words and sentences, presented auditorily or written, and impaired comprehension of non-verbal situations and of pictures and objects. Such individuals, who have word production disorders that result from a conceptual deficit, produce paraphasias (naming errors) that are not necessarily related to the target word, such as “bridge” for “grapefruit.” They do not typically make phonological errors, and should not have any specific problems in non-word tasks such as nonword repetition (providing they understand the task), because nonword production does not involve the conceptual system. They fail not only in verbal tasks but also in conceptual tasks such as odd-one-out in pictures (for example, a picture of a pen, pencil, eraser, and a tooth brush), and picture association test (asking, for example, which of two pictures is more closely related to a picture of an umbrella – a picture of the sun or a picture of a rainy cloud). Such a conceptual impairment is typical for at least some of the aphasics who are diagnosed with Wernicke’s aphasia.

19.3.2 A deficit in the semantic lexicon, semantic anomia

A deficit in the semantic lexicon would also cause incorrect naming, but would take a different form. Individuals with semantic anomia mainly produce semantic paraphasias, such as *table* for *chair*, *giraffe* for *zebra*, or *apple* for *plum*. They might show an imageability effect, with better production of high-imageability (concrete) words than of low-imageability (abstract) words, and prototypicality effect, with more typical items of the category, like *apple*, being produced instead of the less typical ones, like *plum* (Cohen-Shalev and Friedmann 2011). Because the semantic lexicon is most probably shared by production and comprehension processes, individuals with impairment at the semantic lexicon fail not only in word retrieval but also in the comprehension of written and heard words. Unlike individuals with a conceptually based deficit, the deficit of individuals with an impairment to the semantic lexicon is limited to the verbal system. They perform well in picture tasks such as picture odd-one-out and picture association, but fail in the parallel tasks that involve words. Thus, they show difficulties in written and spoken word versions of the

odd-one-out and word association tasks (asking, for example, to choose a word that is more closely related to the written/heard word “umbrella,” between the two written or heard words “sun” and “rain”). Because semantic anomia is rooted in a lexicon, and not in the phonological buffers, individuals with this type of anomia read and repeat nonwords correctly, and do not make phonological paraphasias in naming.

A further phenomenon that is ascribed to a deficit in the semantic lexicon is category-specific naming impairment. Individuals with this deficit may be impaired, for example, in retrieving the names of living things but not of nonliving things, or show a reverse pattern, with greater difficulty with nonliving things than living things (e.g., Capitani *et al.* 2003; Humphreys and Forde 2001; Laiacona *et al.* 2001; Mahon and Caramazza 2006; Tyler and Moss 2001; Warrington and McCarthy 1983, 1987; Warrington and Shallice 1984). The deficit applies not only to word retrieval but also to input tasks such as providing attributes pertaining to heard names of animals. The grain of specificity of the category specific impairment can be quite striking. For example, patients have been described who have a deficit in living animate things (i.e. animals) but not in living inanimate things (i.e. fruit/vegetables, Caramazza and Shelton 1998; Damasio *et al.* 1990; Silveri and Gainotti 1988). Other patients show a selective deficit in fruits and vegetables. As an explanation for these category-specific semantic impairments, Caramazza and Shelton (1998) suggested that the semantic-conceptual system is organized domain-specifically, according to an innate sets of categories.

19.3.3 A deficit in the phonological output lexicon: lexical-phonological anomia

A deficit in the phonological output lexicon also causes incorrect naming, but this naming disorder exhibits different characteristics than the two previously described deficits. Individuals who are impaired in this level understand concepts well, and can access their corresponding representation in the semantic lexicon, but fail to activate the correct entry in the phonological output lexicon. As a result, they make phonological paraphasias. Typically, these individuals make not only phonological paraphasias but also semantic paraphasias, possibly because they do not have access to the phonological representation of the target word and thus a representation of a semantically related word is activated (see Caramazza and Hillis 1990, and Howard and Gatehouse 2006).

Because the phonological output lexicon is organized by frequency, these individuals show a frequency effect (Jeschniak and Levelt 1994), whereby they make more errors on the least frequent target words (in fact, the phonological output lexicon is the only module in the speech production system that is sensitive to word frequency, and causes such a frequency effect). Because this deficit is in a stage following the conceptual and lexical-semantic stages and

does not affect them, individuals with a deficit in the phonological lexicon perform well on conceptual comprehension tasks with pictures. Because their deficit is in the phonological output lexicon, which is separate from the phonological input lexicon, they understand heard (and written) words well. Because nonword production does not involve the lexicons, they do not have problems in reading or repeating nonwords.

Another implication of a deficit in the phonological output lexicon relates to reading aloud. Recall that the phonological output lexicon is part of the lexical reading route (Figure 19.2), the route that allows for accurate and rapid reading, and is especially crucial for reading aloud of irregular words. When the phonological output lexicon is impaired, reading aloud, even of words, cannot proceed via the direct lexical route, and reading must therefore be done via the sublexical route (see Gvion and Friedmann 2012c for a report of individuals with acquired and developmental anomia in the phonological output lexicon who demonstrated surface dyslexia in reading aloud). Reading through the sublexical route results in words being read more slowly, and irregular words being read aloud incorrectly (for example, *walk* may be read with a pronounced *l*). The incorrect reading is most evident in reading potentiophones, words that when read via the sublexical route can be read as other existing words. These potentiophones can be read as the other word – for example, *know* can be read as *now*, and *angle* as *angel*. Thus, a deficit in the phonological output lexicon results not only in phonological and semantic errors in naming but also in regularization errors in reading aloud, which are characteristic of surface dyslexia (Broom and Doctor 1995; Castles, Bates, and Coltheart 2006; Castles and Coltheart 1993, 1996; Coltheart and Byng 1989; Coltheart and Funnell 1987; Coltheart et al. 1983; Ellis et al. 2000; Ferreres et al. 2005; Friedmann and Lukov 2008; Howard and Franklin 1987; Judica et al. 2002; Marshall and Newcombe 1973; Masterson 2000; Newcombe and Marshall 1981, 1984, 1985; Temple 1997; Weekes and Coltheart 1996).

Semantic errors occur both in a semantic lexicon impairment and in a phonological lexicon impairment; however, there are several straightforward ways to distinguish between these impairments. Firstly, word comprehension is impaired in a semantic lexicon impairment but not in a phonological output lexicon impairment. Secondly, phonological output lexicon impairment also entails phonological errors, whereas individuals with semantic lexicon impairment do not make phonological errors. Finally, deficits at the semantic and phonological lexicons are affected by different types of cues and treatments. As Nickels (2002) indicated, for each type of naming deficit, resulting from a deficit in different stages of processing, a different type of treatment will be suitable. Thus, a treatment that focuses on word meaning (e.g., picture–word matching task) would help in cases of semantic naming deficit, and a treatment that focuses on the phonemes of the word (e.g., word repetition task) would be effective in cases of phonological naming deficit (see also Makin et al. 2004, and Nickels 2002). Indeed, phonological treatments (using repetition, rhyming, or first phoneme) were found to improve

word retrieval for phonologically impaired patients in several studies (Lambon Ralph, Sage, and Roberts 2000; McDonald et al. 2006; Miceli et al. 1996; Raymer et al. 1993). An improvement due to a semantic treatment was reported by Hillis (1998), who described a patient with a semantic impairment (as well as impairments in other levels of processing) whose naming was improved after a treatment that focused on “teaching specific semantic features.”

This difference between semantic and phonological facilitation was also found for cues. Namely, when, upon retrieval failure the experimenter provides the patient with a cue, either phonological or semantic. Biran and Friedmann (2002) reported that individuals with impairment at the lexical-semantic level were assisted by a semantic cue and individuals who were impaired at the phonological level were assisted by phonological cues.

19.3.4 A deficit in the connection between the semantic and the phonological output lexicons

Impaired naming can result not only from a deficit in the components themselves, but also from disconnections between them (as Carl Wernicke had noticed already in his 1874 book). The impaired connection that is most easily described and distinguished from a deficit in the components is the impaired connection from the semantic lexicon to the phonological output lexicon. This disconnection results, like lexical-phonological anomia, in phonological and semantic paraphasias, alongside good comprehension of pictures and words, and good reading and repetition of nonwords (and this was in fact the pattern presented by the classic case of patient Johann Voit, described by Garshey and discussed by Wernicke 1886; see De Bleser's 1989 translation of Wernicke's paper, and Bartles and Wallesch 1996 for a discussion). It differs from lexical-semantic anomia in that individuals with this disconnection are expected to understand heard and read words well, but to fail in producing them. It differs from lexical-phonological anomia in that reading can still be done via the phonological output lexicon, and hence the reading should not include regularizations of irregular words.

19.3.5 A deficit in the phonological output buffer: phonological buffer anomia

Individuals with phonological output buffer impairment also have word production problems. Their error pattern includes only phonological errors, and no semantic errors (they may, when failing to produce a word, produce instead another word that is similar in meaning, but they would know that it is not exactly the word they meant to use). Because their deficit lies a long way after the conceptual and semantic stages, they have no problems in comprehension tasks of pictures, written words, or auditorily presented words. They do have a marked difficulty with

nonwords, because the phonological output buffer is responsible for holding and composing phonemes of nonwords, in reading and repetition tasks. Their difficulty with nonwords and new words is often more severe than their difficulty with real words, because nonwords cannot rely on activations from the lexicon to support their production.

Because the phonological output buffer is a phonological short-term component, it is affected by the length of the phonemic string it holds (Franklin, Buerk, and Howard 2002; Nickels 1997) – strings that are longer than its capacity are affected, and their phonemes are omitted or substituted. Therefore, a word *length effect* indicates the involvement of the buffer, and naming in phonological output buffer anomia is considerably influenced by the length of the target word (unlike deficits in earlier stages). Additional effects that are unique to anomia in the phonological output buffer, or in the phonetic encoding stage, are the syllable and phoneme frequency effects: individuals with phonological output buffer anomia make fewer errors in frequent syllables than in infrequent syllables, and fewer errors in frequent phonemes than in less frequent ones (Goldrick and Rapp 2007; Laganaro 2005). Syllable and phoneme frequency are inter-correlated, and Laganaro (2005) found that the analysis of frequency by syllables is more reliable. The syllable frequency effect is assumed to be caused by failure of access to the mental store of syllables, which holds pre-assembled syllables (Laganaro 2008). The phonological output buffer is closely related to phonological short-term memory (pSTM). When tested in pSTM tasks, individuals with impaired phonological output buffer typically show poor recall performance in tasks such as digit span, word span, and nonword span (Gvion and Friedmann 2012a,b). In pSTM tasks that involve recognition, without oral recall, these individuals may show normal performance, unless their phonological input buffer is also impaired.

Finally, recall that the phonological output buffer composes words from phonemes and metrical information, and morphologically complex words from morphemes. Therefore, a deficit in the phonological output buffer affects these composition procedures. Importantly, because the buffer handles units of different sizes (phonemes, pre-assembled morphemes, and pre-assembled whole digit names), a deficit in this buffer has different effect on these different types of words. Words are produced with incorrect order of phonemes (fennel → fellen) or with substitution or omissions of phonemes (fennel → feddel); morphologically complex words are produced with substitution, omission, or addition of morphemes (adjustment → adjustive); and numbers are produced with digit substitutions (4068 → four thousand and sixty three). Crucially, because the morphemes and the digits are already pre-assembled in the buffer, individuals with phonological output buffer anomia usually do not make phonological errors within morphemes or within digits. Individuals with (output) conduction aphasia are typically impaired in the

phonological output buffer (Franklin, Buerk, and Howard 2002; Gvion and Friedmann 2012a; Pate, Saffran, and Martin 1987; Shallice, Rumiati, and Zadini 2000).

19.3.6 A deficit in the syntactic lexicon

Deficits in the syntactic lexicon are somewhat different in nature than the aforementioned anomias. Whereas it is a deficit in the process of lexical retrieval, it typically does not cause errors in the traditional single word tasks of picture naming. Such deficits do have far-reaching implications, especially for sentence construction. If we take grammatical gender as an example for information in the syntactic lexicon, in many languages when requested to name a picture of an object, a bare noun suffices, and there is no need to access the gender of the noun. However, when this noun is incorporated in a sentence or a phrase, in many languages this would require the agreement of another constituent (such as the article) with this noun, or in syntactic terms, this would include an agreement feature that needs to be checked. In this case, the gender of the noun in the syntactic lexicon needs to be accessed. If the syntactic lexicon is impaired, the agreement of the noun with the verb, with pronouns, adjectives, and determiners would fail. Therefore, to detect an impairment in the syntactic lexicon, tasks at the phrase or sentence level should be used (Biran and Friedmann, in press; Friedmann and Biran 2003; Schriefers 1993).

Impairments at the syntactic lexicon can also selectively affect the speaker's knowledge of the complements and arguments the verb can appear with (its *predicate argument structure*, *subcategorization*, *thematic grid*, or *complementation frame*). If this information is impaired, this can also critically hamper the construction of sentences. Sentences can be produced without the required complements, or with incorrect complements. Such impairment in the Predicate Argument Structure has been termed "aPASia" (Biran and Friedmann 2012). In a study that used the effect of the number of complementation options on reaction time, Shapiro and Levine (1990) and Shapiro *et al.* (1993) found that individuals with Broca's aphasia showed the effect, and hence demonstrated sensitivity to verbs' PAS. In contrast, individuals with Wernicke's aphasia did not show sensitivity to the number of complementation options that a verb has in its lexical entry. This suggests that an impairment in Wernicke's area (left STG) causes aPASia.

Whereas this impairment in the syntactic lexicon clearly affects the syntactic structure of sentences, it should not be confused with another condition, agrammatic aphasia, which affects the syntactic structure building, but does not necessarily affect the syntactic knowledge at the single word level (Shapiro and Levine 1990; Biran and Friedmann 2012). Similarly, in developmental language impairments,

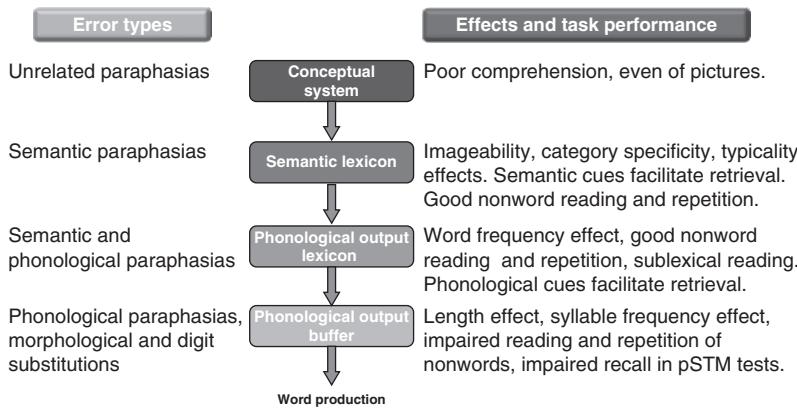


Figure 19.3 Errors and effects that characterize each of the types of anomia.

children may have significant syntactic deficits without a deficit to the syntactic lexicon (Kenan *et al.* 2007).

19.3.7 A summary of the properties of the various types of anomia

This pattern of impairments suggests a way to diagnose the locus of impairment of each person with a naming impairment. In Figure 19.3 we summarize the main error types and effects distinguishing between the different impairment patterns.

19.4 Developmental anomia and its subtypes

Naming deficits are frequent among children with impaired language. For example, Dockrell *et al.* (1998) reported that 23% of the children treated by speech therapists in the UK had naming deficits. When a child has a lexical retrieval deficit, it manifests itself in naming tasks as well as in free conversation (German 2002).

Several studies examined the types of errors children produce when they fail to retrieve a word and the types of preserved information they have. These studies found that children produce various types of errors: semantic errors, phonological errors, “don’t know” responses, unrelated errors, and visual errors (Dockrell *et al.* 1998; Kambaran and Grohmann 2010; Lahey and Edwards 1999; McGregor 1994, 1997, and others). Semantic errors seem to be the most frequent type of naming error in both language-impaired and typically developing English-speaking children aged 3–6 (McGregor 1997), but language-impaired children produced more phonological errors than the typically developing children. McGregor *et al.* (2002) also found that semantic errors were the most frequent, and that most of them were coordinate (i.e. at the same level of object – car-train), in a group of typically developing children aged 5–7 years.

Moreover, there is evidence supporting the separation between processing of semantic and phonological information in children, like in adults after brain damage. Faust, Dimitrovsky, and Davidi (1997), for example, found that Hebrew-speaking language-impaired children in second and third grades could provide semantic information regarding words they could not retrieve phonologically. Namely, they had access to the semantic information of the word but not to its phonological information, indicating separate semantic and phonological levels.

Importantly, it seems that developmental lexical retrieval deficits take very similar forms to the ones revealed in the work on acquired lexical retrieval deficits in adults. Some researchers take the view that it is possible to apply the framework developed in the research of adults with acquired anomia to developmental disorders, of course considering the developmental stage of the child (Best 2005; Friedmann and Novogrodsky 2008; Novogrodsky, Kreiser, and Friedmann 2010). Firstly, within this view, developmental language impairments cause selective impairment in various language abilities. Thus, developmental language impairments include selective deficits in syntax (syntactic SLI or SySLI, Friedmann and Novogrodsky 2008, 2011), with good lexical retrieval abilities, and impaired lexical retrieval (a deficit that received various names, including lexical SLI, LeSLI, developmental anomia, and dysnomia), with unimpaired syntax. Then, within the developmental lexical retrieval deficit, distinct types can be identified, in line with the types described above for adults with acquired anomia.

To demonstrate how children and adolescents with lexical retrieval impairments may be classified into the different anomia patterns described above, we present here four case studies, whose loci of impairment are summarized in [Figure 19.4](#). Esther, Michal, and Ofer are Hebrew-speaking children described in Friedmann, Hadar-Ovadya, and Levy (in press), and Revital was described by Novogrodsky, Kreiser, and Friedmann (2010).

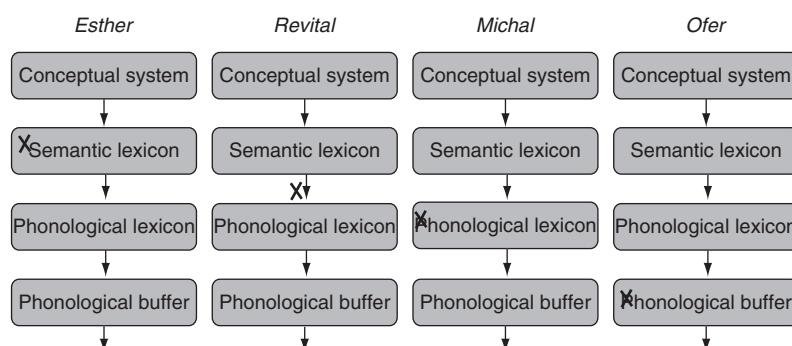


Figure 19.4 The loci of impairment of four children with developmental anomia.

19.4.1 Esther, a girl with developmental semantic lexicon anomia

When we met Esther and evaluated her lexical retrieval abilities, she was in 4th grade and 10;2 years old. She named correctly only 81% of the words in a picture-naming test (on which age-matched typically developing children perform 95% correct and above). Esther performed well in the association tests that involved pictures, and had no semantically unrelated paraphasias, which indicates that her conceptual system was intact. She had many semantic paraphasias, and poor performance in the association test in which the items were words rather than pictures. She demonstrated a category-specific deficit in fruits and vegetables, and semantic cues helped her word production. All these symptoms are consistent with a semantic lexicon deficit. She had no phonological paraphasias, and phonological cues did not help her production, indicating that her phonological output lexicon was intact. She also showed no length effect, and good nonword repetition and reading, indicating that her phonological output buffer was intact.

19.4.2 Revital, a girl with developmental anomia in the connection between the semantic and phonological lexicons

When we met Revital she was in 6th grade and 12;9 years old. She named correctly only 74% of the words in the picture-naming test. Like Esther, she performed well in the picture-matching conceptual tests, and had no semantically unrelated paraphasias, which indicates that her conceptual system was intact. Her impaired picture naming included 28% semantic errors and 8% phonological errors, showing that her deficit could not be ascribed to a pure deficit at the semantic lexicon: had she had a semantic lexicon deficit, one would not expect phonological errors. She also did not have a deficit in the phonological output buffer: her repetition of nonwords and her reading of nonwords were intact. She also showed normal phonological STM. Another indication for the involvement of the phonological stages (and specifically the phonological lexicon, because an impairment in the phonological output buffer was ruled out), on top of her phonological errors in naming, was the fact that phonological cues helped her retrieve words.

To distinguish between a deficit in the phonological output lexicon itself and a deficit in the access to this lexicon from the semantic lexicon, we assessed her oral reading of irregular words and potentiophones. Her very good reading of irregular words indicated that she is able to read via the lexical reading route (the route between the orthographic input lexicon and the phonological output lexicon), and hence her deficit is not in the phonological output lexicon. Thus, all these

symptoms are consistent with a deficit in the access from the semantic lexicon to the phonological output lexicon, and this was indeed our diagnosis.

19.4.3 Michal, a girl with developmental phonological lexicon anomia

Michal was a 10;2 year old girl in 4th grade when we tested her lexical retrieval abilities. She named correctly only 81% of the words in the picture-naming test. She performed well in the association test that involved pictures, and had no unrelated paraphasias, indicating that her conceptual system was intact. She also performed well in the lexical association task, indicating an intact semantic lexicon. In the naming task she produced semantic and phonological paraphasias. She showed word frequency effect, and her reading was characterized by surface dyslexia. All these suggest that her phonological output lexicon was impaired. Her phonological output buffer, conversely, was intact: she showed no length effect, good repetition of nonwords, good reading of nonwords, and good performance in the phonological STM recall tasks.

19.4.4 Ofer, a boy with developmental phonological output buffer anomia

Ofer was a 11;3 year old boy in 5th grade when we tested him. He named correctly only 84% of the words in a picture-naming test. Like the three other participants, he performed well in the association test that involved pictures, and had no unrelated paraphasias, indicating an intact conceptual system. In addition, he had good performance on the association test that included written words, and in word-picture matching tasks, indicating that his semantic lexicon was intact.

He produced mainly phonological paraphasias, and made only few semantic paraphasias, which he immediately corrected. Phonological cues of first phoneme helped him retrieve all but one word he had been unable to retrieve before the cue. All these findings point in the direction of a deficit at the phonological output stages.

The picture with respect to effects on his naming and the production of nonwords provides the last piece of the puzzle, by answering the question of whether his deficit is at the phonological output lexicon or a deficit at the phonological output buffer. Ofer showed no frequency effect on his naming, but did show length effect and syllable frequency effect. He demonstrated poor repetition of nonwords, poor reading of nonwords, and poor phonological working memory, all indicating an impairment in the phonological output buffer.

19.5 The biology of lexical retrieval development

Thus, it is clear that developmental anomia exists, and in fact, various types of developmental anomia exist. Not much is known at this stage about the biological basis of these selective impairments, or about the biological path of the development of lexical retrieval. Some studies focus on the genetic bases of language impairments, as summarized in the chapter by Benítez-Burraco. One recent study pointed at another cause for developmental lexical retrieval impairments. Fattal, Friedmann, and Fattal-Valevski (2011) conducted a study of children who suffered a deficiency in one specific micronutrient, thiamine (vitamin B1), during their first year of life. Their lexical retrieval was tested when they were 5–7 years old. Thiamine plays a central role in cerebral metabolism. It serves as a cofactor for several enzymes involved primarily in carbohydrate catabolism, converting glucose to energy, the only energy source in the brain. These enzymes are important in the biosynthesis of a number of cell constituents including neurotransmitters. Thiamine also has a structural role in membrane structure and function, it intervenes in synaptic transmission, and plays a role in cellular differentiation, synapse formation, axonal growth, and myelinogenesis.

Fattal *et al.*'s study tested the effect of thiamine deficiency on lexical retrieval. They discovered that 88% of these children showed significant lexical retrieval impairments. They had hesitations, "don't know" responses, semantic paraphasias, and some morphological errors. The conceptual abilities of these children were generally intact, their IQ was normal, and their vocabulary was normal. The thiamine deficiency during infancy thus seems to have caused long-lasting disorders in lexical retrieval, in the establishment of the cascaded mechanism for lexical retrieval we described in this chapter. Because of the general role thiamine plays in the brain and in brain development, it is probable that the effect of thiamine deficiency is not on a specific brain region that specializes in lexical processing. One possible mechanism is that when the brain is unable to provide the necessary substrate for the development of the lexical retrieval mechanism during a certain critical period, the damage is permanent. This mechanism cannot develop at a later stage, after the critical period, even if thiamine is already present. This study thus demonstrates that thiamine is crucial for the development of the normal process of lexical retrieval, and it also suggests that the development of this process has a certain early critical period. (See Calderon and Naidu 2000; Yoshinaga-Itano 2003; Yoshinaga-Itano and Apuzzo 1998a, 1998b for studies that report results regarding early critical period for language and communication abilities in general, and see Friedmann and Szterman 2006; Szterman and Friedmann 2003, for critical period for first language syntax. See also Ruben 1997 for different critical periods for different language domains, and Monjaize *et al.* 2005 for a discussion of critical period in children with epilepsy.)

19.6 Lexical retrieval in the brain

There are several methods to map the functional components of lexical retrieval onto specific brain locations. One group of methods assesses brain activations in the healthy brain; the other assesses brain areas in individuals with anomia. Within the realm of imaging of normal brains, one method is using brain imaging results to compare brain activations during different tasks, and looking for brain regions that show specific activation patterns (Indefrey 2007, 2011; Indefrey and Levelt 2000, 2004). For example, brain regions that serve the semantic lexicon or the phonological output lexicon are expected to be active in tasks of picture naming but not during nonword production. Another method, aiming to identify brain regions that are *necessary* for word production, is looking for regions in which transient lesions, induced by electric stimulation, interfere with certain word production tasks (Indefrey 2007, 2011, and references therein). Yet another method is using MEG to break down the time course of naming and identify different parts of sequential naming processes (Indefrey and Levelt 2004; Levelt et al. 1998).

Finally, a way that is more intimately related to the types of anomia we have discussed above is targeted at identifying brain areas that are necessary for the various components of lexical retrieval. This is done using structural imaging of the brains of individuals with anomia, by looking for shared brain areas that are impaired for a group of individuals with an impairment in the same functional component.

19.6.1 Brain areas identified in imaging studies with healthy speakers

Indefrey and Levelt (2000, 2004) and Indefrey (2011) conducted comprehensive meta-analyses of tens of imaging studies of the regional cerebral brain activation patterns observed during various lexical retrieval tasks.

Conceptual processing was found, in imaging studies as well as in aphasia research, to involve widespread activation in a large set of brain regions, including the posterior inferior parietal lobe, middle temporal gyrus (MTG), the fusiform and parahippocampal gyri, the dorsomedial prefrontal cortex, the inferior frontal gyrus (IFG), ventromedial prefrontal cortex, and posterior cingulate gyrus, primarily in the language dominant (usually left) hemisphere (Binder et al. 2009; Binder and Price 2006).

The next stage in lexical retrieval is the access to the entry in the semantic lexicon. Indefrey et al.'s meta-analyses indicated that the mid section of the left MTG is active during word generation and picture naming, but not during reading. They concluded that this region serves the “conceptually-driven lexical selection” (Indefrey 2011), which, within the model we presented above, can be thought of as the activation of the

semantic or the syntactic lexicon from the conceptual system. This mapping is also supported by the fact that semantic priming effects were also found in this region (de Zubicaray *et al.* 2001), and lesions in this region were found to be associated with semantic errors in aphasic patients (Schwartz *et al.* 2009). Data from MEG studies show a relatively large range of activation times, but it is still largely compatible with the assumption that the left MTG is activated around an early time window of 175–250 milliseconds, the time window during which the selection of the entry in the semantic lexicon is assumed to occur (e.g., Maess *et al.* 2002). An MEG study conducted by Levelt *et al.* (1998) reported activation related to the semantic/syntactic lexicon in the right parietal cortex, along the posterior end of the superior temporal sulcus.

Not many studies investigated the neural representation of the syntactic lexicon. However, a series of fMRI studies of the representation of argument structure by Shetreet *et al.* (2007, 2009a, 2009b, 2010; Shetreet and Friedmann 2012) pointed to several areas that are repeatedly activated with respect to lexical-syntactic properties of verbs. These areas include the left posterior superior temporal gyrus (STG), the MTG, the precuneus, and two areas in the IFG, which do not include Broca's area: BA 9 and BA 47.

The phonological output lexicon was identified in some imaging tasks by being involved in the production of words but not of nonwords. This pattern is most evident in the posterior parts of the left superior and middle temporal gyri (namely, the information seems to flow posteriorily and superiorily from the area identified as the semantic lexicon). Another type of evidence for the involvement of the left STG in lexical-phonological activities can also be seen in the reduced activation in this area when a phonological distractor is used (Bles and Jansma 2008; de Zubicaray *et al.* 2002). Analysis of effects showed it to be sensitive to lexical frequency but not to word length and object familiarity – a pattern that is consistent with the phonological output lexicon (Graves *et al.* 2007; Wilson *et al.* 2009). Finally, MEG studies on the left STG show activation times largely consistent with the time window starting at 275 milliseconds after the presentation of the stimulus and onwards, which is assumed to be the time in which the phonological output lexicon is accessed (see Indefrey 2011 and references therein). Levelt *et al.* (1998), in a pioneering MEG study of lexical retrieval, ascribed phonological encoding to the posterior STG and the temporoparietal junction. It should be noted, however, that not all researchers share this view regarding the phonological output lexicon activations in posterior STG. Edwards *et al.* (2010), for example, claim that the left posterior STG participates only in speech comprehension, and its role in speech production is merely auditory self-monitoring.

With respect to the localization of the phonological output buffer, it seems that studies of lesions of individuals with conduction aphasia, which we review below in Section 19.6.2, provide a fuller picture than the imaging studies with healthy participants. Hickok and Poeppel (2007)

suggest that the phonological buffer is represented in area Spt (Sylvian parietal temporal – an area within the Sylvian fissure at the parietal-temporal boundary), which activates Broca's area and sends it motor rather than phonological information. According to Hickok and Poeppel, the phonological buffer is an emergent property of sensory-motor circuits and sensory-motor integration (see also Buxbaum *et al.* 2011).

In search for post-lexical areas, Indefrey (2011) mentions the left posterior IFG in relation to the syllabification process, and presents in detail the controversy about the exact role of this area, and in which post-lexical process it is involved. Data from individuals with an impairment in the phonological output buffer, which we survey below, suggest that the left posterior IFG serves later, post-lexical stages, rather than the phonological output buffer. The right supplementary motor area (SMA) and the left anterior insula were also mentioned as post-buffer phonetic areas, related to articulatory processing (Indefrey 2011; Indefrey and Levelt 2004).

19.6.2 Brain areas identified from lesions of individuals with anomia

DeLeon *et al.* (2007) used a different approach to associate brain areas with specific cognitive functions. They examined a group of 116 aphasic patients who were within 24 hours from an acute left hemisphere stroke. They assessed these patients' functional impairment in lexical retrieval and matched it to their brain lesions. First, they administered to each patient a battery of language tasks, and identified the functional locus of impairment for each patient. They did this by analyzing error types, similarly to the analyses described throughout this chapter. This resulted in classifying the patients into four groups, according to their functional deficit: (1) a deficit in the conceptual/semantic system; (2) a deficit in a lexical stage that is common to speech and writing, which they termed "modality-independent lexical access"; (3) a deficit in the phonological output lexicon; and (4) a deficit in post-lexical stages, i.e. the phonological output buffer or the articulatory stages. Along with this functional classification, DeLeon *et al.* analyzed brain imaging data (obtained using magnetic resonance diffusion and perfusion imaging) to identify the anatomic areas damaged by the stroke in each patient. Eventually they performed statistical analysis to associate the brain areas with the functional deficits. Using this procedure, they discovered several areas that are crucially involved in the various stages of lexical retrieval. An important difference exists between functional imaging studies of healthy participants and studies like De Leon *et al.*'s, which assess brain lesions and their functional effects. Whereas functional imaging studies of healthy participants provide evidence for the areas that are reliably activated in a certain task or comparison of tasks, studies of lesions can provide a window into which areas are *necessarily* involved in a specific functional stage.

DeLeon *et al.*'s study found that lesions in Brodmann areas 21 (which roughly corresponds to the MTG) and 22 (STG, including Wernicke's area) were most frequent in the group of patients with a semantic/conceptual deficit. They therefore concluded that these areas serve the conceptual/semantic system – namely, the conceptual system and/or the semantic lexicon. Indeed, area 21 more or less overlaps the MTG, which, according to the imaging studies on healthy participants described above, serves conceptual processes and "semantically driven lexical access."

A deficit in modality-independent lexical access stage (which might correspond to the semantic lexicon) and in the phonological output lexicon were associated with left Brodmann 37 (posterior, inferior temporal/fusiform gyry) and 39 (angular gyrus), again, areas adjacent to (though not identical with) the left posterior STG and MTG, which were found as the areas serving the phonological lexicon in imaging studies of healthy individuals.

The post-lexical deficit was found, in DeLeon's study, to be associated with lesions in areas 44 and 45 (inferior frontal gyrus, IFG, Broca's area), but it seems that these areas were not involved in the phonological output buffer but rather in later motor and articulatory processes (as can also be seen in another study of a temporary lesion of Broca's area, Davis *et al.* 2008). Generally, studies of impairments in the phonological output buffer in conduction aphasia have consistently indicated that phonological buffer impairments are related to lesions in the STG and inferior parietal cortex (Baldo, Klostermann, and Dronkers 2008), mainly in the left posterior STG (Hickok *et al.* 2000; Hickok and Poeppel 2007) and in the anterior supramarginal gyrus (Palumbo *et al.* 1992; Damasio and Damasio 1980). Within these two areas, the output type of conduction aphasia (and hence, phonological output buffer) is probably localized in the supramarginal area, and the superior temporal areas are more probably related to the phonological input buffer (Axer *et al.* 2001).

19.7 Epilogue

Thus, the lexical retrieval process is a complex, multi-staged process, which is tightly related to other language domains such as reading, comprehension, and syntax. We have described in this chapter the various stages of this process, their biolinguistic bases and neural correlates, and the patterns of acquired and developmental anomias that result from selective deficits in each of the stages. The ever-growing body of theoretical and empirical knowledge on the normal process of lexical retrieval contributed to the study of lexical retrieval breakdown and its neural substrates. Knowing the various ways in which lexical retrieval can break down and the way the various lexical retrieval components are

implemented in the brain, in turn, have provided constraints on the theory of lexical retrieval. Apart from the importance of the theoretical knowledge that has accumulated about lexical retrieval theories, research of normal and impaired lexical retrieval bears insurmountable importance for the diagnosis of the specific deficit of each individual with anomia, and for the selection of the most appropriate treatment for each individual.

20

Genetics of language: Roots of specific language deficits

Antonio Benítez-Burraco

20.1 Genetics of language

The feasibility of the identification and the structural and functional characterization of genes related to language (i.e. genes whose products plausibly play a relevant role in the regulation of the development and functioning of neural centres involved in the processing of linguistic stimuli) seems legitimated (and required) by two groups of evidence of different though necessarily related kinds. The first group concerns that evidence that suggests that the nature of the faculty of language (henceforth, FL) would be substantively innate, which basically means that the linguistic competence finally acquired by individuals at the end of their development cannot be uniquely (and properly) explained as a final result of a process of inductive learning, which would depart from the data which make up the linguistic input such individuals are confronted with during growth (Chomsky 1959a; 1980: 34; Fodor and Crowther 2002). Following Chomsky (1980: 75–76), and borrowing the concepts from the field of genetics, Anderson and Lightfoot (1999) have consequently postulated the existence of (i) a linguistic genotype, which would encompass all the information not derivable from experience which is essential for the acquisition of language and which will, in essence, be necessary for making up a Universal Grammar; and (ii) a linguistic phenotype, which would equate to the grammatical competence finally acquired by each individual. In any case, it is worth bearing in mind that what can be regarded as “innate” necessarily transcends what can be considered “genetic” (cf. Wimsatt 1999), so that, as far as (i) there exist other hereditary mechanisms alternative to genetic codification *stricto sensu*, such as epigenesis or maternal inheritance (Davidson 1986; Wolffe and Matzke 1999); (ii) part of the information which determines the features and functional properties of any biological structure seems to be generated as a consequence of the developmental process

itself (Oyama 2000; Oyama *et al.* 2001); (iii) to some extent, the development (and functioning) of the FL seems to depend on general laws which regulate the self-organization of biological systems (Kauffman 1995, 2000); and (iv) considering the term “innate” should also lead to a critical reappraisal of phenomena such as social learning and culture, which can also be regarded as relevant forms of inheritance in the case of certain behavioural phenotypes (Avital and Jablonka 2000; Jablonka and Lamb 2005). In addition, the term “innate” can point to substantially dissimilar forms of regulation of the development and organization of brain structures, such as those implied by, on the one hand, the Functional System of Language (Lieberman 2000, 2002) or the Faculty of Language in a Broad/Narrow Sense (henceforth, FLB and FLN, respectively) (Hauser *et al.* 2002), or, on the other hand, modularism (Fodor 1983), though when considering the latter possibility, there would also be significant differences between a *pre-wired* brain and a *hard-wired* one (Ramus 2006). These topics will be re-examined and further discussed in Sections 20.4.3, 20.6.1, and 20.6.2.

A second group of evidence concerns the existence of many language impairments characterized by an inherited nature (Tomblin 1989; Bishop and Leonard 2001; for a review see Benítez-Burraco 2009: 83–227). It seems easier (and more enlightening), in order to identify genes related to language, to depart precisely from the analysis of subjects which exhibit an anomalous performance (plausibly as a consequence of the dysfunction of specific components of competence or of the competence as a whole), instead of departing from individuals categorized as normal (as this would demand to discriminate, functionally and structurally, those components when they are intact). And this mainly when there is no sufficient consensus about the actual nature of language, which has been indistinctly (and among many other things) characterized as (i) a component of human behaviour, (ii) a cognitive competence, (iii) a typical behavioral feature of social beings, (iv) an application of social intelligence, or (v) a social and/or a cultural phenomenon (Botha 2000). In fact, there seems not to be an ample consensus even regarding the diverse nativist models which have been improved following those of Chomsky, insofar as, according to the latter paradigm, phenotypes which would deserve to be analyzed could indistinctly be (i) the Universal Grammar *per se*; (ii) some of its components; or, if we abide by the new conception of language which, within Chomskyan thought, represents the Minimalist Model (Chomsky 1995b), (iii) FLN, i.e. what Chomsky himself has pointed out as the main innovation for human language from a biological perspective, namely, recursion (Hauser *et al.* 2002), or (iv) FLB, i.e. all the aspects related to contents to be expressed and interpreted, and also to signals employed in their transmission (Hauser *et al.* 2002). So, in essence, a crucial working hypothesis regarding the characterization of genes related to language has been the following: a

specific language impairment or disorder, whether of an inherited nature, must be a consequence of a gene mutation which, affecting a (specific) component of competence, would nevertheless leave unaffected the other cognitive capacities/abilities (some of which will be involved in performance); on the other hand, a comparative analysis among different linguistic impairments or disorders at the genetic level should allow us to discriminate among (and determine the interaction of) the different genes involved in the regulation of FL development.

20.2 Methodological issues concerning the identification and characterization of genes related to language

20.2.1 Which phenotype do we start from?

The experimental analysis of competence, dysfunctional or afunctional (but also functional), must necessarily depart from an assessment of performance. A main goal at this level should be a maximization of the linguistic nature of the experimental tests designed to evaluate the linguistic capacities of the subjects and, consequently, to infer the nature of their grammatical knowledge, in order to minimize the effect exerted on performance by the cognitive or pragmatic factors necessarily involved in communication. On the other hand, a proper design of such tests should allow us to discriminate among what we usually categorize as the diverse functions or functional components of competence (phonological processing, syntactic processing, etc.) as they are described and characterized by linguists. As will be discussed in [Section 20.4.2](#), an inappropriate delimitation, characterization and/or categorization of language impairments and disorders at the phenotypic level will crucially condition the relevance and the signification of the results derived from their analysis at the remaining levels of biological complexity, particularly at the genetic level.

Otherwise, the convenience (and necessity) to leave aside an older conception of language impairments and disorders which conceptualized these as discrete categories (and which correlatively saw competence as a cognitive capacity with a bimodal distribution [affected/nonaffected]) becomes progressively more evident. In fact, language impairments seem to be more properly described as concrete intervals within a continuum, conventionally delimited in virtue of certain measures of a statistical nature, and of which those individuals who exhibit a linguistic competence regarded as normal would also be part. What also seems particularly relevant in this context is the fact that a characterization of such impairments as bimodal variables significantly simplifies their genuine genetic (and molecular) nature, suggesting that only a few principal genes are involved, and that these, when mutated, give rise to the condition. In

fact, the number of candidate genes for different language impairments and disorders, of genes which can be considered risk factors in their appearance, and of *loci* (i.e. physical places on the chromosome) which can be linked or associated to them have exponentially increased with time (Smith 2007; Benítez-Burraco 2009: 240–281). On the other hand, such a categorization minimizes the foremost role played, in the regulation of the development of the neural substrate of linguistic competence of the people affected, by the complex way in which, as will be discussed in [Section 20.6](#), a multiplicity of genetic, epigenetic, ontogenetic, and environmental factors interacts (of which only some will be dysfunctional or afunctional) during ontogeny (for a review concerning the FL per se, see Benítez-Burraco 2009: 355–364). A characterization of language impairments and disorders (but also of the linguistic competence of the non-affected population) as continuous variables is also at the core of the use of the so-called QTLs (quantitative trait loci) for the genetic analysis of this kind of conditions (see also [Section 20.2.4](#)).

20.2.2 Heritability of language impairments

Different methodological tools allow us to evaluate the extent to which genetic factors contribute to the emergence of a particular dysfunctional or afunctional cognitive phenotype. Among others, the following deserve to be mentioned: (i) studies of familial aggregation, which determine the incidence of the impairment among the affected subject's relatives (though it must also be kept in mind that such analyses do not allow us to discriminate the aggregation caused by cultural transmission); (ii) analyses of identical and non-identical twins, which evaluate the incidence of the impairment among individuals of equal or different genetic background raised in similar family environments; (iii) studies of adopted individuals, which settle on the incidence of the condition among individuals of identical genetic background but raised in different milieus, in order to discriminate the hypothetical effect exerted by the (familial) environment on the emergence of the trait; or (iv) studies of kinship, which analyze the pattern followed by the segregation of the phenotype, in order to establish how the impairment is transmitted (Stromswold 1998; Bishop 2001a, 2001b; Stromswold 2001).

20.2.3 Cloning strategies

20.2.3.1 Basic characterization

Once the hereditability of the phenotype to be analyzed is properly attested, a foremost and particularly productive tool for identifying the gene (or genes) presumably affected is *positional cloning*, which allows us to correlate, in the absence of significant evidence about the aetiology of

the disorder, the phenotype with a particular chromosomal fragment. In order to properly identify and delimit this chromosomal fragment it is necessary to find out the cohereditability of the trait with a suitable number (in statistical terms) of polymorphic genetic markers (usually, SNPs [*single nucleotide polymorphisms*]), whose position along each chromosome is previously known. The number of markers to be considered depends on whether the kin relationships among the subjects which integrate the experimental group are known or unknown. If the former is the case, just around four hundred highly polymorphic markers could be enough to properly identify chromosomal regions between 10 and 30 Mb (1Mb = 1,000,000 DNA bases), though it is evident that such small fragments will plausibly still contain several dozens of genes (Francks *et al.* 2002). Anyway, in such circumstances the analysis is conventionally known as *linkage analysis*. When phylogenetic relationships among the individuals which constitute the experimental groups are unknown and/or suitable lineages (i.e. pedigrees which not only encompass affected people but which are also large enough to obtain statistically significant results) are not available, it is necessary to substantially increase the number of genetic markers in order to properly delimit the region of interest, to the point even of multiplying by six or seven hundred the number of markers employed in linkage analyses (Cardon and Bell 2001). Alternative studies of this kind are known as *association analyses*.

Once the relevant region is conveniently delimited, it is necessary to generate a population of overlapping clones; such clones should be sequenced to settle on the identity and nature of the gene (or genes) they comprise (Brzustowicz 1998). A crucial step here is the in-depth (and suitable) computational analysis of the sequences, and particularly, the comparison with sequences of genes of known function. This not only allows us to determine the presence of putative genes, but also to obtain relevant information about their structural features, and also about the nature and function of the biochemical products encoded by them. Candidate genes will then be appropriately analyzed in functional terms.

When the impaired phenotype specifically entails the atypical presence, accumulation, or degradation of one particular biochemical product, and the identity and the biological activity of the enzyme involved in its biosynthesis or catabolism are known, the so-called *functional cloning* significantly simplifies the identification of the gene presumably mutated in the disorder. This methodological strategy basically consists of a screening of different expression libraries (if the sequence(s) of the gene(s) which encode(s) the corresponding enzyme(s) in other organism(s) is/are also unknown) or different cDNA or gDNA human libraries using heterologous probes (whenever such information is accessible) (Brzustowicz 1998).

Similarly, when information about the genetic basis of akin disorders in other species is available – i.e. disorders (i) whose structural and/or functional features at the neural level are similar to those described in humans, (ii) whose phenotypic characteristics at the cognitive level properly resemble those observed in our own species, and/or (iii) which affect any brain region which can be accurately regarded as homologous to any of the areas which make up the neural substrate of the FL – *comparative cloning*, an alternative cloning strategy, also simplifies the identification of the affected gene(s). In this case, the cloning process demands the employment of heterologous probes (using highly homologous DNA fragments) for the screening of human gDNA or cDNA libraries (Brzustowicz 1998).

20.2.3.2 Implementation

To some extent, the aforementioned tools designed for the identification and cloning of genes related to language have been simplified (or complemented) thanks to the following circumstances:

- (i) the fact that genes which can be linked or associated to language (or to any of its dysfunctional or afunctional phenotypes) seem to be not randomly localized in the genome, plausibly because a non-arbitrary distribution allows for a coordinated transcriptional activation of (many of) them. So, in particular, the density of such genes reveals as notably high in certain regions of chromosome 7 (Ramsay 2000; Benítez-Burraco 2009: 197–205)
- (ii) the analysis of probands whose karyotype exhibits significant evidences of chromosomal rearrangements. A detailed analysis of the breaking points allows us to easily establish if a particular functional gene has been affected. The analytical yield of this kind of surveys has been crucially enhanced by the recent improvement of new tools with an increased resolution capacity. Fluorescence *in situ* hybridization (FISH) is a leading example, as it can detect translocation events between chromosomal fragments of just 100 kb (1 kb = 1,000 DNA bases) (Stankiewicz and Lupski 2006; Volpi and Bridger 2008).
- (iii) the recent development of the so-called *genome-wide association studies* (GWASs); as these studies make use of the whole genome, they offer two major advantages (Zondervan and Cardon 2007; Potkin *et al.* 2009):
 - (a) they make unnecessary linkage analyses; so the aforementioned gain (but also the difficulty) of having lineages which encompass affected individuals substantially vanishes;
 - (b) they allow us to simultaneously establish the presence and the localization of multiple *loci* of susceptibility to a certain disorder (or related to a particular cognitive capacity), unlike linkage and association analyses, which exclusively focus on just one *locus* (or a few *loci*).
- (iv) the categorization of language impairments as continuous variables, and hence, and as we previously pointed out, the application of the

- former cloning techniques to the identification of QTLs (Lander and Kruglyak 1995; Risch and Merikangas 1996), whose ultimate aim is the detection of multiple genes which exert a relatively small effect on a particular trait (Bishop 2002). QTLs present the additional advantage of allowing the apprehension of the variability detected in (and characteristic of) groups regarded as normal (i.e. nonaffected).
- (v) the employment of endophenotypes as the starting point for linkage and association analyses. An endophenotype can be defined as any quantifiable component of the different levels of biological complexity between linguistic competence (or one particular dysfunction of it) and genes, and their nature can be cognitive, neuroanatomical, neurophysiological, endocrine, or biochemical (Gould and Gottesman 2006). The ultimate (biological) justification for their employment will be fully discussed in [Section 20.4.3](#), but for the moment it will be sufficient to keep in mind that they provide more direct evidence of the genetic foundations of a certain cognitive capacity (or of the causes of a particular cognitive impairment), as they refer to concrete (and more physiological) aspects of brain functioning (Gottesman and Gould 2003). The use of endophenotypes also allows a more justified, rigorous, and productive utilization of animal models when analyzing genes related to language and to disorders which arise as a consequence of a dysfunction of linguistic competence ([Section 20.2.5](#)), because in most cases it is possible to find at all those levels particular components which can properly be regarded as homologous to the ones previously analyzed in humans (Gould and Gottesman 2006, and [Section 20.2.5](#)).

20.2.3.3 Shortcomings/special considerations

In any case, it deserves to be mentioned that the analytical output and yield of the above-mentioned techniques for identifying and characterizing genes related to language is also limited by different factors:

- (i) QTLs do not allow us to fully apprehend the complex interaction among (multiple) genetic, epigenetic, ontogenetic, and environmental factors (of which only some will be dysfunctional or afunctional) which seems to underlie inherited language impairments, as we previously pointed out. The principal reasons for this are that this kind of analysis (a) looks for the identification of merely genetic factors, and so cannot provide any information about the remaining (molecular) factors involved in the emergence of the phenotype; (b) it cannot either properly detect highly polymorphic loci, as it assumes that the phenotypic variation observed in the analyzed population is caused by a reduced number of genetic variants (Cardon and Bell 2001), while this is not always true when considering language impairments and disorders.
- (ii) positional cloning manifests another crucial (and particularly significant) limitation, i.e. it establishes correlations between certain

phenotypes and certain genes whose validity is restricted to concrete populations and particular environmental conditions; so the assumption of a universal validity of such correlations must be avoided (Hofmann 2003).

- (iii) the results obtained when resorting to positional cloning are of a statistical nature; this means that correlations between phenotypes and genes cannot properly be interpreted as compulsory causal relationships between them (Fisher 2006); what is more, such an interpretation would contravene the genuine role which genes play in the regulation of the development of the neural structures implied in language processing (Sections 20.5 and 20.6).
- (iv) when positional cloning departs from a particular endophenotype, it is worth bearing in mind that although each of the identified genes could explain most of the variance of the endophenotype (see Flint and Munafo 2007 for an opposite opinion), it can only explain a minimal percentage of the variance of the whole cognitive capacity (Leboyer *et al.* 1998; Almasy and Blangero 2001). On the other hand, a succeeding identification of genes related to one particular cognitive capacity will always heavily depend on an appropriate selection of the endophenotypes to be later employed in linkage and association analyses.
- (v) although (molecular) tools conventionally used for the genetic analysis of the FL (and particularly of genes and their direct products [ARN and proteins]) have been largely improved in recent years and exhibit a noteworthy analytical yield, this is not the case with tools devoted to the analysis of (i) epigenetic and maternal factors, nor with factors associated to/generated by the developmental process itself; (ii) levels of complexity located between genes (and their products) and brain areas in whose development they play a crucial role (metabolomes, subcellular organelles, brain cells, synaptic activities, and specific brain circuits) (Choudhary and Grant 2004); and (iii) the complex interaction which takes place among all the former factors and levels, which is usually a non-linear interaction.

20.2.4 Structural and functional analysis of cloned genes

As was previously suggested, *in silico* analysis of candidate sequences should be necessarily complemented with functional studies (*in vitro* and *in vivo*), in order to fully and properly characterize the transcriptional and translational profiles of the identified genes, the biochemical properties of the products they encode, and, finally, the physiological role carried out by such products, in a way that makes it feasible to establish how the mutation of the genes contributes to the emergence of the disorder (Gibson and Gruen, 2008). Within this kind of studies a crucial step is usually the searching for, and the analysis of, genes which can be regarded as homologous,

orthologous, and paralogous to the identified human ones (generally from different model organisms, typically rat and mouse, but also songbirds in the case of the FL [see [Section 20.3.1](#)]). This is not only justified from an evolutionary perspective, but also because of the relevance of the analysis of the physiological consequences of their ad hoc modification for productively inferring the physiological role of the corresponding human genes, and hence, the phenotypic effects that their mutation would plausibly cause in human beings (see [Sections 20.2.5](#), [20.3.1](#), and [20.3.2](#)).

Whatever the case may be, once the product of the gene expression is elucidated and properly characterized, it is also important to evaluate the functional value of the different variants (functional, dysfunctional, or afunctional) that have naturally arisen in different human populations, with the aim of trying to establish the pertinent correlations between the variations detected at the structural level (in genetic and protein terms [alleles and protein polymorphisms, respectively]), and those detected at the phenotypic level (in terms of linguistic competence): a leading hypothesis here will be that the former should be related to the latter in causal terms at some level of biological complexity. The main analytical tool for this endeavor is known as *allelic association analysis* ([Wahlsten 1999](#)).

20.2.5 Animal models

At first glace, a genetic analysis of language seems to be necessarily circumscribed to (i) our own species, and (ii) dysfunctional or afunctional phenotypes that have naturally arisen in different human populations. The first contingency is due to the fact that, as Chomsky ([1972b/2006; 1980/2005](#)) (but also many others) has accurately pointed out, there seems to exist a fundamental discontinuity between language and the communicative systems employed by the remaining species (nevertheless, the adoption of the Minimalist Program [[Chomsky 1995b](#)]) has implied a significant inflection of his initial thesis about the formal discontinuity and functional inspecificity of language [[Section 20.6.2](#)]). The second contingency is due to the fact that because of ethical concerns we cannot proceed in such analyses in a reverse way (*reverse genetics*), i.e. deliberately mutating sequences of interest and subsequently establishing and evaluating their consequences at the phenotypic level (and in general, at all the remaining levels of biological complexity of the FL). Nonetheless, the fact that (i) at the cognitive, neurobiological, and molecular levels there exists a genuine continuity between the FL and certain capacities, structures, and genetic sequences, respectively, which are present in other organisms, and which consequently can be properly regarded as homologous to the ones present in human beings; and (ii) it is feasible (and ethically admissible) to deliberately alter such genes or such brain regions in the former species, decidedly justifies the employment of model organisms for analyzing certain language disorders ([Sections 20.3.1](#) and [20.3.2](#), for instance) and the FL as a whole.

As previously discussed, this analytical strategy has become increasingly customary when functional studies must be conducted in order to clarify the physiological role of the products encoded by the candidate genes previously identified due to functional cloning. The main reason for this is again that the possibility of making ad hoc modifications of homologous, paralogous, and orthologous genes (either inactivating them [*knockout*], or altering their expression levels [*knockdown*]) allows a more rigorous and productive analysis of such a role. A main objective here is particularly to clarify if such modifications give rise to neural anomalies similar to those detected in humans (both at the structural and functional levels), and especially if they also give rise to articulatory, perceptive, or cognitive deficits which satisfactorily recapitulate those described in our own species. As will be discussed in [Section 20.3](#), this particular circumstance has been attested for some of the genes which have to date been found related to different language impairments and disorders.

However, although the degree of homology between two different genes (or even between two particular neural structures) can be (quite) precisely established, this is not always the case with behavior, where a presupposed homology can actually be a case of analogy (Wahlsten [1999](#)). As (i) linguistic competence must be properly regarded as a multifactorial trait (at all levels of complexity), and as (ii) interactions holding among all the relevant factors involved turn out to be particularly complex, it seems especially compulsory to develop precise tools for the statistical analysis of the genotype-phenotype correlations; these tools should be powerful enough to accurately detect mild or less perceptible variations in behavior as a result of *knockdown* and *knockout* experiments. Such tools should also be integrated in a methodological framework of multidisciplinary analysis of complex behaviors, a practice increasingly known as *phenomics* (Gerlai [2002](#)). This phenomics would ideally pay attention to the phenotypic consequences of the modification of the expression of a significant number of genes (ideally, the whole genome) (Wahlsten [1999](#)), as GWASs already do.

20.3 Genetics of specific language impairments

In the last few decades many different hereditary syndromes, disorders, conditions, or diseases in which only language seems to be impaired have been identified, characterized, and clinically categorized. Most relevant are the ones subsequently described:

20.3.1 Specific language impairment (SLI)

SLI is regarded to be present in those subjects who exhibit an abnormal process of language acquisition in the absence of non-linguistic causes, such as neurological dysfunctions, cognitive (in a broad sense) or mental

delay, auditory impairments, or an inadequate or insufficient exposure to linguistic stimuli during development, plausibly as a consequence of the socio-educational peculiarities of the milieu in which they have been reared (Bishop and Leonard 2001; Leonard 2002).

Several QTLs linked or associated to diverse varieties of SLI have already been identified, though the most prominent results correspond for the moment to a plausible subtype of the condition which would be caused by mutations in just a single gene, namely FOXP2, which has been regarded as “the language gene” *par excellence*. This gene encodes a transcriptional repressor (Shu *et al.* 2001; Vernes *et al.* 2006) which seems to be involved in the regulation of neural differentiation (and plausibly in establishing the cellular identity and/or function) needed for the correct organization and/or development of cortico-thalamic-striatal circuits associated with motor planning, sequential tasks, and procedural learning (for a review, see Marcus and Fisher 2003; Vargha-Khadem *et al.* 2005; Fisher and Scharff 2009). The recent elucidation of (part of) the regulatory network in which the gene is presumably integrated and the identification of several dozens of its physiological targets have jointly confirmed the relevant role played by this gene in modulating brain development (Spiteri *et al.* 2007; Vernes *et al.* 2007; Konopka *et al.* 2009), as well as the hypothesis that the mutation of some of its targets could give rise to different (in clinical terms) disorders affecting (partially or fully) linguistic competence. It seems to be the case with CNTNAP2 regarding SLI and autism (Vernes *et al.* 2008). This gene encodes a neurexin presumably involved in the stabilization of the location of certain K⁺ channels (Poliak *et al.* 1999), and plausibly also in the regulation of synaptogenesis (Dean *et al.* 2003).

A point mutation or a deletion of FOXP2 give rise to different deficits, which are simultaneously of motor and linguistic nature: (i) a decreased level of lexical knowledge; (ii) a reduced capacity for morphological and morphosyntactic comprehension; (iii) diverse problems concerning morphology during production; (iv) a deficit in the capacity of the verbal component of the working memory for properly storing phonologically relevant information, or even for sequentially articulating elements with phonological value; (v) a decreased capacity (in qualitative and quantitative terms) for retrieving elements from the lexicon; and (vi) a developmental orofacial dyspraxia, or even a spastic dysarthria, which would be accompanied by residual errors during development (Gopnik 1990b; Vargha-Khadem *et al.* 1995; Watkins *et al.* 2002; Vargha-Khadem *et al.* 2005; Shriberg *et al.* 2006). It has been suggested that the nuclear deficit of the disorder associated with the mutation of the gene would be an impairment of the capacity for event sequencing or for procedural learning, which would concurrently affect the planning and the execution of oromotor and cognitive (both general and specifically linguistic) tasks (Watkins *et al.* 2002). This would be in consonance with the localization of the primary pathology in the caudate nucleus, one of the neural structures which make up the basal ganglia,

which seem to play a key role (in certain models of linguistic processing) in the computation of the sequential tasks involved in phonation and syntax (Ullman 2001a; Lieberman 2002).

In the mouse the *knockout* of the *Foxp2*, but also the presence of substitutions identical to the ones detected in affected human subjects, gives rise to structural anomalies which are basically circumscribed to the cerebellum, and which seem to be a consequence of an alteration of the normal pattern of migration and/or maturation of cerebellar neurons (Shu *et al.* 2005), though the degree of affection seems also to be variable (French *et al.* 2007; Fujita *et al.* 2008; Groszer *et al.* 2008). Concerning specifically the physiological anomalies that have arisen, the most conspicuous of these concern an increase of the long-term potentiation (LTP) in Purkinje cells and a decrease of the long-term depression (LTD) in certain neurons of the striatum (Groszer *et al.* 2008), and hence, a concomitant reduction of the long-term plasticity related to learning tasks in which basal ganglia play a relevant role (Yin *et al.* 2006). Significantly, mice who carry the human (non-mutated) sequence exhibit physiological effects substantially opposed to what has been previously described, at least in the case of the latter cerebral structure: the human variant gives rise to (i) a decrease of dopamine levels (it is worth mentioning that a reduction of dopamine levels in the striatum has been specifically correlated with an increase in the speed and accuracy of phonological [but not syntactic] processing [Tettamanti *et al.* 2005]), (ii) an increase of the dendrite length of medium spiny neurons of the striatum, and (iii) an increase of LTP in this region, consequently favoring the synaptic plasticity and plausibly potentiating (some of the) diverse processes involved in motor learning (Enard *et al.* 2009). Finally, and concerning the phenotypic consequences of *Foxp2* mutation (but significantly, also the presence of the human mutated variant), it has been suggested that the most conspicuous feature would be a decrease in the frequency of the ultrasonic vocalizations in the pups (Shu *et al.* 2005). Although this abnormal trait would probably match one of the core deficits in SLI, namely, a deficit in the capacity for discriminating brief, or very close, auditory stimuli (and plausibly certain sound frequencies) (McArthur and Bishop 2001), it could also be secondary to the developmental delay caused by the mutation of the gene (Fisher and Scharff 2009).

On the other hand, *FoxP2 knockdown* in zebra finch, which mainly affects neurons of the X-area of the song circuit (a region homologous to the basal ganglia), significantly shortens the critical period for song learning (giving rise to a praecox “crystallization” of songs) and reduces the accuracy of the imitative processes involved in song learning, concomitantly increasing its variability from an executive point of view (Haesler *et al.* 2007). This phenotypic effect is plausibly in accord with the reduced capacity for repeating words and pseudowords, but also sentences, which is characteristic of affected people carrying a mutation in *FOXP2* (Watkins *et al.* 2002). It has been suggested that the anomalous

phenotype linked in birds to the *knockdown* of the gene would be specifically caused by a deficit in adjusting the sound sequence performed by the bird to the pattern memorized during the learning phase. From a neurophysiological perspective, this deficit would be rooted in a defective integration of glutamatergic and dopaminergic afferences in the spiny neurons of the X-area (Haesler *et al.* 2007). Correlatively, it has been found that *FoxP2* expression in different songbirds is higher during the phase in which song is remodelled (Haesler *et al.* 2004). On the whole, it seems quite plausible that, if *FOXP2* has played a prominent role in the origin of language (as it seems), this role would be linked to quite an extent not only to mechanisms involved in the regulation of the development (and the structural and functional patterning) of certain brain regions, but also to the modulation of neural plasticity related to the learning of motor tasks (Fisher and Scharff 2009; White 2010).

In any case, and given that (i) the nuclear pathology in the variant of SLI linked to the mutation of *FOXP2* seems to correspond to a “sensorimotor impairment” (Watkins *et al.* 2002; Shriberg *et al.* 2006), and (ii) this circumstance would confront the clinical criteria which delimit what can be regarded as SLI (these criteria will certainly exclude subjects exhibiting any type of non-linguistic symptoms or deficits), and since (iii) *FOXP2* sequence seems to be normal in most people affected by SLI, the elucidation of the molecular foundations of other (i.e. “canonical”) variants of SLI has received increased attention in the last few years. Contrary to the variant linked to the mutation of *FOXP2*, these variants seem to be explained as a result of the accumulative effect of diverse (mutated) genes of lesser importance, but not of the prominent effect caused by a small group of main genes (Bishop 2001a). To date, several QTLs linked or associated to SLI have been described (Bartlett *et al.* 2002; SLI Consortium 2002; Fisher *et al.* 2003; SLI Consortium 2004). One of these plausibly corresponds to one of the putative risk factors for SLI, namely, *ATP13A4* gene, which encodes a cation-transporting P₅-type ATPase (Kwasnicka-Crawford *et al.* 2005). Another two promising candidates for SLI are *CMIP*, which seems to modulate neural migration and/or the assembly of synaptic complexes, and *ATP2C2*, which also encodes a cation transporter; both *CMIP* and *ATP2C2* have been associated with the phonological component of the working memory (Newbury *et al.* 2010).

20.3.2 Dyslexia

Dyslexia can be clinically defined as a learning disability which impairs the precise and/or fluid recognition of written words, and which gives rise to a significant reduction in the capacity for word decoding and spelling (Lyon *et al.* 2003). Though the appearance of dyslexia is conditioned by environmental factors, it is also true that this condition exhibits a high heritability (Francks *et al.* 2002), as is also the case for the different

capacities which are at the bottom of reading competence (phonological processing, spelling, or orthographic processing) (DeFries *et al.* 1987; Stevenson *et al.* 1987; Gayan *et al.* 1994). Linkage and association studies have led to the identification of nine loci related to this disorder (DYX1 to DYX9) (Williams and O'Donovan 2006; Gibson and Gruen 2008). Nevertheless, there seem to be many additional loci which confer susceptibility to dyslexia and which would correspond, consequently, to genes which can be regarded as a risk factor for reading disability (Smith 2007).

From three of these loci (DYX1, DYX2 and DYX5) a total of four different genes have been identified and cloned that have been regarded as traditional candidate genes for dyslexia. With the exception of ROBO1, which corresponds to locus DYX5, the remaining genes encode proteins which are involved in the regulation of the radial migration of cortical neurons. This is specifically the case with (i) DYX1C1, which corresponds to locus DYX1 and which encodes a regulatory factor that is presumably integrated in a multiprotein complex (Taipale *et al.* 2003); (ii) DCDC2, which corresponds to locus DYX2 and which encodes a protein with two functional domains which interact with microtubules; the protein could consequently be involved in the regulation of the correct positioning of neurons onto the different cortical layers (Meng *et al.* 2005); and (iii) KIAA0319, which also corresponds to locus DYX2 (the gene is located very close to DCDC2) and which encodes a membrane protein which seems to function *in vivo* as a dimer; the protein would plausibly promote or facilitate the anchoring of neurons to the fibers of the radial glia during neuronal migration (Paracchini *et al.* 2006; Velayos-Baeza *et al.* 2008) (though both DCDC2 and KIAA0319 refer to the same locus, it has been suggested that both genes could affect different components of dyslexia and also that their functional products would physiologically interact at some level [Harold *et al.* 2006; Ludwig *et al.* 2008]).

The improvement and the analysis of animal models of dyslexia have made plausible the hypothesis that a causal link could exist between the mutation of the former genes and the structural and functional anomalies described in the brains of dyslexic subjects. These anomalies would be a consequence of the disruption of the normal pattern of neural migration and interconnection, which would in turn originate the processing deficits which are characteristic of (and which give rise to) the disorder (primarily a dysfunction of the phonological component of the verbal working memory). Accordingly, in rats and mice an induced decrease of mRNA levels (a process known as RNAi, from RNA interference) of the orthologous genes not only gives rise to structural changes which are similar to those observed in dyslexic people (Paracchini *et al.* 2006; Rosen *et al.* 2007; Burbridge *et al.* 2008), but also to auditory and cognitive deficits which significantly resemble those detected in this group (Galaburda *et al.* 2006; Threlkeld *et al.* 2007). Additional evidence which seems to corroborate the aforementioned hypothesis is the fact that subjects who are affected by other

pathologies caused as well by an abnormal pattern of neural migration also exhibit a reduced reading competence: periventricular heterotopia would be a prominent example (Fox *et al.* 1998; Chang *et al.* 2005).

As far as the *ROBO1* is concerned, it seems to encode a protein which seems to be involved in the regulation of axonal growth, particularly of those axons which cross from one brain hemisphere to the other (Hannula-Jouppi *et al.* 2005; McGrath *et al.* 2006), and possibly of nerve fibers which project outside the cerebral cortex or constitute the so-called thalamocortical projections, as has been attested in mice (Bagri *et al.* 2002).

Only quite recently candidate genes for loci *DYX3* and *DYX8* have been proposed. The locus *DYX3* could correspond to *MRPL19*, which encodes one of the protein components of mitochondrial ribosomes, or perhaps to *C20orf3*, which encodes a protein of unknown function (Anthoni *et al.* 2007). Concerning the locus *DYX8*, it contains a gene called *KIAA0319L*, which is homologous to *KIAA0319* and whose mutation has been related to certain endophenotypes of dyslexia in certain individuals (Couto *et al.* 2008). On the other hand, the analysis of different chromosome rearrangements has made feasible the identification of other candidate genes for this condition, or at least genes which can be regarded as risk factors for the disorder in certain subjects or certain populations. One of the most promising is *DIP2A* (Poelmans *et al.* 2009), which seems necessary for a proper development of cognitive processes which depend on hippocampal activity, as is the case with learning and memory (Collingridge and Isaac 2003) and which are impaired in dyslexics (Swanson *et al.* 2006). This gene encodes a protein which plays a key role in the regulation of synaptic plasticity (Yu *et al.* 2001; Collingridge and Isaac 2003), probably acting as a positional cue that determines the direction of growth of certain kinds of axons and consequently, the interconnection pattern displayed by certain groups of neurons (Mukhopadhyay *et al.* 2002).

20.3.3 Speech-sound disorder (SSD)

SSD usually manifests in the form of errors concerning the generation of speech-sounds, caused by impairments of a diverse nature, which compromise speech articulation, phonological processing, and/or linguistic processing, (Shriberg *et al.* 1999).

SSD is a highly heritable trait (Stein *et al.* 2004). One of the loci related to the disorder is 3p12-q13 (Nopola-Hemmi *et al.* 2001), which is preferentially linked to the endophenotype of phonological memory (Stein *et al.* 2004). This locus corresponds to *ROBO1*, which is also related to dyslexia (Hannula-Jouppi *et al.* 2005; McGrath *et al.* 2006; and above). On the other hand, the existence of a noteworthy linkage has been documented as well between SSD and 15q14, concerning particularly three specific endophenotypes of the condition: phonological memory, articulatory capacity, and oromotor function. This locus seems also to be subject to imprinting (Stein

et al. 2006). The relevance of this linkage is increased by the circumstance that 15q11–13 region has additionally been associated with autism (*locus AUTS4*) (Shao *et al.* 2003), and is also duplicated in 1–3% of autistic people (Cook *et al.* 1997; Schroer *et al.* 1998; Filipek *et al.* 2003). Interestingly, in autistics this duplication has been specifically associated to (i) a deficit in phonological conscience, (ii) an impairment in the capacity for reading single words, (iii) different articulatory problems, (iv) a generalized language impairment, and (v) a dyspraxia (Boyar *et al.* 2001). On the other hand, the deletion of this chromosomal region also gives rise to two other different developmental disorders, even though they share with SSD some of its typical articulatory and/or linguistic deficits and are also subject to *imprinting*: (i) Angelman syndrome, which usually appears when the deleted fragment is of maternal origin (Kishino *et al.* 1997) and which is characterized, among other symptoms, by the absence of language, though some affected subjects are able to learn and to emit a few isolated words (Alvares and Downing 1998); and (ii) Prader-Willi syndrome, which is caused by the deletion of the paternal fragment (Magenis *et al.* 1990; Robinson *et al.* 1991), and whose distinctive symptoms include diverse articulatory and oromotor impairments of variable severity (Cassidy *et al.* 1997), which usually go along with a decline of communicative and reading competences (Butler *et al.* 2004).

20.3.4 Other specific language impairments

Other cognitive disorders in which only linguistic competence seems to be impaired have been described and characterized up to now, although their prevalence is considerably inferior compared to the ones previously discussed:

20.3.4.1 Landau-Kleffner syndrome

This syndrome is clinically described as a type of acquired aphasia, in which a regression of receptive and/or expressive linguistic abilities is the most outstanding symptom. The regression is not due to neurological anomalies, an impairment of hearing, or a reduction of cognitive capacities (Ansink *et al.* 1989). Though this disorder is highly heritable, at present no gene has been related to it.

20.3.4.2 Rolandic (or Sylvian) epilepsy and speech dyspraxia

Scheffer *et al.* (1995) were the first to describe this disorder, which is characterized, among other symptoms, by an oromotor apraxia which compromises the correct organization and coordination of movements needed for articulation, although dysarthria is not a common symptom. People affected by the disorder also exhibit mild difficulties for understanding certain linguistic structures, while a cognitive retardation is almost never detected. Moreover, Scheffer *et al.* (1995) have suggested

that this type of epilepsy could correspond to a mild variant of the Landau-Kleffner syndrome, in such a way that these two disorders would represent the ends of a clinical spectrum also comprising the so-called continuous spike-and-wave discharges during slow-sleep syndrome (CSWS) (Roll *et al.* 2006). In some individuals affected by the disorder the SRPX2 appears to be mutated. This gene encodes a protein with three “sushi” motifs and a HYR motif. The SRPX2 protein could be involved in the regulation of the perinatal and postnatal maturation of certain circuits of the cerebral cortex (including those responsible for the control of speech) (Roll *et al.* 2006). A mutation in the SRPX2 gene seems also to give rise to a variant of a different disorder, namely, bilateral perisylvian polymicrogyria, a condition characterized by the occurrence of a disproportionate number of small gyri in the perisylvian cortex and by an abnormal distribution of the cortical layers of this region, whose number becomes reduced to just four (Kuzniecky *et al.* 1993); in this case, dysarthria (or even the absence of spoken language) seems to be a prominent symptom, but also is a mild mental retardation (Kuzniecky *et al.* 1993). Significantly, SRPX2 is a physiological target of FOXP2 (Roll *et al.* 2010).

20.3.4.3 Chromosome 22q13 deletion syndrome

The most noteworthy symptom in this syndrome is a severe delay in language development, which can totally fail to appear, while the incidence of other cognitive impairments is much less evident (Prasad *et al.* 2000). In one of the subjects affected by the disorder, Bonaglia *et al.* (2001) found a reciprocal and balanced translocation between chromosomes 12 and 22, which interrupted the sequence of one of the introns of the gene *FLJ10659* (*DIP13β*), but also one of the exons of *PSAP2* (*SHANK3*), a gene localized in 22q13.3. *DIP13α* is a protein akin to the one encoded by *DIP13β* and seems to regulate the activity of DCC, which is implicated in cellular apoptosis (Liu *et al.* 2002); hence the suggested possibility of protein *DIP13β* to be involved in one of the diverse signal transduction pathways which regulate the cellular cycle. On the other hand, *SHANK3* encodes a structural protein which is mainly localized in the post-synaptic neurons implied in excitatory synapses (Wilson *et al.* 2003); the protein seems to play a key role in the arrangement of the so-called post-synaptic density (PSD) (Banker *et al.* 1974), which is regarded to be crucial for a proper structural and functional organization of the cellular apparatus involved in the reception of neurotransmitters in the post-synaptic neuron (Ziff 1997) and, hence, for the induction of neural plasticity, and in turn for the functionality of cognitive processes in animals (Grant *et al.* 2005; Pocklington *et al.* 2006). The recent identification and characterization of new chromosome rearrangements in which *SHANK3* gene is involved (Durand *et al.* 2007) has confirmed the correctness of the proposed correlation between the dysfunction of the gene and the emergence of language impairments; in these new probands, linguistic

disorders co-occur with autistic spectrum disorders (Durand *et al.* 2007), though it cannot be totally discarded that the mutation of an additional gene also (and perhaps notably) contributes to disturb the normal development/acquisition of competence (Wilson *et al.* 2008).

20.4 (In)specificity of specific language impairments

20.4.1 How specific are actual specific language impairments?

The exhaustive analyses of this kind of disorder at different levels of biological complexity (phenotypic/clinical, cognitive, neuroanatomical, neurophysiological, and genetic) have given rise to an increasing body of evidence which suggests that they cannot be (strictly) specific at all those levels:

- (a) from a clinical perspective, language impairments and disorders are characterized by a heterogeneous nature; moreover, the different categories they represent do not seem to properly apprehend the symptomatic variability which the affected individuals usually exhibit. This circumstance has led to postulation of the existence of different subtypes of each disorder, which are in turn characterized by a more marked dysfunction of one (or some) of the different capacities involved in language processing. In the case of SLI, for instance, there are several subtypes which are ultimately grouped in three different fundamental categories: (i) phonological, (ii) expressive (iii) expressive-receptive (Rapin and Allen 1983; American Psychiatric Association 1994). Nevertheless, what seems particularly noteworthy here is the fact that in many cases the results obtained by the affected subjects in the diverse psycholinguistic tests conventionally used for the diagnosis of the disorder are actually interpretable in terms of the occurrence of different specific language impairments, but also of diverse disorders in which language and other cognitive abilities seem to be simultaneously impaired, consequently suggesting a comorbidity among several of these conditions. So, for instance, dyslexia is frequently comorbid with SLI and SSD (Smith *et al.* 1996; Shriberg *et al.* 1999; Stein *et al.* 2004), but also with the attention deficit-hyperactivity disorder (ADHD) (Purvis and Tannock 1997; Shaywitz 1998). Conversely, SLI seems to be comorbid not only with dyslexia, but also with SSD (Shriberg *et al.* 1999) and autism (Norbury 1995; Tager-Flusberg 2006). This comorbidity plausibly extends to several disorders which onset in childhood and which impact learning and acquisition of specific competencies (reading, language, writing, attention) (Angold *et al.* 1999). Besides, it deserves to be mentioned that in most clinical cases up to now documented in which there is a dysfunction of linguistic competence, this dysfunction affects quite general aspects of language, but not necessarily and

exclusively any of the linguistic functions or grammatical entities which are nuclear for the different theoretical models of language developed by linguists (Newmeyer 1997; Poeppel and Embick 2005).

- (b) at the cognitive level, it has consequently been suggested that in these disorders other (cognitive) abilities besides linguistic competence could be simultaneously impaired, or alternatively, that they could be caused by a broader (cognitive) impairment (Nobre and Plunkett 1997). The first possibility would imply that the typological heterogeneity which usually characterizes specific language impairments and disorders would be due to the fact that the clinical entities they represent should be more properly described in terms of conglomerates of different dysfunctions simultaneously characterized by a common symptomatic profile. Each of these dysfunctions would be caused, in turn, by a dissimilar underlying deficit, in such a way that each of these deficits would increase the probability of suffering from a disorder which can be clinically categorized as a specific language impairment. So, for instance, although the core deficit in dyslexia seems to correspond to a dysfunction of the phonological processing capacity, the condition could also arise as a consequence (or by the contribution) of non-specifically linguistic deficits, such as a cerebellar dysfunction (Nicolson and Fawcett 2006), a deficit in the capacity for visual processing (Lovegrove *et al.* 1980), a dysfunction of the magnocellular pathway (which is involved in the activation and redirection of attentive mechanisms in the upper cortical regions) (Livingstone *et al.* 1991; Stein and Walsh 1997), and/or a deficit in the capacity for processing (and discriminating among) sensorial impulses of acoustic (linguistic or non-linguistic) nature which quickly follow one another (Temple *et al.* 2000). According to the second possibility, language impairments would actually be particular subtypes of a broader (cognitive) disorder, i.e. different clinical manifestations of a broader (cognitive) deficit, in such a way that there would be other subtypes or manifestations of the disorder that would correspond to the conditions we usually describe as comorbid disorders. This could be the case, for instance, with the dysfunction of the phonological memory, which seems to give rise to (or significantly contribute to the appearance of) SLI (Bishop 2002), dyslexia (Shaywitz *et al.* 1998), and SSD (Shriberg *et al.* 1999). The characterization at this level of the variant of SLI linked to the mutation of the FOXP2 constitutes another noteworthy example. It was initially suggested that individuals affected by this mutation showed evidence of solely linguistic deficits, mainly of expressive nature, which would be essentially restricted to an incapacity to properly apply certain morphological rules, both derivative and inflectional (Gopnik and Crago 1991). Nevertheless, it has also been claimed that (i) linguistic deficits in these subjects (which would not actually be circumscribed to the morphosyntactic level, but would also affect the phonological one) would co-occur

with an orofacial dyspraxia (Vargha-Khadem *et al.* 1995) and overall, with diverse oromotor impairments (Watkins *et al.* 2002; Fisher *et al.* 2003); alternatively, that (ii) oromotor and linguistic impairments would co-occur with another (and plausibly broader) cognitive deficit of some kind, as far as the procedural IQ values obtained by the affected individuals are slightly below the values exhibited by the non-affected members of their own family (Vargha-Khadem *et al.* 1995; Fisher *et al.* 2003); or instead, that (iii) affected subjects would really manifest a general deficit in the capacity for event sequencing or for procedural learning, which would concurrently affect both oromotor and cognitive (linguistic or non-linguistic) tasks (Watkins *et al.* 2002a).

- (c) at the neuroanatomical and neurophysiological levels, disorders of this kind usually entail the occurrence of structural and functional anomalies of different types in diverse brain areas. Nevertheless, the affected regions can hardly be regarded as exclusively devoted to linguistic processing, even in the improbable case that these putative “language areas” could be precisely delimited (in the sense of univocally correlating particular brain regions to particular linguistic functions or grammatical components, as they are characterized by Linguistics), since their nature and extension differ (to some extent) (i) according to the nature of the computational tasks to be made; (ii) from one individual to another when confronting the same tasks, but moreover in view of the fact that their precise localization also differs (to some extent) (a) during ontogeny, (b) when learning an L2; (iii) in response to brain lesions of variable extent and localization; (iv) according the subject’s (clinical) profile and history, and in general, (v) according to the linguistic environment in which the individual has grown up (Bates *et al.* 1992; Dehaene *et al.* 1997; Thomas *et al.* 1997; Müller *et al.* 1999; Deacon 2000b). Conversely, it is frequently also the case that the impairment of such regions gives rise in other subjects to different deficits and/or different (including non-linguistic) disorders. For instance, there is a noteworthy corpus of evidence which suggests that basal ganglia are involved in the processing of all kinds of sequential actions, both of motor and cognitive nature (and hence, not only of those related to phonation and syntax), in order to allow the performance of routine tasks, but also their modification in response to environmental changes which demand an alteration of such processes (Marsden and Obeso 1994; Monchi *et al.* 2001), as is conveyed by the fact that they are specifically involved in reward-based associative learning, with the particularity that their response is constructed upon memory and learning (Graybiel 1995). In addition, their dysfunction can be regarded as a main aetiological factor for the appearance of other non-exclusively linguistic conditions, such as Huntington’s disease (Gusella and MacDonald 2006), parkinsonism (Duffy 2005), 3- α -methylglutaconic aciduria (Gibson

et al. 1998), glutaric acidemia I (Merinero *et al.* 1995), or progressive supranuclear palsy (Short *et al.* 2002; Kertesz 2003; Goldman *et al.* 2005). Dyslexia once again represents a particularly illustrative example. At the neurological level this condition is characterized by a underactivation of the two processing subsystems of the dispositive for reading ability which are located in the posterior regions of the left hemisphere. One of them is specifically placed in the ventral portion of the occipito-temporal region and is integrated by different areas of the temporal-medial and occipital-medial gyri (Horwitz *et al.* 1998; Shaywitz *et al.* 1998; Paulesu *et al.* 2001). Nevertheless, this region is also involved in the visual recognition of faces and its dysfunction gives rise as well to a different disorder (in clinical terms) which is known as prosopagnosia (Sorger *et al.* 2007; Dricot *et al.* 2008).

- (d) at the genetic level, the genes identified up to now which can be considered as causal (or risk) factors for the appearance of these specific language impairments and disorders cannot either be labeled as exclusively linguistic, since (i) they are expressed in other brain regions besides those involved in language processing (yet again if it was the case that these regions can properly be delimited [see above]), and even in other body tissues, but also in view of the fact that (ii) their mutation can give rise to more than one different impairment, which can be or not specifically linguistic from a clinical point of view. FOXP2, for instance, is expressed during embryonic development in the lung, the intestine, and the heart besides the brain (Shu *et al.* 2001), while it is also expressed in different adult tissues (Lai *et al.* 2001). The case of ROBO1 is quite similar, as it is expressed not only in the brain, but also in the lung, the skeletal muscle, the heart, and the pancreas (Sundaresan *et al.* 1998), while it is mutation has been associated both with dyslexia (Hannula-Jouppi *et al.* 2005) and SSD (Nopola-Hemmi *et al.* 2001). A final and illustrative example would be the region 11p15.5, which encompasses the DRD4 gene, which encodes the dopamine D4 receptor; although this gene corresponds to one of the loci for dyslexia (DYX7) (Hsiung *et al.* 2004), it is also a candidate gene for ADHD (Eisenberg *et al.* 2000; McCracken *et al.* 2000; Roman *et al.* 2001; Schmidt *et al.* 2001).

Consequently, it seems that many data allow us to put into question the specifically linguistic nature of specific language impairments, in the sense that they cannot always and univocally be correlated with (i) deficits which can be characterized as strictly linguistic, (ii) structural alterations and/or dysfunctions of particular brain regions (exclusively dedicated, in principle, to the processing of linguistic stimuli), or (iii) the mutation of certain genes (that, in principle also, would exclusively affect the development and functioning of such regions). What we actually observe is that (i) these disorders usually entail other (cognitive) dysfunctions besides specifically linguistic impairments and/or (ii) they seem to arise as a result of a broader cognitive

dysfunction; (iii) at the clinical level there seems to exist a (certain) comorbidity among different language (and cognitive) disorders; (iv) these disorders are symptomatically/phenotypically heterogeneous; (v) the same disorder can be caused by (or its emergence can be influenced by) several different underlying deficits; (vi) the same underlying deficit can give rise to diverse linguistic (and cognitive) impairments; (vii) brain regions which seem to be structurally or functionally disturbed in affected people are also involved in computational tasks not directly related to language processing; (viii) the identity and extension of such areas are variable (as it is also in the normal population); (ix) genes which are mutated in individuals who exhibit this kind of impairments and disorders (and which can plausibly be regarded as causal or risk factor for their emergence) are also expressed in other brain regions besides those which present structural and/functional anomalies in affected individuals (and/or in brain regions not related to language processing in healthy people), and/or (x) genes cloned from people affected by specific language impairments are also mutated in other cognitive (i.e. non-specifically linguistic) disorders.

20.4.2 To what extent are we dealing with methodological caveats?

To a certain extent, the aforementioned difficulties for achieving a precise characterization of (specific) language impairments and a distinctive separation (at all biological levels previously discussed) between them and other language impairments – both specific or linked to a broad cognitive dysfunction – are of a methodological nature.

20.4.2.1 Shortcomings concerning the clinical characterization of the disorders

Occasionally tests routinely employed for the diagnosis of individuals who are presumably affected by a specific language impairment actually demand the intervention of other cognitive abilities besides linguistic competence (in fact, it has been suggested that an experimental differentiation between competence and performance might be impossible, due to the obliged implication of other cognitive systems [perhaps not yet characterized] in passing from the former to the latter [Newmeyer 1997]). This can in turn lead us to wrongly conclude that the nature of the disorder is cognitive (in a broad sense) or that the condition is comorbid with other language impairments (and/or other cognitive disorders), since this conclusion would lack biological validity. Similarly, there are usually different tests for the diagnosis of the same disorder; the possibility that such tests are based on dissimilar criteria can lead again to the wrong conclusion that (i) there exist several subtypes of the disorder; (ii) the disorder is caused by two (or more) underlying deficits, and/or (iii) the disorder is comorbid with other language impairments (and/or other cognitive disorders). Thus, Bishop *et al.* (1995) have suggested, for instance, that the phonological,

expressive, and expressive-receptive subtypes of SLI would probably not correspond to different conditions.

20.4.2.2 Shortcomings concerning the categorization of the disorders

The analysis of the performance of individuals affected by different specific language impairments (but also of the performance of the healthy population) frequently implies the homogenization of the results obtained by these affected individuals in the different psychometric tests and the subsequent establishment of the different groups which represent the clinical categories conventionally used in the diagnosis (in a basic sense, subjects will be classified in two categories: affected and non-affected). Although both the homogenization and the adscription processes follow properly normalized statistical procedures, the possibility cannot be discarded that (i) the boundaries which separate such groups (and particularly, the limit which divides the affected phenotype from the phenotype regarded as normal) lack any biological entity and/or significance; or (ii) a variability which possesses a real biological basis is rejected and, as a result, (iii) the neurobiological (and hence, genetic) nature of the disorder gets simplified, as a consequence of being treated as a category with a bimodal distribution (normal/anomalous), but not as a dimensional category. The case of dyslexia is again revealed as particularly illustrative in this sense (cf. Shaywitz *et al.* 2008).

20.4.2.3 Shortcomings concerning the analysis of the neural substrate

The way in which individuals are characterized (and categorized) in clinical (i.e. linguistic) terms on the basis of the tests customarily employed for evaluating performance (once again: these tests can be diverse and/or inadequately designed) necessarily determines (and limits) the actual relevance of the subsequent neuroanatomical analyses, because such studies primarily consist in establishing correlations between the observed lesions or anomalies, and the linguistic deficits previously detected (which will be, in principle, distinctive of the disorder). Moreover, these tests are also frequently the basis for diverse sorts of neurofunctional analyses. Consequently, if their solving demands the intervention of other cognitive capacities besides linguistic competence, the detected neural activity will be a summation of different computation processes of dissimilar nature. This circumstance would lead us to wrongly conclude that the brain areas implicated in language processing are multifunctional and/or that certain structures are involved when this is not actually the case.

At this level a relevant problem, also linked to the former, is always the particular way in which we correlate the structural anomalies shown by the different neuroimaging techniques (but especially the visual representations corresponding to the computational tasks specifically demanded by the experimental tests used in functional studies) with the diverse linguistic (dys)functions or (dys)functional components of language.

At the same time it is also not clear how such functions (which we tend to interpret as homogeneous entities and to isolate from other functions of dissimilar nature) arise from the coordination of multiple computational processes which occur at the neural level within a system which we cannot emphatically state is discrete in functional terms (Kosik 2003). If we rigorously consider what we know at present about these topics, we will be compelled to conclude that a definition, in functional terms alone, of the brain areas so identified represents an evident simplification of the state of the art, particularly if we take into account what these areas actually do *in vivo* (Kosik 2003). Broca's area constitutes an especially illustrative example (mainly in light of the latter discussion), since not only during the nineteenth century, but even in some quite recent neurolinguistic models (cf. Geschwind 1970), it has been consistently regarded as the "area for syntax." On the contrary, however, this area (i) seems to play a more restricted role during language processing, which would be limited to (a) the processing of phonetic features with phonological value during reception (Blumstein 1998; Martin *et al.* 1999) and generation of words (Desmond and Fiez 1998; Uylings *et al.* 1999; Kaan and Stowe 2002), being one of the constituents of the phonological component of the verbal working memory; but also to (b) the application of the so-called transformational rules during sentence comprehension, as well as the computation of certain aspects of the elaboration of the syntactic hierarchy during sentence production (Grodzinsky 2000); and (ii) it would also play a relevant role in non-linguistic tasks, mainly in motor tasks essentially related to the perception of others' actions (Schubotz and Fiebach 2006; Fadiga *et al.* 2009). This topic will be examined in a broader context in Section 20.6.1.

Finally, it is worth keeping in mind the fact that the resolution of most non-invasive neuroimaging techniques conventionally used for this type of structural and functional analyses never extends beyond 0.1 mm, even in the most highly resolved examples (Koizumi 2004). This circumstance can lead again to a wrong endorsement of a multifunctional nature to a particular brain area, while it would in fact be the case that there is some kind of histological dissociation among different (but very closely located) neural populations which process different types of specific stimuli.

20.4.2.4 Shortcomings concerning the analysis of the genetic (and molecular) analysis

At the genetic level, all the shortcomings previously discussed concerning (i) the nature and the (proper) design of the psychometric test routinely employed for the diagnosis and the evaluation of the affected subjects, and (ii) the way in which the disorders are usually categorized (and subcategorized) are again highly relevant. One main reason for this is that the identification of *loci* linked or associated to the diverse conditions (and ultimately, of genes which can be regarded as candidate or risk factors in their emergence) normally start from individuals assigned to such

categories (or subcategories) in virtue of this kind of test. Linkage or association analyses will establish statistically significant correlations between certain chromosomal fragments and certain phenotypes independently of how (and how precisely) such phenotypes are defined, categorized, and delimited. An imprecise definition, categorization, or delimitation of one particular disorder (in the sense previously discussed) would give rise to results that could be interpreted as indicative of the involvement of the same gene in the emergence of more than one different impairment, while this common aetiology can disappear when linkage or association studies start off from more biological aspects or components of brain functioning, like those represented by the endophenotypes. To this shortcoming should be added the caveats associated with the conventional tools for the genetic (and molecular) analysis of language disorders which were referred to in [Section 20.2.3](#). As a general rule, and concerning the emergence of a dysfunctional phenotype, these tools are still not able to satisfactorily discriminate the effects exerted on the trait by (i) the epigenetic, ontogenetic, and environmental factors also involved, nor by (ii) the complex interaction that characteristically takes place among the diverse genetic factors implicated, both functional and afunctional/dysfunctional. Accordingly, the genuine molecular nature of the disorder can become simplified, sometimes contributing to establish causal links between certain genes and certain language disorders that possibly would not be real, nor surely, of a universal validity.

20.4.3 To what extent are we dealing with the genuine biological nature of language?

Putting aside the relevant consequences due to the significant methodological/technical shortcomings previously discussed, the genetic (but also the neurobiological, cognitive, and linguistic) analysis of the specific language impairments seems to suggest that the difficulties which arise when trying to discriminate among these disorders at all these levels of biological complexity, but also to distinctively separate them from other cognitive disorders, can actually be caused by the genuine structure and organization of cognition, but above all by the true nature of the neural substrate of language and the genuine way in which genetic factors (among other molecular factors) contribute to the regulation of its development, as will be discussed in detail in [Section 20.5.4](#).

In fact, the controversy around the specificity or the inspecificity (at all those levels of biological complexity) of this kind of inherited language disorder is basically related to the controversy that also exists around the completeness or incompleteness of the dissociations between cognition and language (but even among the different functional components of language) that hypothetically could be detected and described in certain individuals affected by acquired language impairments. It is also clear that

such controversies ultimately point to the diverse hypotheses which have been proposed concerning the biological nature of both cognition and language, and particularly, the anatomical and functional organization of the brain.

The hypothesis that a (full) dissociation between linguistic competence and the remaining cognitive capacities – or even among the diverse processing capacities which would be responsible for the different grammatical functions and entities related to language, as they are defined and characterized by Linguistics – is feasible (and experimentally discernible and discriminable) and that it is also a consequence of the dysfunction of a particular brain area or the mutation of a particular gene, actually responds to a concrete conception of the nature of cognition and of the way in which the brain is organized, i.e. the model which is usually referred to as strict modularism. According to this view, modules are encapsulated and autonomous computational devices, innately specified, which exhibit a domain specificity and which evolved independently to satisfy particular functions (cf. Fodor 1983; Coltheart 1999) (certainly, at first only peripheral devices were portrayed in this way [see Fodor 1983], but soon after diverse [if not all] higher cognitive capacities were also judged to be modular in that precise sense [see Pinker 1994; Spelke 1998; Sperber 2001, among many others]).

Nevertheless, the idea that the diverse (specific) language impairments arise as the result of the interaction between a defective cognitive capacity (linguistic competence) and the remaining, but intact, cognitive abilities would not take into account either (i) the dynamic followed by the development of the brain during ontogeny (Karmiloff-Smith 1998) or, consequently, (ii) the effect exerted by the experience on the structure and the functioning of the FL, or (iii) the genuine biological nature of the latter. Although this topic will be discussed in a more detailed way in [Sections 20.5.2](#) and [20.6.1](#), we can already put forward, taking into account all we have argued up to this point, that (i) the neural substrate of the FL does not seem to be integrated by neural structures exclusively devoted to language processing (“language areas”), and (ii) nor are those genes which seem to play a relevant role in the regulation of the development of such substrate exclusively devoted to this role (“language genes”). As was the case with brain structures, to define genes in strictly functional terms seems to be insufficient to properly apprehend what they actually do *in vivo* ([Section 20.5.3](#)).

In conclusion, in most cases the real scenario can be described basically as follows:

- (i) the dysfunction of a particular gene related to language (i.e. one which is mutated in some individuals clinically characterized as affected by a particular [specific] language disorder) gives rise to different structural and functional anomalies in several brain areas, anomalies which in turn originate one or more deficits, which will cause different

symptoms that are susceptible of being clinically categorized as dissimilar conditions; sometimes these conditions will be heterogeneous and sometimes they will be comorbid, and they will be exclusively linguistic, exclusively cognitive, or simultaneously linguistic and cognitive in nature;

- (ii) the dysfunction of a particular brain area related to language (i.e. one which presents structural and/or functional anomalies in some of the individuals clinically characterized as affected by a particular [specific] language disorder) gives rise to one or more deficits which can cause different symptoms that are susceptible of being clinically categorized as dissimilar conditions; once again, sometimes these conditions will be heterogeneous and sometimes they will be comorbid, and once again, they will be exclusively linguistic, exclusively cognitive, or else they will be simultaneously linguistic and cognitive in nature.

20.4.4 (Relevant) Implications for a genetic analysis of language

In practical terms, the former discussion has two particularly relevant implications for a genetic analysis of language:

- (i) it seems to legitimate (and also to demand) the analysis of general cognitive (i.e. non-specifically linguistic) disorders if genes involved in the regulation of the development and functioning of the neural devices implicated in language processing are to be identified and characterized. The number of such conditions largely exceeds the quantity of specific language impairments, since a dysfunction of linguistic competence is an occurring (or distinctive) feature in disorders such as autism, Norman-Roberts syndrome, Angelman syndrome, Prader-Willi syndrome, Asperger syndrome, Smith-Fineman-Myers syndrome, certain variants of X-linked alfa-thalassemia, fragile X mental retardation syndrome, Rett syndrome, X-linked West syndrome, MASA syndrome, nonsyndromic X-linked mental retardation, cortical dysplasia-focal epilepsy syndrome, familial hemiplegic migraine (type 2), benign neonatal epilepsy, Cohen syndrome, Alzheimer's disease, neural ceroid lipofuscinosis, dementia with Lewy bodies, progressive supranuclear palsy, hereditary dysphasic disinhibition dementia, frontotemporal dementia, Pick disease, pallidopontonigral degeneration, primary progressive aphasia, corticobasal degeneration, anomia aphasia, progressive nonfluent aphasia, different types of spinocerebellar ataxia (2 and 5), Machado-Joseph disease, hereditary spastic paraparesis (type 4), Mowat-Wilson syndrome, Sotos syndrome, Beckwith-Wiedemann syndrome, 22q11.2 fragment deletion syndrome, Potocki-Lupski syndrome, Smith-Magenis syndrome, Floating-Harbour syndrome, and schizophrenia, as well as diverse metabolic disorders, such as Walker-Warburg syndrome,

galactosemia, Salla disease, Amish infantile epilepsy syndrome, Krabbe disease, Smith-Lemli-Opitz syndrome, sulfocysteinuria, α -methylglutaconic aciduria (type I), Canavan disease, phenylketonuria, mucolipidosis (type IV), Schindler disease (types I, II, and III), Hurler syndrome, Scheie syndrome, mucopolysaccharidosis (type IIIA), β -mannosidosis, leukoencephaly, metachromatic leukodystrophy, glutaric acidemia (type I), Leigh syndrome, or the alteration of thyroid hormone, carnitine, ornithine or glycine metabolisms (for a review see Benítez-Burraco 2009: 88–94, 168–172, and 177–227).

- (ii) it seems to corroborate (and also to vindicate) the convenience of starting off the genetic analysis of language from the endophenotypes. Given that their nature is exactly cognitive, neuroanatomical, neurophysiological, endocrine, or biochemical (see section 20.2.3), they allow for a more biological analysis (but also a more biological characterization) of the way in which brain functions, in comparison with the utilization of the clinical categories which represent the diverse language impairments and disorders (Gottesman and Gould 2003; Gould and Gottesman 2006).

20.5 Genes and language

20.5.1 Structural and functional characterization of genes related to language

It is evident that when also considering those cognitive disorders which are not/cannot be regarded as specifically linguistic, but simultaneously impair linguistic competence (Section 20.4.4), the number of genes related to language that have to date been identified and cloned substantially increases (for a list intended to be exhaustive and which encompasses, in particular, all the genes mentioned below, see Benítez-Burraco, 2009: 177–227). Although these genes are notably heterogeneous, they can still be classified, in a first approach and attending to the biochemical nature and function of the products they encode, in the following categories:

- enzymes involved in basic brain metabolic reactions: *ALDH5A1*, *ATP10A*, *COMT*, *PRODH*, *UBE3A*, and *ZDHHC8*.
- membrane transporters (or proteins associated to them): *APOE*, *ATP1A2*, *ATP13A4*, *CACNA1A*, *CACNA1C*, *CNTNAP2*, *KCNQ2*, *KCNQ3*, *SLC25A12*, *SLC6A4*, and *SPTBN2*.
- structural proteins of the cell or enzymes involved in basic cellular metabolic reactions (including, significantly, those specifically implicated in the regulation of the functioning of the cellular cytoskeleton): *CLN2* (*TPP1*), *CLN3*, *CLN5*, *CLN6*, *COH1*, *CTSD*, *CYLN2*, *FLNA*, *GLO1*, *HD* (*HTT*), *LIS1* (*PAFAH1B1*), *MAPT*, *PSAP2* (*SHANK3*), *RFC2*, *SCT*, *SGCE*, *SPG4*, and *STXA1*.

- proteins implicated in cell-to-cell interactions (including, significantly, those specifically involved in the regulation of adhesion and recognition between neurons): *JPH3*, *KIAA0319*, *L1CAM*, *NLGN3*, *NLGN4* (*NLGN4X*), *PSEN1*, *PSEN2*, and *SRPX2*.
- extracellular signaling proteins: *RELN* and *WNT2*.
- membrane receptors: *CHRNE*, *DRD4*, *GABRB3*, *GRIK2*, *HTR2A*, *MET*, *NOTCH3*, *NTNG1*, *NTRK2*, *RAY1* (*ST7*) , and *ROBO1*.
- proteins integrated in signal transduction pathways (excluding membrane receptors): *AVPR1A*, *CALN1*, *cAMP-GEFII* (*RAPGF4*), *CDKL5*, *DMPK*, *FLJ10659* (*DIPI3β*), *HRAS*, *LIMK1*, *LRRK2*, *MAB21L1*, *MTR1*, *NBEA*, *PRNP*, *PTEN*, and *STIM1*.
- regulators of gene expression (including transcriptional factors, translational regulators, proteins which interact with the DNA and ncRNAs): *APP*, *ARX*, *ATRX*, *BAZ1B*, *DCDC2*, *DISC2*, *DYX1C1*, *EN2*, *FMR1*, *FOXP2*, *GTF2I*, *MADH6* (*SMAD9*), *MECP2*, *NF41*, *NSD1*, *OTX2*, *PSEN1*, *PSEN2*, *RAI1*, *SCA8*, *TCAG_4133353*, *WSCR1*, and *ZFHXB1B*.

On the other hand, these genes can also be classified, in an alternative approach and attending to the physiological process in which their products are involved at the brain level, in the following categories:

- regulators of brain metabolism. This class would encompass all genes whose products are implicated in (i) the homeostasis or the modulation of the physiological activity of certain neurotransmitters (*ALDH5A1*, *CHRNE*, *COMT*, *DRD4*, *GABRB3*, *GRIK*, *HTR2A*, *PRODH*, *SLC6A4*, *SPTBN2*, *ZDHHC8*) or (ii) certain hormones (*AVPR1A*, *NSD1*), or which (iii) exert a direct effect on brain function (*SCT*), or (iv) are involved in the transport of certain substances inside the brain (*APOE*).
- regulators of basic cellular processes. This second class would encompass all genes whose products are involved in (i) cellular interaction and/or adhesion (*KIAA0319*, *MTR1*, *PSEN1*, *PSEN2*, *SGCE*, *STIM1*), (ii) the transport of certain products (*SLC25A12*), (iii) the regulation of inwards and outwards cell vesicle trafficking (*COH1*, *CYLN2*, *HD*, *RAY1* [*ST7*]), (iv) the regulation of organelle morphology and location (*HD* [*HTT*]), (v) general transcriptional activity (*GTF2I*), (vi) general translational activity (*WSCR1*), (vii) mRNA processing (*HD* [*HTT*]), (viii) the interaction among diverse intracellular membrane structures (*JPH3*), (ix) the cell cycle regulation (*ATRX*, *PSEN1*, *PSEN2*), (x) DNA replication and reparation (*RFC2*), or (xi) the stabilization and remodeling of the cellular cytoskeleton (*LIMK1*, *MAPT*, *SPG4*), and consequently in (xii) the regulation of the cell size and shape (*DMPK*) and (xiii) cell movement (*FLNA*).
- regulators of neuron specific cellular processes. This third class would include all genes whose products are implicated in (i) the establishment of the ionic gradients related to neural function (*ATP13A4*, *CACNA1A*, *CACNA1C*) or to (ii) the generation of action potentials involved in

nerve impulse transmission (*ATP1A2*, *CNTNAP2*, *KCNQ2*, *KCNQ3*), but also those concerned with the regulation of (iii) vesicle trafficking out of the Golgi apparatus (*NBEA*), (iv) the functioning of certain neural subcellular organelles (*CLN2* [*TPP1*], *CLN3*, *CLN5*, *CLN6*) or (v) the different steps encompassed by the synapses (*CLN1*, *PSAP2* [*SHANK3*], *SNCA*, *SNCB*), including (vi) cell adhesion processes during synaptogenesis (*NLGN3*, *NLGN4* [*NLGN4X*]), and (vii) the release of the synaptic vesicles in the presynaptic terminal (*STXA1*).

- regulators of neural proliferation and migration. This fourth class would encompass all genes whose products are involved in the regulation of (i) neural proliferation and (ii) the differentiation of neural precursors into mature cells (*PRNP*), but also those implicated in the regulation of (iii) neural migration, either promoting it (*ARX*, *DYX1C1*, *DCDC2*, *LIS1* [*PAFAH1B1*], *ZFHX1B*), or inhibiting it (*RELN*), and/or (iv) the organization of cellular architecture (*PTEN* and possibly also *RAI1*).
- regulators of synaptogenesis and axonogenesis. This fifth class would include all those genes whose products are implicated in the regulation of (i) the directionality of axon growth (*NTNG1*, *ROBO1*) or (ii) dendrite proliferation and arborization during development (*L1CAM* and probably also *STXA1*, *CALN1*, *CYLN2* and/or *LIMK1*).
- regulators of neural identity and/or functionality. This sixth class would encompass all genes whose products are involved in the regulation of (i) the brain basic patterning and regionalization (*OTX2*), (ii) neural differentiation (*EN2*, *FOXP2*, *HRAS*, *MAB21L1*), (iii) the determination of cellular fate (*NOTCH3*, *WNT2*), and particularly (iv) (embryonic, perinatal, and postnatal) maturation of certain cerebral circuits (*MET*, *SRPX2*).
- regulators of basic brain processes. This final class would encompass all genes whose products are implicated in the regulation of phenomena such as (i) memory (*NTRK2*), (ii) LTP (*NTRK2*, *UBE3A*), (iii) neural plasticity, and/or (iv) the critical periods for synaptic development in response to experience (*FMR1*, *LIMK1*, *MECP2*).
- It is also worth considering the relevant role played by those genes which encode ncRNAs. These genes seem to be involved in the regulation of the expression of other genes, although the precise biological functions they contribute to have not been yet elucidated. This would be the case with *DISC2*, *SCA8*, and *TCAG_4133353* among others.

On the whole, a high percentage of genes related to language play an eminently modulatory role, as their products belong to pathways involved in signal transduction towards the cell, or else they function as regulators of the expression of other genes. Concerning the physiological functions in which these products are implicated, and leaving aside the aforementioned fact that they are diverse, a noteworthy number of these genes are related to the regulation of neural proliferation and migration, synaptogenesis, and axonogenesis, and the establishment of neural identity and functionality.

20.5.2 Genetic (but not exclusively) regulation of the development of the neural substrate of language

Genes related to language represent one among diverse biological factors (belonging to different levels of complexity) involved in the regulation of the development and functioning of the neural substrate of language (besides the genetic factors themselves, consideration should also be given to those previously mentioned in [Section 20.1](#), namely, epigenetic factors, factors related to maternal inheritance and other systems of innate information storing, ontogenetic factors, and environmental factors). If it is accurate to speak (in a loose sense) of the existence of a developmental program, and particularly of a genetic developmental program (though see Longa [2008] for a critical reappraisal of the concept “genetic program”), it would be convenient and necessary to distinguish between at least two different, but closely related, subprograms. The first one would respond to a considerable extent to a complex genetic regulatory mechanism and would be more important during the first stages of ontogeny, determining the basic histological organization and conformation of the main anatomic macrostructures which constitute the neural substrate of language. This subprogram would be integrated by all the genes previously mentioned (and plausibly by many others not yet discovered and characterized), which could be grouped, as in Ramus ([2006](#)), into three fundamental categories: (i) genes directly related to brain developmental processes; (ii) genes responsible for determining neural identity; and (iii) genes responsible for establishing the spatial-temporal expression pattern of genes belonging to the former two categories. Even so, and because of the genuine relationships between genes and language ([Section 20.5.3](#)), the actual contribution of each of these genes to the final phenotype will, in general, be limited, difficult of prediction, and substantially conditioned to the effects exerted by the remaining involved genes (and also to effects due to the molecular, ontogenetic, and environmental contexts). According to the consequences caused by their mutation, and basically following Winterer and Goldman ([2003](#)), some of these genes could be regarded as principal (i.e. their mutation would constitute a main causal factor in the emergence of a particular disorder), while others should be considered as secondary or modulatory (i.e. their mutation would just represent a risk factor in the appearance of the condition).

Nevertheless, the “instructions” of this first subprogram would determine in such a loose way the interconnection patterns among the neurons which make up the different components of the neural substrate of the FL that it is scarcely probable that a fully operative computational system would actually arise at the term of development (Ramus [2006](#)). Hence the existence (and the importance) of a second subprogram (again in a loose way of the expression), which would be more relevant during

the subsequent stages of brain ontogeny, and of a more local scope. This subprogram would be responsible for the precise determination of the definitive cytoarchitecture of such a neural substrate and, in sum, for the generation of fully operative devices. Unlike the first subprogram, this second one would be an eminently physiological subprogram, given that it would encompass all the structural and functional changes which occur in neurons as the result of the functional interactions which take place among different brain regions, but also between them and the environment. These changes would ultimately occur as a consequence of the effect exerted by synaptic activity on the developing cells, due to diverse physiological mechanisms such as the synthesis and capturing of different growth and chemotactic factors, or the stabilization and destabilization of certain synapses, although many genes must eventually be involved, as is suggested by the analysis of the synaptic proteome (Grant 2003) or of the neural plasticity (Kaufmann and Worley 1999).

20.5.3 The (genuine) relationships between genes and language

The real contribution of the genetic component to the regulation of the development (and functioning) of the neural substrate of the FL can be summarized in the following crucial points (for a more detailed discussion, see Benítez-Burraco 2009: 355–364):

- (i) the role played by genes does not consist in determining language *per se*, but just in synthesizing a particular biochemical product, which will contribute to a certain physiological function;
- (ii) ordinarily, a particular gene will play different roles in diverse moments and places during ontogeny (*pleiotropy*);
- (iii) many genes will contribute (each to a different extent) to the development and functioning of the neural substrate of the FL (*polygenism*);
- (iv) the relevance of the particular roles played by each of the involved genes is clearly surpassed by the effect due to the precise balance kept, in a particular moment and place, by the different biochemical products these genes encode (these products are typically arranged in gradients or specific combinations of signaling molecules);
- (v) gene activity (and the activity of the products encoded by them) will necessarily and decisively be conditioned by the molecular and the ontogenetic contexts, but also by the (linguistic) environment in which development takes place;
- (vi) genes are closely related to (and their expression will be compulsorily modulated by) all the elements and factors belonging to the remaining levels of biological complexity that characterize the FL (molecules, organelles, cells, tissues, circuits, and brain areas, structures and macrostructures);

- (vii) linguistic performance and the context in which communication takes place will modulate the response of the whole system, and consequently the expression of genes themselves; and
- (viii) the final phenotype (functional, afunctional, or dysfunctional) will always arise, in sum, as the result of a non-lineal interaction among the diverse (genetic, epigenetic, ontogenetic, maternal, environmental) factors involved.

If we seriously consider the former points (and in fact, all we have discussed up to now), it is evident that we still cannot regard genes either as a primary cause, or as the final point of a biological analysis of language. On the contrary, they just represent one more among the diverse levels of biological complexity of the FL, with the particularity (previously mentioned) that each of these levels regulates (and is regulated by) the activity of the remaining ones. As Oyama (2000: 40) has correctly pointed out, “a gene initiates a sequence of events only if one chooses to begin analysis at that point.”

20.5.4 The (genuine) relationships between genes and language impairments

The genuine relationships which exist between genes and language also allow us to explain many of the most noteworthy (but also apparently paradoxical) conclusions derived from the analysis of the genotype-phenotype correlations established to date in people affected by different (specific) language impairments and disorders, which can be summarized as follows:

- (i) most genes related to language have been identified in individuals who not only exhibited a (partially or totally) impaired competence, but in whom other cognitive functions were also disturbed;
- (ii) genes that were initially cloned from individuals who only exhibited an impaired competence (but not other cognitive incapacities) are also expressed in brain structures or areas not apparently involved in language processing, and even in different tissues outside the nervous system;
- (iii) in some of the individuals affected by a particular language disorder the sequence of the genes conventionally regarded as candidate (or risk) factors for its emergence is normal (*phenocopy*);
- (iv) some of the individuals who are endowed with an anomalous variant of a particular candidate gene (or of a gene regarded as a risk factor for the emergence of a particular disorder) are not impaired at all (*null penetrance*) or are just mildly impaired (*reduced penetrance*);
- (v) in many cases the same gene has been linked or associated with different language (and/or cognitive) impairments or disorders;

- (vi) for each language disorder the existence of diverse candidate genes and multiple genes that can be considered as risk factors for its emergence seems plausible;
- (vii) the identity of such genes differs (to a certain extent) from one population to another and/or depending on the subtype of the disorder.

This complex outcome can be substantially explained if we assume that:

- (i) in a pleiotropic context the mutation of a particular gene can affect the normal development (and functioning) of two (or more) different brain areas, giving rise to diverse structural and functional anomalies which will in turn originate two (or more) diverse deficits; these deficits will subsequently give rise to different symptoms, susceptible of being clinically categorized as two (or more) dissimilar disorders (sometimes heterogeneous, sometimes comorbid).
- (ii) in a polygenic context it frequently occurs that:
 - (a) the mutation of two (or more) functionally related genes (i.e. genes belonging to the same developmental program) can give rise to basically similar structural and functional anomalies in the same brain area(s), and consequently to an equal deficit and ultimately to similar symptoms susceptible of being clinically categorized as a unique (and the same) language disorder;
 - (b) in other cases, the mutation of two (or more) functionally related genes can give rise to diverse structural and functional anomalies in two (or more) brain areas, which will originate two (or more) different deficits; these deficits will in turn give rise to diverse symptoms, susceptible of being clinically categorized as two (or more) dissimilar disorders (sometimes heterogeneous, sometimes comorbid);
 - (c) the contribution of each dysfunctional or afunctional product to the anomalous phenotype will always be conditioned by the effect exerted by the remaining (and numerous) genes involved in the regulation of the development of a particular brain area; given that this contribution will be necessarily small and scarcely predictable, the mutation of the same gene can give rise to diverse deficits in different individuals and, hence, to dissimilar symptoms, susceptible of being clinically categorized as two (or more) different disorders (sometimes heterogeneous, sometimes comorbid);
 - (d) the contribution of each dysfunctional or afunctional product to the anomalous phenotype will also be conditioned to the effect exerted by the molecular and ontogenetic contexts, by the remaining levels of biological complexity of the FL, and by the linguistic stimuli received during growth; as this contribution will again be subtle and by no means easy to predict, the mutation of

the same gene can give rise, once more, to diverse deficits in different individuals and hence, to dissimilar symptoms, susceptible of being clinically categorized as two (or more) different disorders (sometimes heterogeneous, sometimes comorbid).

20.6 Conclusions

20.6.1 Genes and language

From the genetic (and neurobiological) analysis of the inherited language disorders (but also from the elucidation of the evolutionary itinerary of some of the genes thus far related to the development of the FL; see Chapter 25 in this volume) the following leading conclusions arise:

- (i) to understand language in genetic terms fundamentally means precisely to dissect the particular architecture of the genetic (but not solely genetic) program (in the loose sense of the term) which contributes to regulate the development (and to some extent, the functioning) of certain neural devices in order to allow them to compute the tasks involved in language processing; hence, this endeavor cannot properly be equated to the finding of genes exclusively related to linguistic competence (though maybe there are some);
- (ii) most of these genes belong to our evolutionary lineage; the general architecture of the program they constitute also has a prolonged evolutionary history;
- (iii) to understand language in neural terms fundamentally means to dissect precisely the particular way in which certain neural devices are interconnected in order to allow them to compute the tasks involved in language processing; hence, this endeavor cannot properly be equated to the finding of portions of brain tissue exclusively related to linguistic competence (though it cannot be discarded that maybe there are some such tissues);
- (iv) most of these structures belong to our evolutionary lineage; the general architecture of the program which connects them also has a prolonged evolutionary history;

Consequently, a foremost conclusion, which could seem paradoxical at first glance, is the following: the circumstance that both the particular instructions (= genes) which contribute to regulate the development of the neural substrate of the FL and the particular pieces (= brain circuits/areas) which make up such substrate are substantially shared with other cognitive capacities is nevertheless entirely compatible with the uniqueness and the idiosyncrasy of the program itself (= set of genes) and of the neural substrate itself (= set of interconnected circuits/areas) related to the FL, and therefore entirely compatible with the functional autonomy and specificity which also characterize the FL from a cognitive perspective. Moreover, this is also

fully compatible with the multiple evidence which suggests that the brain is substantially structured at the anatomical level, but particularly so at the functional level, as is shown by (i) the persistence and repeatability of certain neuroanatomical features (Felleman and Van Essen 1991), (ii) the typical discrepancy which exists between the ontogeny of linguistic competence and the developmental itineraries of the remaining cognitive capacities during growth (Crain 1991), or (iii) the existence of substantially similar patterns of brain activation in response to similar tasks related to language processing (for a review see Benítez-Burraco 2005). As Marcus (2006) has pertinently pointed out, although two cognitive modules can be functionally distinct (and in fact, they have to be if they are to be considered modules), they need not be completely independent in genetic, neurobiological, or evolutionary terms (and in fact, they never are). And even though the genetic (and in general, the neurobiological) data do not yet allow us to opt in an absolutely unequivocal way for a particular model of language (and cognition) organization and acquisition, it seems clear that the data we have at present favor the less rigorist of both the functionalist and the modularist proposals and, conceivably, the one rooted in the Chomskyan Minimalist Program, as will be discussed in [Section 20.6.2](#).

Otherwise, a neurobiological characterization of language like the latter, which leaves aside an excessively rigorist conception of the nature of the different modules which constitute human cognition, would match (and simultaneously explain to a large extent) (i) the difficulties mentioned in [Section 20.4](#) for achieving an effective separation between the diverse afunctional or dysfunctional phenotypes of the competence and those corresponding to other cognitive capacities; and (ii) the characteristic discrepancy which can also be adverted during development between the ontogenetic itineraries followed by linguistic competence and the remaining cognitive abilities in people affected by language disorders, a discrepancy that in many cases the correcting therapy is unable to fully revert, as paradigmatically exemplifies the case of dyslexia (Shaywitz *et al.* 1995; Shaywitz and Shaywitz 2005).

20.6.2 Genes and minimalism

Language depiction derived from the Chomskyan Minimalist Program (Chomsky 1995b; Hauser *et al.* 2002) pretty satisfactorily fits the characterization of the FL emerging from the genetic analysis of linguistic competence (and in general, the [neuro]biological nature of the FL), as it has been outlined in the present contribution. This is true on the one hand, because it is still licit to understand the biological idiosyncrasy of the FL, as it was summarized in [Section 20.6.1](#), in terms of domain specificity, which would reside now, as discussed, in the particular way in which the diverse components related to it interact at the different levels of biological complexity, and particularly, in the precise architecture of the genetic program (once

again, in the loose sense of the term) involved in the regulation of the development of its neural substrate and also in the particular interconnection pattern which gives (structural and functional) cohesion to that substrate. And it is true on the other hand because it is surely a conception of the FL which also fits more accurately than others (even the preminimalist Chomskyan conception) the recent findings concerning the way in which evolutionary and developmental processes take place in living beings (with regard to which the FL should not represent any exception, notwithstanding its significant idiosyncrasy at the phenotypic level).

According to Lorenzo (2001: 140) we would be dealing in particular with an “internalist functionalism,” in view of the fact that the features of the FL would now fundamentally depend on (and also be a consequence of an evolutionary perspective) “the settlement of the faculty [of language] in the global architecture of the human mind” (Lorenzo 2004) or, in an equivalent formulation, on “the adjustment of multiple tensions in occasion of the integration of previously autonomous faculties into a system which puts them all in reciprocal communication and confers on them new and more complex functions” (Lorenzo 2004). The hypothesis that the FL represents an interface between a conceptual system responsible for thought and the sensorimotor systems involved in its externalization and internalization, with the particularity that the coupling between such systems (usually known as “external”) would be mandatory during development whenever growth takes place in the presence of a threshold amount of linguistic stimuli (Hauser *et al.* 2002), seems particularly relevant in the context of any genetic analysis of language. A main reason for this is that, as Lorenzo and Longa (2003) correctly point out, it implies a very noteworthy reduction of the genetic charge necessary for the ontogenetic (but crucially, also for the phylogenetic) development of the FL (and concomitantly, a pertinent reformulation of the innate nature of the latter). Different reasons seem to justify this:

- (i) a significant part of the genetic information needed for the constitution of the linguistic functional system would now correspond to the genetic information needed for the development and functioning of the “external” systems. This would in turn imply that a considerable percentage of the genes related to language, as they have been characterized in the present piece of work, and also a significant fraction of the genetic component of the developmental program related to the FL, as it was outlined in Section 20.5.2, would not be now directly related to the FLN, though their alteration would necessarily affect the language ability *per se*, i.e. the FLB;
- (ii) the development and the functioning of the FL would now depend to a certain extent on the general laws which regulate the organization of biological systems (also known as “the third factor”), which are independent from the environment and from the genome (Chomsky 2001: 1-2; 2005); and

- (iii) a considerable amount of the genetic information needed for the development of the FL would now be epigenetic, i.e. it would be biologically determined by the features of the ontogenetic environment in which development takes place (Vercelli 2009).

20.6.3 Concluding remarks: future perspectives

The feasibility of fractioning language into different biological levels and components seemed especially pertinent (and useful) in our challenge for establishing in a more accurate way its structure, nature, and evolutionary origin. Even though genetic/molecular data are still scarce, it nevertheless seems reasonable to venture that a leading hypothesis that will guide future research about these topics will be that the modification of the sequences of a few regulatory genes (and/or their transcriptional profiles) would have allowed to achieve in a brief time span (in evolutionary terms) a noteworthy remodeling of certain neural circuits (or their exaptation) and their being devoted (surely not in an exclusive way) to the processing and computation of symbolic elements, whose result would have been transformed in both externalizable and internalizable structures by motor and perceptive systems previously evolved. The final adjustment of these circuits would depend, thanks to the very organization dynamics of the nervous system, on the external stimuli to which the individual is exposed, but also on the developmental context represented by the remaining nervous structures they necessarily interact with. The outcome is an FL (or a “language organ,” or a linguistic module) which could be regarded as (i) substantially innate (in the extended sense of the term which implies considering innate any kind of information not derivable from experience) and (ii) only partially genetically encoded (hence, and bearing in mind the biological characterization of innateness followed in this work, the epigenetic and maternal information, as well as the information derived from/generated by the developmental process itself, should also be considered as innate), with the additional concern that (iii) the genetic information, in particular, implicated in its development (in ontogenetic, but also in phylogenetic terms) which could be regarded as novel (i.e. specifically linguistic) would be notably lesser than the information *a priori* expected, considering the significant lack of continuity which can be adverted at the phenotypic level between human language and the communication devices of the other animal species: the reason is that such information would largely consist in the information also involved in the regulation of the development of the physiological substrate of the remaining systems/capacities that also integrate the FL(B) – as would be the case, in particular, with those related to thought and the transmission/reception of linguistic signals – these systems/capacities plausibly having a much more long-lasting evolutionary history and responding to much more complex genetic programs.

Part III

Language
evolution

21

The cognitive capacities of non-human primates

Klaus Zuberbühler

21.1 Introduction

An influential way to study the evolution of human attributes is by comparisons with our closest primate relatives, particularly chimpanzees and bonobos. If homologues of attributes are found in all three species, then it is relatively safe to conclude that their common ancestor, which lived some 6 million years ago, already possessed the attribute under investigation. The alternative (the attribute has evolved independently in the different lines) is evolutionarily less probable. This comparative method has been widely applied and with considerable success in understanding the biological roots of human behaviors, including language. Since human language has left no direct traces in the archaeological record, comparative psychology has inevitably come to play a key role in the study of its evolution both in the laboratory and the wild (Fitch 2010). The purpose of this chapter is to review recent developments, with a particular focus on the cognitive abilities of non-human primates, and to discuss the implications for the ongoing debate about the evolution of human language.

What is the nature of human language? According to one intellectual tradition, language is mental activity that generates cognitive images, constructs realities, enables future planning, and allows for relations of symbols (Jacob 1982). A related but distinctly different view is that language is the result of specific cognitive activity but functions primarily as a communication device that enables social interactions with others (Tomasello 1998). Although both positions assume a complex cognitive architecture, only one links language intrinsically to communication. The implications of these two positions for questions about language

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evolution are considerable. If language did not primarily evolve for communication, but for abstract thought, then investigations into the biological roots of language do not need to be concerned with communication in other species. If, however, language is a direct descendant of earlier forms of non-linguistic communication, then primate communication becomes directly relevant. This is not so much because current forms of primate communication are “missing links” of earlier human communication, but because they reveal something about how the common ancestor might have communicated and which aspects of human language are derived and which are more ancestral traits.

The purpose of this chapter is to shed light on the biological basis of mental activity involved in language, by reviewing recent evidence of non-human primate cognition and communication in their natural habitats.

21.2 The evolution of cognition

Compared to other groups of mammals, primates have unusually large brains relative to their body size (Jerison 1973). This has often been taken to suggest that primates may be able to carry out complex cognitive operations. Intelligent behavior has been seen in a wide variety of species, including many non-primates, but primates may be special in that they reveal behavioral flexibility over a wide range of contexts. Cleaner fish, for example, show highly complex social interactions when dealing with each other and their clients, but this may be a specialist ability that has evolved exclusively to boost foraging success (Bshary 2002).

One explanation why primates have such large brains is that this is the evolutionary product of living under conditions of high social complexity, the “social intelligence hypothesis” (Humphrey 1976). Primates tend to live in relatively large groups where membership is individually defined and changes in group composition are restricted to migration of individuals of specific age/sex categories. The typical pattern is that males are expelled from their native groups once they reach adulthood. Female migration is generally less common, although observed in some species, including chimpanzees and humans. The main argument of the social intelligence hypothesis is that, due to social complexity, natural selection has favored individuals who are able to better cope with the social problems generated by group living, provided that socially successful individuals perform better in terms of surviving, mating, reproducing and ensuring their own offspring’s survival. Any cognitive ability or motivation that enhances an individual’s skills towards this end will then be favored by natural selection. Intelligence, in other words, may be the product of an arms’ race in social skills.

Although the social intelligence hypothesis enjoys wide support there are a number of challenges, many of which have not been properly addressed. Strictly taken, the hypothesis predicts a direct relation between

group size, social complexity, and social intelligence. However, many species live in small and socially simple groups with one adult male and a few females; large groups with several adult males and females are the exception across primate species (Smuts *et al.* 1987). Nevertheless, primate species living in small groups do not consistently have smaller brains than expected. Another problem for the social intelligence hypothesis is that it has difficulties in explaining how primates came to evolve such large brains in the first place. Primates are not the only group of mammals that live in individualized social groups, suggesting that natural selection should have favored enhanced encephalization in other social animals as well. Possessing a powerful brain is likely to be advantageous for individuals of any species, so why were primates the chosen ones?

Brain tissue is metabolically expensive to maintain, requiring its owner to provide and secure a constant flow of energy (Armstrong 1983). The evolution of encephalization may thus be constrained by the level at which the required energy can be produced over a lifetime. Since relative brain size is related to basic metabolic rate across species, it has been suggested that larger brains are a reflection of being able to secure high basal energy (Isler and van Schaik 2006). Habitats with large fluctuations in food supply may thus limit how much energy can be devoted to brain tissue, suggesting that primate habitats may have played a key role in brain evolution. Primates tend to specialize on high-energy foods, such as ripe fruits, and species with a higher proportion of fruit in their diet tend to have larger brains than others, regardless of group size (Barton *et al.* 1995). Relative brain enlargements are typically in the visual system, suggesting that encephalization is a direct response to foraging specializations. Primates may have been more successful than other groups of animals in monopolizing habitats with relatively high and reliable food production compared to other groups of animals. Reliable access to high-quality food combined with living in complex societies appears to lie at the core of why humans have evolved such large brains, the organ responsible for language.

What exactly is the primate brain capable of? Cognition is usually defined in terms of how an organism acquires, processes, stores, and acts on information from the environment, thus the focus is on the mechanisms of perception, learning, memory, and decision-making (Shettleworth 1998). Others prefer to restrict the term “cognitive” to only declarative but not procedural mental processes (“knowing that” vs. “knowing how”; McFarland 1991), although the two processes can be difficult to separate empirically. Unsurprisingly, it is even more difficult to be assertive about the kinds of cognitive processes that are pivotal for language. Endorsed by Chomsky (2005), Jacob (1982) has suggested a list of core cognitive activities that collectively make up “language,” cognitive images, future planning, and relating symbols. What is the evidence that some of these are also available to non-human primates?

21.2.1 Future planning

Future planning has been documented in primates, both in the laboratory and the wild. In recent experiments great apes could be brought to select, transport, and save specific tools for a future goal, even if they were prevented from seeing the task during the moment the selection was made (Mulcahy and Call 2006). In the wild, chimpanzees have been observed to transport natural hammers to nut-cracking sites over considerable distances in ways that suggest basic planning (Boesch and Boesch 1984). The hammers were usually large rocks necessary to crack difficult nuts, particularly *Pan troglodytes*. Individuals appear to remember the location of suitable stones throughout their home range, a conclusion reached because chimpanzees transport stones to keep the transport distance minimal. Other research in the wild has shown that monkeys are able to plan their travel paths in a manner that suggest they maintain relatively complex spatial maps of their home ranges, including the location of relevant feeding trees. A typical forest monkey's home range contains as many as 100,000 trees, although only a small fraction is relevant as food sources at any given time (Janmaat *et al.* 2006a). Field research has shown that primates are more efficient in finding the relevant trees than what random search models predict, and this has been taken to suggest that they are able to foresee successful outcomes to some extent (Valero and Byrne 2007). In another example, mangabees approached fruit-bearing trees significantly faster than trees without fruit and they encountered fruit-bearing trees significantly more often than predicted by chance (Janmaat *et al.* 2006a). This is only possible if individuals can maintain and update mental representations of the locations of relevant trees.

What strategies primates use to predict which trees are most likely to contain fruit has also been the subject of research. Gray-cheeked mangabees of Kibale forest, Uganda, were seen to bypass a considerable number of fruit-bearing fig trees in order to revisit trees in which they had successful feeding experiences earlier (Janmaat *et al.* 2006a), suggesting they rely on past memories of individual trees to make foraging decisions about where to find food in the future. Another way to predict the presence of food is to monitor the states of synchronously fruiting species. In one such study, Japanese macaques were artificially provisioned with the fruits of a local vine species. Monkeys responded, not only by inspecting more trees of the same vine species (*Akebia trifoliata*) but also by inspecting trees of another vine species (*Akebia quinata*) (Menzel 1991). Because both species produce fruits simultaneously, the most reasonable explanation is that the monkeys were able to generalize and make predictions about future events from one very specific experience, finding a piece of ripe fruit within their home range prior to the natural fruiting season. A third documented strategy, pursued by primates to predict the location of food in the wild, is to monitor meteorological cues. Temperature and solar radiation have a known impact

on the ripening rates of forest fruits, which has direct consequences on the availability of ripe fruits. In free-ranging gray-cheeked mangabeys it was found that if monkeys revisited a fruit tree, then the average temperature (since the preceding visit) was higher than if they bypassed a fruit tree, suggesting that they had taken average temperature (and its impact on ripening rates) into account when deciding whether or not to revisit a tree in search for fruits (Janmaat *et al.* 2006b).

21.2.2 Mental representations

Thinking is sometimes defined as the ability to go beyond what is perceptually available by maintaining mental representations and making inferences, something that has been demonstrated by captive primates on multiple occasions (e.g. Schmelz *et al.* 2011). In free-ranging Diana monkeys, similarly, adult males produce predator-specific alarm calls that appear to invoke mental representations of the corresponding predator class in other individuals (Zuberbühler *et al.* 1999). In one demonstration, monkeys first heard playbacks of typical predator vocalizations, for instance the shrieks of a crowned eagle or the growls of a leopard to which they responded vigorously with predator-specific alarm calls (Figure 21.1). After this initial experience and following a period of silence, monkeys heard the same stimulus again with the result that, this time, they no longer produced predator-specific alarms. In a test condition, the initial predator calls were replaced by the corresponding alarm calls of a male Diana monkey, so that monkeys first heard eagle alarm calls followed by eagle shrieks. Again, receivers failed to respond to eagle shrieks, despite the fact that the two playback stimuli were acoustically very different. Their only commonality was in terms of their presumed semantic content, i.e. the fact that both calls referred to the presence of an (invisible) eagle. In a final control condition, monkeys first heard the alarm calls of a male monkey, followed by the calls of a *non*-corresponding predator (e.g. eagle alarm calls followed by leopard growls). In this case, receivers responded strongly to both stimuli, presumably because the stimuli referred to different predator types. It is difficult to explain such patterns without invoking some intermediating mental representation of the corresponding predators. The evidence also suggests that primates comprehend some of their own vocalizations as meaningful units in the sense that the calls appear to refer to underlying mental representations of the corresponding external events.

In humans, however, semantic knowledge cannot always be reduced to the physical properties of an external reference, or, expressed in Chomsky's (2005) prose: "even the most elementary concepts of human language do not relate to mind-independent objects by means of some reference-like relation between symbols and identifiable physical features of the external world, as seems to be universal in animal communication systems."

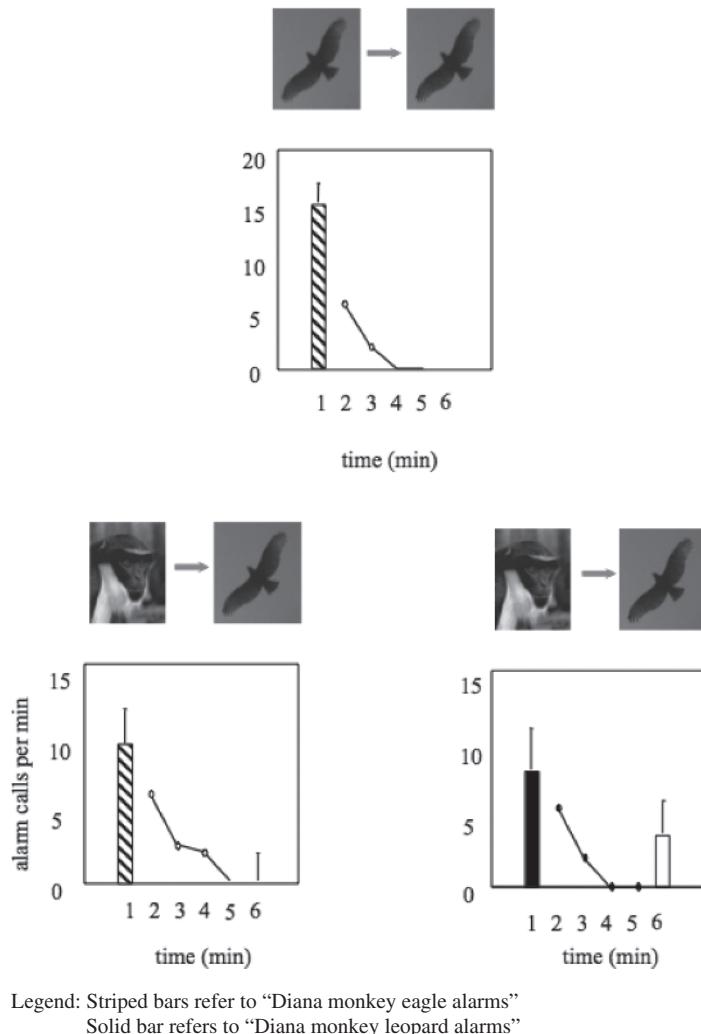


Figure 21.1 Experimental design and results to determine whether Diana monkeys are able to process the semantic properties associated with alarm calls (Zuberbühler *et al.* 1999). The top graph represents the vocal response of the Diana monkey groups to a sequence of two eagle shrieks, separated by five minutes. The bottom left graph represents the vocal response of Diana monkey groups to a male Diana monkey's eagle alarm calls, followed by eagle shrieks five minutes later. The bottom right graph represents the vocal response of Diana monkey groups to a male Diana monkey's leopard alarm calls, followed by eagle shrieks five minutes later.

We sought to address this with a series of studies on tool use in free-ranging chimpanzees (Gruber *et al.* 2009). The basic question was whether non-human primates possess individually acquired conceptual entities that can be activated in context-specific ways, that is, whether they maintain mental concepts that cannot be reduced to the physical properties of the external world. We did this by studying how wild chimpanzees approached a novel foraging problem as a function of their own cultural background. Chimpanzees are by far the most capable tool users in the animal kingdom and they show

community-specific “cultural” differences in the types and methods of tool use (McGrew 1992). To this end, we chose two communities living in different forests in Uganda, the Kanyawara and the Sonso group. Both groups had been studied for more than 20 years, which revealed significant differences in their tool use behavior (Reynolds 2005). For example, Kanyawara chimpanzees use sticks for a variety of foraging problems, while Sonso chimpanzees have never been observed to do so, despite decades of observations. Instead, the Sonso chimpanzees sometimes produce leaf sponges by chewing a bunch leaves into a soft wedge, which then serves as a sponge to extract water from tree holes.

In the experiments, we tested members of both groups with a novel foraging task, liquid honey trapped in a small cavity in a large fallen tree. The hole was made so that it was too small and not shallow enough for the chimpanzees to access the trapped honey with their fingers alone (Figure 21.2 (a)). Natural honey is usually waxy, produced by honeybees that build their hives in vertical tree trucks. We were interested in how members of the two communities attempted to solve this perceptually novel problem, which was identical for both groups. We found that most Sonso chimpanzees tried to access the honey with their hands, although this did not lead to much success. Some, however, proceeded to produce a leaf sponge, which they then inserted into the cavity, typically with good results. At Kanyawara, the response was very different. Here, individuals consistently manufactured sticks from the surrounding vegetation, which they then inserted into the cavity to retrieve the honey (Gruber et al. 2009). Based on the results of this simple experiment, we concluded that chimpanzees do not possess mental concepts that can be reduced to the physical properties of the external world, but that they view the external world from their own individual perspective, determined by their cultural background and prior learning experiences.

In a follow-up study, we provided a multi-functional tool next to the experimental honey-filled cavity (Gruber et al. 2011; Figure 21.2(b)). The tool consisted of a 40 cm branch of a common shrub species with its leaves removed over half of its length. The remaining leaves could be used to produce a leaf sponge but the tool also functioned as a stick supplier or it could be used a brush (by inserting the leafy end to scoop out the honey). Again, we continued to find group-specific differences. No chimpanzee used the provided tool as a brush. Instead, a majority of individuals at Sonso simply ignored the tool. Amongst those that grasped the tool, all proceeded to detach the leaves from the stem, before discarding the stick and producing a leave sponge. At Kanyawara, many individuals seized the tool but all inserted the bare end (not the leafy end) to acquire the honey. Some removed the remaining leaves, while others ignored the tool and instead manufactured a stick from material in the surrounding vegetation.

We have also tried to facilitate stick use in the Sonso community, by already inserting the tool into the honey-filled cavity. Interestingly, no Sonso chimpanzee recognized this as a viable way of extracting honey. Some ignored the inserted stick completely, others touched and retrieved it, but all then discarded the tool. Hence, although the tool’s functional

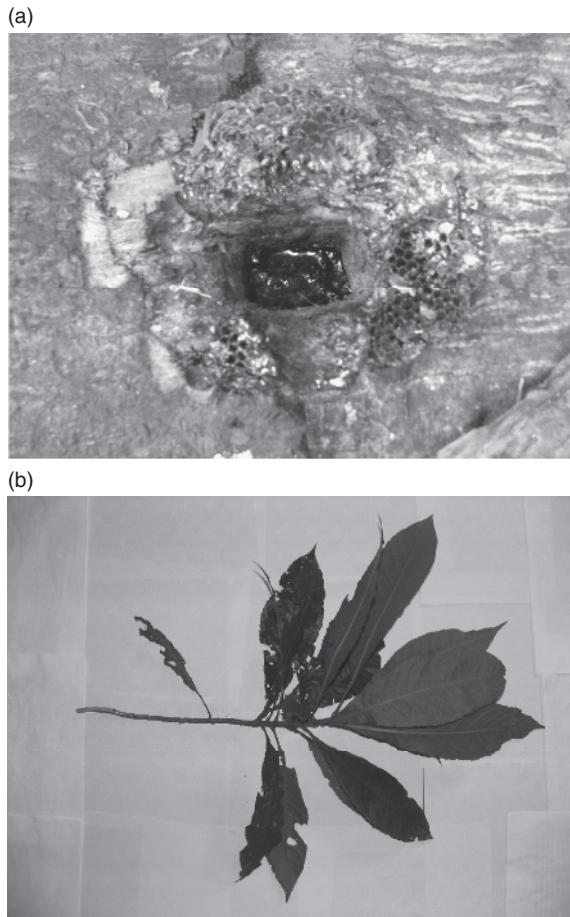


Figure 21.2 (a): Photograph of the artificial beehive, consisting of a cavity with a narrow opening filled with honey beneath the surface (photograph by Thibaud Gruber). (b): Photograph of the “leafy stick,” the multi-functional tool provided to subjects to extract honey in three different ways: as a “stick” (by inserting the bare end), “brush” (by inserting the leafy end), or “leaf sponge,” by removing and chewing its leaves and inserting the resulting sponge (photograph by Thibaud Gruber).

features were presented in a most salient way, no Sonso chimpanzee discovered functional properties of this tool apart from being a supplier of leaves for sponges, a demonstration that chimpanzees perceive their external world, not in terms of “identifiable physical features” but as a result of existing knowledge. This may change the salience of their environments and constrain how they perceive and evaluate it. We concluded that a chimpanzee’s elementary semantic elements are related to the external world in a complex interaction of conception and action.

21.2.3 Symbolic communication

Theories of language usually rely on at least the following elements: an inventory of lexical items, a mechanism to relate these to mental

concepts, and rules to construct an infinite number of structures from them. There is no evidence that any animal communication system fulfills these requirements although some may possess relevant components. For example, in important early work dolphins could be trained to comprehend artificial sentences and sequences of gestures along pre-determined semantic and syntactic rules (Herman *et al.* 1993), while similar results have been obtained from language-trained apes and an African grey parrot (Kako 1999). Although these studies have been hugely influential, some of the implications of their findings have been criticized, on various grounds (Terrace *et al.* 1981). An important weakness with artificial communication systems is that it is usually not clear whether they are of any importance to the animals' natural lives and communication needs, that is, whether there is any evolutionary relevance.

To this end, we have investigated a number of natural communication systems of non-human primates to understand how external events trigger signaling behavior, how they link with underlying mental representations, and how multiple signals are used to construct more complex structures. Concerning the third point, it has long been known that many songbirds produce syntactically organized acoustic structures (Catchpole and Slater 1995/2003). This behavior appears to have evolved in response to sexual selection, an evolutionary force that is the result of clashes in reproductive interests. Sexually selected adaptations are usually in terms of traits that function in advertising territory ownership, attractiveness as mating partners, or intra-sexual competitive ability. The function and meaning of these signals, in other words, are largely in terms of self-promotion. They do not refer to anything external but provide an indicator of the signaler's overall fitness and value.

The degree to which sexual selection has acted on primate cognition and communication is not so well understood. One relevant system has been described in Lar gibbons (Clarke *et al.* 2006). Similar to songbirds, gibbons produce songs as part of their routine daily behavior, a vocal activity that appears to function in the advertisement of territory ownership and pair bonding (Mitani 1992). At the same time, there have been consistent reports that gibbons use songs in response to the presence of predators (Uhde and Sommer 2002), a behavior that is difficult to explain with a sexually selected function. To investigate this, we carried out a field experiment by presenting different predator models to free-ranging groups of Lar gibbons at Khao Yai National Park, Thailand, in order to elicit singing responses. We found that, although the songs to predators and the regular duet songs were made from the same notes, both song types were assembled in different but context-specific ways (Figure 21.3). Results showed that these apes utilize a finite set of acoustic units to construct complex utterances to relate to different events, a relevant finding for questions of how syntactic systems can evolve.

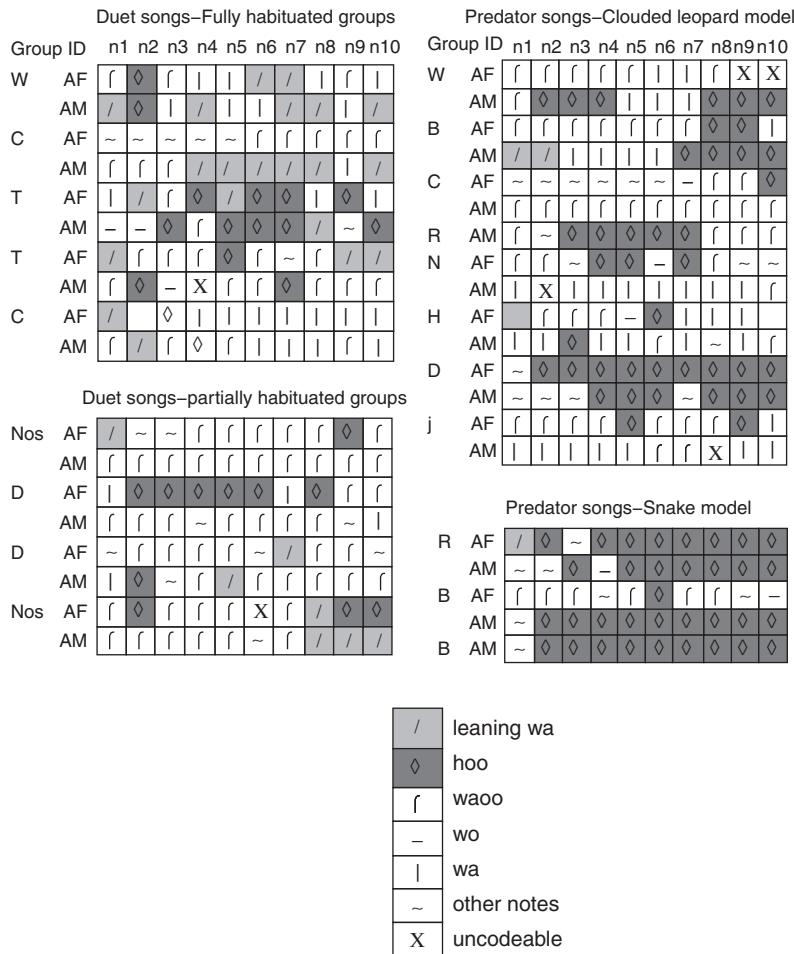


Figure 21.3 Sequential analyses of the first 10 song notes in both predatory and duet song contexts.

Other potentially interesting communication systems, in terms of syntactic features, have been found in forest guenons. In one species, the putty-nosed monkey (*Cercopithecus nictitans*), males produce at least three different call types, “booms,” “hacks,” and “pyows” (Eckardt and Zuberbühler 2004). This primate species is found in forested areas of many parts of sub-Saharan Africa. Typical groups consist of one adult male with several adult females and their offspring, around 20 individuals. The males produce a variety of call sequences usually in response to external disturbances but sometimes also spontaneously, a behavior that has been investigated at Gashaka Gumti National Park, Nigeria. “Booms” were very rare, but “pyows” and “hacks” could be given as part of uniform or transitional series. Sometimes, however, these series were interrupted by “wrong” calls, e.g. a “pyow” series containing a few “hacks.” Closer examination revealed that one particular structure, pyows followed by a

few hacks, was particularly common and often given prior to major group movements (Arnold and Zuberbühler 2006). This suggested that the pyow-hack sequence functioned specifically as a kind of “imperative” to other group members to cease current activities and follow the caller to another part of the home range.

We tested this possibility experimentally, by playing back recordings of leopard growls, a stimulus that reliably triggered vocal responses from putty-nosed monkey males, typically series of “pyows.” As expected, males sometimes also produced “pyow-hack” combinations in response to leopards, which allowed us to investigate the relation between the utterance and its effects on subsequent group movement. Results revealed that, if the male called and produced at least one “pyow-hack” combination as part of his vocal response, then the group traveled significantly further during the subsequent observation period than if the male called but did not produce a “pyow-hack” combination (Arnold and Zuberbühler 2006).

In a follow-up study, we monitored the behavior of individual group members, the presumed target of the males’ communication efforts (Arnold and Zuberbühler 2008). We found that playbacks of the “pyow” or “hack” series did not normally lead to enhanced traveling. Instead subjects stayed where they were and responded with specific anti-predator behaviors and increased vigilance, as if they had anticipated either a crowned eagle or danger on the ground. “Pyow-hack” combinations, however, reliably triggered increased movements towards the presumed caller, and artificially composed “pyow-hack” combinations were as effective as naturally produced ones, suggesting that receivers attended to the overall structure, not just to the individual calls. It was also the case that receivers only responded with increased movement if we played back the calls of their own group male, while the calls of other males were not effective. The “pyow-hack” combination, in other words, conveys a message to other monkeys that is independent of the component calls whose normal function it is to refer to specific external events, such as the presence of specific types of predators.

We then investigated what information exactly was conveyed by the “pyow-hack” combination (Arnold and Zuberbühler 2012). Interestingly, the “pyow-hack” sequence can vary in the total number of calls (ranging from two to seven). Since “pyows” and “hacks” appear to have some own independent meanings (“hack” series refer to crowned eagles; “pyow” series to events on the ground), we were interested in how individual calls contributed to the overall meaning of the sequence. We could think of two main possibilities. First, individual calls might contribute additively with their own semantic content. Playbacks of “hack” series inhibit movements in monkeys, presumably because they are reliable indicators of eagles, a situation in which locomotion is unsafe. Playbacks of “pyows,” on the other hand, tend to trigger some movement towards the caller,

presumably because this utterance is referentially less specific and requires receivers to obtain additional information from the caller. If “pyow” and “hack” calls contribute to “pyow-hack” combinations in such a way, then the prediction is that receivers should move less with increasing numbers of “hacks” in a sequence but move more with increasing numbers of “pyows.” The other possibility is that the monkeys perceive the “pyow-hack” sequences as holistic units, whose meaning is determined by the syntactic structure regardless of the number of components.

We carried out a series of playbacks to test the monkeys’ responses to four distinct sequence types, (a) one “pyow” followed by five “hacks,” (b) three “pyows” followed by three “hacks,” (c) five “pyows” followed by “one hack,” and (d) one “pyow” followed by one “hack.” If the meaning of the sequences is determined by its constituent parts, then sequence (c) should trigger significantly more movement than the other sequences. If the meaning is determined by the syntactic structure, then the number of components should be irrelevant. Results supported the second hypothesis, suggesting that the “pyow-hack” combination functioned as a holistic unit, whose meaning could not be decomposed into or explained by any of its constituent parts (Arnold and Zuberbühler 2012).

Call sequences of the kinds reported from putty-nosed monkeys are not uncommon in forest monkeys, although very little research has been carried out to investigate their function. One exception is the Campbell’s monkey (*Cercopithecus campbelli*). Similar to putty-nosed monkeys, this primate lives in the sub-Saharan forest belt, although its range is restricted to West African forests. Similar to putty-nosed monkeys, males produce three basic call types, “boos,” “krak,” and “hok” (the latter two are acoustically analogous to the putty-nosed monkeys’ “pyow” and “hack”). In addition, callers are able to produce interesting acoustic variation, by means of frequency transitions and adding an acoustically invariable suffix, which leads to a repertoire of six call types (“boom,” “hok,” “hok-oo,” “wak-oo,” “krak,” “krak-oo”; Ouattara *et al.* 2009a). Semantically, we found that suffixation was associated with a change from predator-specific alarm calls to more generalized alert calls (Ouattara *et al.* 2009a). Importantly, males almost always produced these six call types as part of longer sequences. We could identify a large number of different sequences, according to the composition of the different call types. These different sequences were not produced randomly but in relatively clear and context-specific ways (Figure 21.4).

It is currently unknown whether, as receivers, Campbell’s monkeys are able to extract meaning from the different sequences. Some pilot work suggests that this may be the case. In one study, different groups of Diana monkeys were played artificial Campbell’s monkey call sequences. Both species often form mixed species groups, presumably as a way to enhance mutual protection from predation, and there

Sequence composition	Context										N Total	
	Non-predatory			Leopard			Crowned eagle					
	Cohesion &Travel	Tree/Branch	Inter-group	Real	Model	Calls	Real	Model	Calls	Alarm		
1 	13	--	--	--	--	--	--	--	--	--	13	
2 	--	53	--	--	--	--	--	--	--	--	53	
3 	--	--	76	--	--	--	--	--	--	--	76	
4 	--	--	--	3	6	--	--	--	--	--	9	
6 	--	--	--	--	4	8	5	--	--	--	17	
5 	--	--	--	--	--	2	11	--	--	1	4	18
7 	--	--	--	--	--	--	--	--	3	2	5	
8 	--	--	--	--	--	--	--	--	3	2	5	
9 	--	--	--	--	--	--	--	11	10	3	4	28

Figure 21.4 Composition of Campbell's monkey call sequences in different behavioral contexts. "Alarm" indicates leopard or eagle alarm calls given by sympatric Diana monkeys (Quattara *et al.* 2009b).

is good empirical evidence that they understand each others' call sequences to crowned eagles and leopards (Zuberbühler 2000, 2001). In the experiment, Diana monkeys heard series of Campbell's monkeys "hoks" or "kraks," reliable indicators for the presence of a crowned eagle or leopard, respectively. Diana monkeys responded to these call sequences accordingly, i.e. by producing their own predator-specific eagle or leopard alarm call sequences. In the test condition, the same two call series types were used, this time however preceded by a pair of Campbell's monkey "booms," a call type associated with non-predatory disturbances (Figure 21.4). Here, Diana monkeys' responses to the Campbell's "hok" and "krak" sequences were significantly reduced, as if the addition of a "boom" series, prior to these calls, removed their predator-specific semantic content (Zuberbühler 2002). Although this is indicative of an understanding of the syntactic structure of these call combinations, more detailed investigations into the various naturally produced call combinations of Campbell's monkeys are needed.

21.3 Social cognition

Human language tends to take place in a state of high mutual awareness between speakers and receivers; it is grounded in mutual knowledge, beliefs, and assumptions (Clark and Brennan 1991). What degree of social awareness is available to non-human primates during natural acts of communication? One source of heightened social awareness in non-human primates is seen when vulnerable kin are involved. Male blue

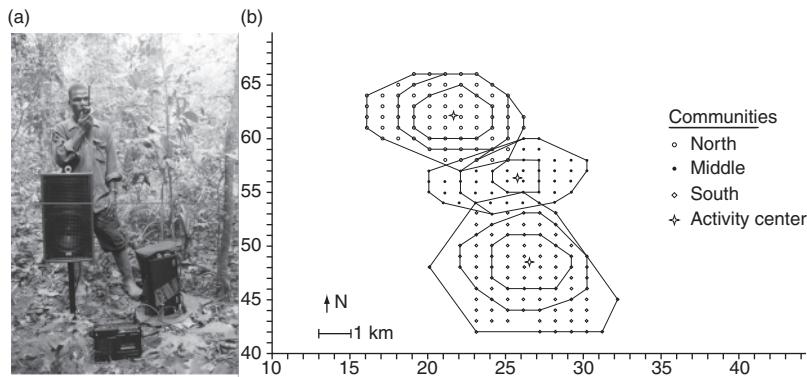


Figure 21.5 (a) A playback experiment is carried out in the Tai Forest, Ivory Coast, to investigate the communication of wild chimpanzees in response to members of their own group as well as of neighboring and stranger groups. (b) Playbacks were carried out in the border areas of the home ranges of three chimpanzee groups habituated to the presence of human observers (Herbinger *et al.* 2001, 2009).

monkeys, for example, produce more alarm calls if their own group is close to a suspected eagle than if they are further away, irrespective of their own position relative to the eagle (Papworth *et al.* 2008). Similarly, male chimpanzees cooperate with each other in situations when other group members are exposed to danger, for example, when having to cross a road that separates part of their habitat (Hockings *et al.* 2006) or during encounters with hostile neighbours.

To investigate how chimpanzees deploy communication signals during cooperative acts we carried out a playback study with wild chimpanzees in the Taï Forest, Ivory Coast (Herbinger *et al.* 2009). Using a powerful playback speaker, we simulated the presence of a neighboring group of chimpanzees from the border area by playing back pant hoot vocalizations of a neighboring group member (Figure 21.5). Subjects responded strongly and in typical ways by producing a range of vocalizations and gestures, which differed from their response to familiar group members (Table 21.1).

The overall response pattern, especially in the gestural domain, revealed that chimpanzees produced a range of affiliative signals (Table 21.1), as if they first intended to build mutual trust in the face of this potentially dangerous situation. Similar effects were found when the calls of unfamiliar strangers were played back, although in this condition subjects tended to remain silent, perhaps because they had no prior knowledge about the strength and potential dangerousness of this suddenly invading group.

Other research on ape vocal behavior has revealed a number of instances where individuals deploy vocalizations in ways that suggest awareness of the calls' potential impact on others. For example, when finding food chimpanzees are more likely to produce food grunts if they are in a

Table 21.1 Communication behavior of chimpanzees in response to the simulated presence of members of their own or other groups (Herbinger *et al.* 2009).

Behavior	Playback condition Member (N=11)	Neighbour (N=22)	Stranger (N=20)
Vocal			
Screams	0.45	0.91	0.65
Pant hoots	0.73	0.05	–
Hoots	0.09	0.05	–
Pant grunts	–	0.05	0.05
Barks	0.27	0.05	0.20
Other calls	0.09	–	–
Gestural			
Drumming	0.18	0.14	0.05
Hunch	0.45	0.77	0.80
Grin	0.27	0.73	0.55
Mount	0.09	0.41	0.25
Embrace	–	0.14	0.15
Kiss	–	–	0.10
Touch	–	0.09	0.05
Finger in mouth	–	0.14	0.05
Display	0.09	0.14	–
Non-gestural			
Groom	0.27	–	0.15
Eat	0.18	0.18	0.20

Relative frequencies of communication signals produced within the first five minutes after a pant hoot vocalization (N trials).

party with individuals with whom they have good grooming relations compared to other individuals (Slocombe *et al.* 2010). In a recent study, chimpanzees were more likely to produce alert calls to a snake if they were in the presence of a naïve audience compared to when others were already aware of the danger (Crockford *et al.* 2012). Inhibitory effects on call production are also common, particularly if the audience is unfavorable. Female chimpanzees in oestrous tend to suppress their copulation calls if they mate with low-ranking males or if they are in a party with many other females (Townsend *et al.* 2008). Chimpanzee females can be similarly inhibited in their pant grunting behavior, a vocal greeting signal, if the audience is unfavorable. The normal pattern is for lower-ranking individuals to produce pant grunts when encountering higher-ranking group members. Observations have shown that if females encounter a higher-ranking male, then the likelihood of them giving a pant grunt to the male is significantly reduced if the alpha male is already present, suggesting that his presence has an inhibitory effect on her willingness to interact socially with other higher-ranking group members (Laporte and Zuberbühler 2010). The composition of the audience also has demonstrated effects on the acoustic structure of some calls.

Chimpanzees that are victims of aggression tend to produce specific vocalizations, the victim scream, acoustically graded signals that reflects the severity of the attack experienced by the caller (Slocombe and Zuberbühler 2007). However, the acoustic structure of victim screams is additionally influenced by the composition of the audience. If high-ranking group members are present who are potentially able to interfere and disperse the aggression, then victims tend to produce screams that are acoustically shifted towards the more severe end, regardless of the nature of the attack, as if to persuade the nearby group members to intervene on their behalf.

Chimpanzees are not the only species in which call production is audience dependent. In bonobos, for example, it was found that females produce copulation calls during sexual interactions with both males and other females (Clay *et al.* 2011). During female–female interactions it is always the lower-ranking one of the two partners that produces the calls and call rates are positively affected by the absolute rank of the partner. Overall, females appear to be particularly keen to advertise successful sexual interactions when her partner is high-ranking.

These recent studies demonstrate that great apes appear to be relatively aware of the social implications of some of their calls and they are able to adjust their production accordingly. It is likely that this ability, to take the impact of one's signals into consideration, is an important precondition to the uniquely human ability to share intentions and ground acts of communication in shared knowledge and mutually experienced mental states.

21.4 Conclusion

The current evidence from studies on non-human primates demonstrates that human language is unlikely to have evolved *de novo* during the last few million years after the human lineage had split from the common primate ancestor. More likely, language is the evolutionary product of a mosaic of adaptations that have related functions in the social and communication abilities of modern primates, which suggests common ancestry. Humans are not alone in their ability to form mental representations, and these can be shaped by cultural experiences. They are also not alone in their ability to associate mental representations with arbitrary communication signals, some of which are syntactically complex and assembled from basic units. Despite such parallels, humans appear to be the only primate species with advanced and voluntary motor control over their vocal apparatus, an ability that enables play with sounds and generation of a potentially infinite number of vocal structures. Humans may also be unique in their social cognitive ability to take into account what others know and assume about the world before they address them.

22

Birdsong for biolinguistics

Kazuo Okanoya

22.1 General Introduction

Throughout the year and all over the world, we can hear various sounds that birds are producing. Among these bird sounds, a class of vocalizations that consist of more than one type of sound pattern arranged in a certain temporal sequence is sometimes called a “birdsong,” not only because of the organization of sound patterns, but also because our musical aesthetics intuitively allow such an analogy (Hartshorne 1973). Scientific investigations to date suggest that certain properties of birdsong extend beyond a musical to a developmental analogy (Marler 2000). More recently, birdsong has been discussed as a useful biological model that might provide clues to the evolutionary origin of human speech (Bolhuis, Okanoya, and Scharff 2010). Here, I first introduce the basics of birdsong science. I then discuss the degree and relevance of behavioral, neural, and evolutionary analogies between birdsong and human speech by supplying examples from the current literature as well as from our own research (Okanoya 2004a).

Humans are singular, but birds are many. Thousands of bird species emit songlike vocalizations (Catchpole and Slater 2003), but we are a single species of primate that talks. When we say “birdsong and human speech,” we are comparing a behavioral property of one species of ape (us) versus that of thousands of birds, which is as nonsensical as a comparison goes. Thus, when we say “a bird” or “a birdsong,” this means a bird or a birdsong in a very general sense, usually biased toward passerine songbirds in temperate or semitropical zones and those species in which only the male sings. In these species, birdsong conveys the singer’s species identity and vigor, and functions in a sexual context. Against conspecific males, birdsong is a sign of territorial defence, while for females, the same song is interpreted as the message for mating (Catchpole and Slater 2003). Compared to birdsong, human speech conveys much more than an

intention to mate, although that is certainly one of the most important functions of human speech (Miller 2000).

The analogy between birdsong and human speech or music was discussed by early philosophers such as Herder (1772) and Rousseau (translated) (Rousseau and Singh 2006), Kant (translated) (Kant, Davids, and Churton 1899), and, of course, Darwin (1871). Furthermore, excellent current reviews have dealt with various aspects of birdsong, and I recommend that readers utilize such reviews to familiarize themselves with topics not addressed in this chapter. For behavioral and developmental comparisons, Doupe and Kuhl (1999) provide a comprehensive overview. For the neural mechanisms of birdsong learning, Ölveczky and Gardner (2011) and Mooney (2009) offer an advanced description, and Hahnloser and Kotowicz (2010) give more technical accounts on auditory representations of song. Margoliash and Nusbaum (2009) give consideration to comparative studies on syntax evolution, especially using birdsong, and Berwick *et al.* (2011) provide an in-depth argument on the level of syntax in birdsong. Finally, Bolhuis *et al.* (2010) make the most up-to-date comparisons between birdsong and human speech, from genes to behavior.

22.2 Basic description

Before going into specific discussions, we need to introduce general descriptions of birdsong in relation to human speech. I selected the following five topics for comparison: behavior, development, syntax, the brain, and genes.

22.2.1 Behavior

I first compare physical features of human speech and birdsong (Marler and Peters 1981). Human speech ranges from 100 Hz to 10–20 kHz and is rich in harmonics, and particular frequency regions are emphasized to form formants. Several transitional components exist, including voiceless sounds, frequency modulations, and click-like elements that are important linguistic features. Birdsongs generally occupy narrower frequency ranges (1–8 kHz), and varieties of acoustic structures range from noise-like to pure-tone song notes. In general, defining “phonemes” in birdsong is not possible, but for each species, basic components of songs are limited to a few acoustic elements. The degree of segmentation is much more pronounced in birdsong than in human speech. Thus, one can define behavioral hierarchy based purely on this morphology.

In the birdsong literature, we often use a sound spectrograph to explain the structure. Here, I define some of the most commonly used terms in birdsong (Figure 22.1) (Catchpole and Slater 2003). A continuous trace in a sonogram is a “note.” Of course, whether the trace is continuous depends

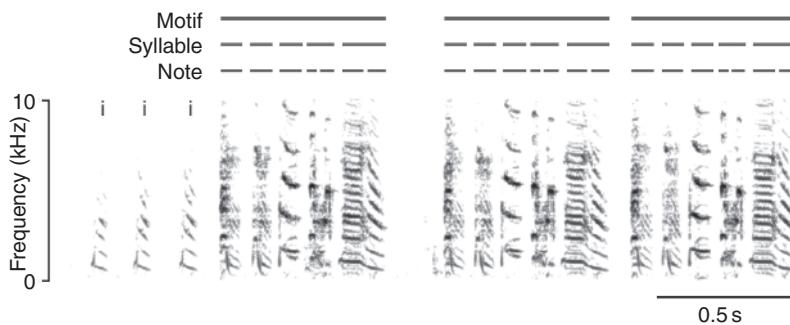


Figure 22.1 Birdsong terminology. See the text for details.

on the gain of the sonogram and whether obvious breaks occur in the birdsong. A set of notes that comprises a unit of repetitions is a “syllable,” and this definition differs from that in human speech, in which syllables refer to consonant and vowel combinations. At this level and beyond, the terms begin to gain ambiguity. A syllable in birdsong could contain one or more song notes, and some researchers never use the term “note.” An aggregate of syllables that contains a subset of the bird’s note repertoire is a “motif.” Some species have multiple motifs, while others have a single motif. The note sequence within a motif may vary due to different numbers of repetitions in each rendition of motif or occasional omission of song notes. These motifs may also be sung in quick succession with a few intervals, forming a song bout. A song bout is separated from another song bout by relatively longer intervals of a few seconds.

22.2.2 Development

In passerines, parrots, and hummingbirds, birdsong is a learned behavior. About 5,000 species exist within these three classes of birds. Among these, differences occur in the degree of learning devoted to song development. In some species, including several parrots and mynas, learning determines most of the vocal morphology. In these species, isolation-reared birds sing highly abnormal songs that cannot function in the mating context. In others, including some warblers and finches, vocal morphology is mostly determined by genetic programming, with little room for learning. Thus, in these species, songs are more or less similar between geographically isolated locations.

Birdsong learning comprises two distinctive phases: the sensory phase and the sensorimotor phase (Doupe and Kuhl 1999). In the sensory phase, auditory learning takes place in which characteristics of conspecific songs are memorized by carefully listening to the proper song. Different degrees of innate predisposition determine which types of songs are remembered. Without a proper model, a bird could learn heterospecific songs, but if

later exposed to a conspecific song, it might relearn the song, depending on when such an opportunity comes. This result indicates that some innate predisposition should be related to the sensory property of the song to be memorized, but that this predisposition can be overridden by external stimuli if the proper stimulation is not available. Through the sensory phase, the bird develops an auditory template that can be used as an internal reference when the bird begins to vocalize during the sensorimotor phase.

The initial vocalizations are usually very premature, consisting of a train of noisy, short elements with many variations. The vocalizations, or the song template, are gradually shaped to reflect the characteristics of the remembered song. Isolation and deafening experiments (Konishi 1965) have shown that auditory feedback of the bird's own vocalizations is important in shaping the song. Through auditory feedback, the auditory template and actual vocalizations are matched; if discrepancies exist between them, the vocal output is modified until it matches the template. These have been highly conceptual descriptions, but neural substrates for these mechanisms are gradually being understood. Following the description in human speech literature, the initial ambiguous vocalizations are termed "bubbling" or a "subsong," and the intermediate vocalizations, which have some structural resemblance to the adult song, are called "plastic songs." The final product of this practice is called a "crystallized song," implying that the song will not change after this stage.

The above description is an idealized case, and in reality, many variations exist. In some species, such as zebra finches, the two phases mostly overlap because they are remembering the song template as they practice it. In other species, such as white-crowned sparrows, these two phases are separated by hundreds of days. This is because in such migratory species, sensory learning occurs at the site at which they were born and reared, but sensorimotor learning occurs at the site at which they reproduce.

22.2.3 Syntax

Birdsong conveys the motivational, nutritional, cultural, and developmental condition of the singer (Nowicki, Peters, and Podos 1998). Each note of birdsong has specific acoustical properties. These song notes are ordered according to rules and are typically referred to as "syntax." However, they are not connected to any referential meanings (Hauser 1996). In this sense, birdsong does not have syntax, but only ordering of the elements. Songs are sung by fully motivated individuals in response to potential mates or rivals (Catchpole and Slater 2003). Singing itself establishes a context for mating or fighting, and the style of singing governs the intensity of the signal. For example, European blackbirds change the arrangement of song notes depending upon how many rivals are singing. The order of song notes becomes increasingly jumbled as the aggressiveness of the singer

increases (Dabelsteen and Pedersen 1990). In this sense, the song is a graded or holistic system of communication (Hauser 1996) rather than a compositional system like language in which different combinations convey different meanings.

Thus, one cannot directly compare the syntax of birdsong with that of human language. Instead, we need to identify a strategy for drawing valid comparisons between the two systems. One possible approach involves restricting the definition of “syntax.” Indeed, formal language theory addresses only the form, not the content, of language (Hopcroft, Motwani, and Ullman 2001). This theory uses the Chomsky hierarchy to characterize different degrees of linguistic complexity. At the lowest end of the hierarchy is regular grammar, in which sequences of language are determined only by local transitional relationships. This class of grammar has been described in terms of finite-state syntaxes, in which state transitions produce certain strings. Context-free grammar, in which the production rule can contain itself and thus enable the recursive production of strings (Hauser, Chomsky, and Fitch 2002), forms the next rung in the hierarchy. Higher classes of grammar have been described, but are beyond the scope of this study.

After it is assigned to the appropriate level of the hierarchy, birdsong “syntax” can be compared to the different structural rules underlying human speech. Analyses of song sequences have suggested that local transition rules adequately describe the order of the structures that contain the elements of birdsong. For example, the sequences of the elements in starling songs were analyzed with the first-order (a to b), second-order (ab to c), and third-order (abc to d) transitional matrices, and the resulting entropy states were compared (Gentner 2007). Results indicated that the first-order analyses provided full descriptions of the starling songs. The same analyses have been performed in Bengalese finches; the second- or third-order matrix was necessary for a full description, but no self-embedding structures were found (Okanoya 2004b). Thus, finite-state syntax, a level of syntax that can be expressed by local transition rules alone, is an appropriate level of description for birdsong because no evidence of recursive structures has been identified with regard to this phenomenon.

22.2.4 Brains

Birds and humans are distantly related, but the basic vertebrate brain architecture remains unchanged between these two groups of animals (Ulinski 1983). However, unlike the mammalian brain, the bird brain does not have folds on the surfaces of the cerebral hemispheres. This gives the impression that the bird brain is very primitive compared to the mammalian brain. In fact, the basic plans of the mammalian and avian forebrains are quite different, but one cannot say that one is more

primitive than the other. In mammals, layers of neurons exist in the pallial structure of the forebrain, i.e. the cortex. In birds, however, the forebrain does not have a pallial structure, but instead has a compartment structure. Nevertheless, we can discuss the correspondence between avian and mammalian brains based on connectivity with other areas, gene expression profiles, and biochemical properties. Conceptually speaking, each layer in the mammalian cerebral cortex roughly corresponds to each compartment in the avian forebrain (Reiner *et al.* 2004).

In the human brain, the so-called “language areas” reside between the motor and perceptual systems (Petersen *et al.* 1989). Broca’s area is in the interface between the motor area (frontal cortex) and the association area (prefrontal cortex). Wernicke’s area is in the very caudal part of the auditory area, close to areas related to vision and spatial cognition. Broca’s area controls sequential aspects of language and motor output, while Wernicke’s area controls semantic aspects of language and auditory or visual perception. In the bird brain, the “song system” also resides between the motor and perceptual areas (Bolhuis *et al.* 2010). The higher vocal centre (HVC) is directly connected to the robust nucleus of the archipallium (RA), the motor cortex equivalent to a bird’s vocal control (Figure 22.2). The caudomedial nidopallium (NCM) and caudomedial mesopallium (CMM) are considered to be the memory stores for learned song and are located close to the primary auditory area in the bird brain. Thus, the topography of the HVC and NCM/CMM are analogous to that of Broca’s area and Wernicke’s area. Several direct and indirect connections exist between the HVC and NCM/CMM in the avian brain.

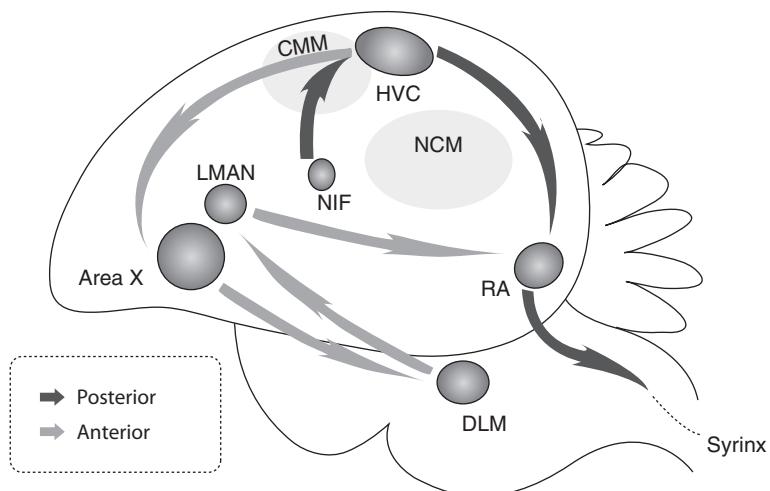


Figure 22.2 The schematic diagram of the brain of the songbird; description of the song system. The left side shows the front of the animal. Anterior pathway originates from the HVC and connects with area X, DLM, LMAN, and RA. Posterior pathway also originates from the HVC and directly connects to the RA.

Several cortex–basal ganglia–thalamus–cortex loop connections occur in the mammalian brain. Human speech is also controlled by some of these loop connections, and the basal ganglia play an important role in learning and maintaining normal speech. In the songbird brain, the HVC is connected to area X, which is considered to be a part of the avian basal ganglia. From there, the connection goes to the thalamus and then back to a part of the cortex-equivalent structure, the lateral magnocellular nucleus of the anterior nidopallium (LMAN). This loop structure is important for learning and maintaining the song. Note that birds with lesions in area X sing stuttering songs (Kobayashi, Uno, and Okanoya 2001), and humans with lesions in the basal ganglia also stutter their utterances (Alm 2004).

From the HVC, a direct projection of motor commands goes to the RA, the motor-cortex equivalent of the avian brain. Another projection goes to the RA from the LMAN. How these two inputs to the RA interact with each other is one of the most interesting topics in birdsong neuroscience and will be discussed elsewhere in this chapter. From the RA, a motor output goes to the hypoglossal motor nucleus (XIIts), a song motor nucleus that directly controls the effector organ, the syrinx. The pathway through the HVC, the RA, and nucleus XIIts is called the dorsal pathway, while that through the HVC, area X, the LMAN, and back to the RA is called the anterior forebrain pathway (AFP). In general, a lesion in any part of the dorsal pathway results in a song production deficit, but AFP lesions produce more complicated effects, especially during learning (Doupe and Kuhl 1999). Note that a direct pathway exists from the RA (a cortical structure) to nucleus XIIts (a medullar nucleus) in songbirds, but not in pigeons (Wild and Arends, 1987). A direct pathway from the cortex to the medulla that controls vocal output is scarce in primates; it is only evident in humans and not in other primates, including chimpanzees (Jurgens 2002).

22.2.5 Genes

FoxP2 is one of the genes that regulates when and where other genes are transcribed into proteins. *FoxP2* is a highly conserved gene among animals from worms to humans, but the number of genes regulated by *FoxP2* has gradually increased over the course of evolution. Through the mammalian evolution of about 60 million years, three point mutations have occurred in *FoxP2*; two of these took place during the 6 million years since our ancestors departed from the chimpanzees. This means that *FoxP2* is a gene whose specific mutation was accelerated and reserved in the human lineage (Lai *et al.* 2001). An English family was found in which some of the members have degenerations of the *FoxP2* gene. Some of these individuals showed deficits, particularly in speech articulation, and as a result, they had a difficulty in producing and comprehending language (Lai *et al.* 2001).

This finding naturally stimulated birdsong researchers because birdsong, like human speech, is a learned vocal behavior. These researchers suspected that *FoxP2* should have something to do with song learning, although avian *FoxP2* and human *FoxP2* both have several amino acid replacements. Gene expression studies examined the expression patterns of *FoxP2* in the brain, finding that *FoxP2* is highly expressed during song learning, but not before or after that period (Teramitsu and White 2006). Direct manipulation of *FoxP2* expression was made possible by using a viral infection technique to knock down *FoxP2* in the basal ganglia in zebra finches, showing that such treatment did impair song learning (Haesler *et al.* 2007).

FoxP2 is but one of several hundred genes that may be related to birdsong and speech, and it is erroneous to consider *FoxP2* a “monolith” (as in the film *2001: A Space Odyssey*) of vocal learning. In our laboratory, we found that a family of cellular adhesion molecules, cadherins, changes the expression pattern during the sensory and sensorimotor phase of song learning in Bengalese finches (Matsunaga and Okanoya 2008). Cadherin 7 is downregulated during the sensory learning period. Our results showed that overexpression of cadherin 7 via a viral infection during the sensorimotor period in juvenile Bengalese finches disrupted song learning, just as the knockdown of *FoxP2* did in juvenile zebra finches (Matsunaga *et al.* 2011). Thus, studies on the genetic mechanisms of speech and birdsong should not overemphasize the contribution of a single gene, but instead consider the functioning of networks of multiple genes.

22.3 Specific issues

After reviewing the basic facts of birdsongs, I now provide more specific points that are useful in making advanced comparisons between birdsong and human speech, especially in the context of biolinguistics.

22.3.1 Birdsong syntax: Can it be a model of syntax in human language?

Singing a song establishes a sexual context between birds, but each element of birdsong or combinations of them do not have corresponding meanings. This observation has been used to ascertain that the term “syntax” should not be used in the birdsong literature. However, element sequential ordering in birdsong can well be analyzed and modelled by concepts of formal syntax theory, and this provides an interesting biological model for that theory (Berwick *et al.* 2011). To enable a fruitful comparison between birdsong syntax and syntax in human language, we should restrict the comparison to the level of string manipulations that include studies of perceptual, motor, and developmental segmentation and chunking of song element strings.

To describe the syntax that characterizes birdsong, both the terminal symbols and the structure by which these are ordered need to be identified. The first issue relates to the basic unit of song production and perception. Although birdsong consists of a set of elements that are delimited by silent intervals, these elements are not guaranteed to correspond to the unit of perception and production. Statistical analysis of co-occurring elements and behavioral analysis of song productions and perceptions are necessary in this regard. The second issue is related to the statistical and rule-based relationships between the units and the resulting topography. These relationships can be expressed in several ways, including traditional transition diagrams, state notations, sets of derivation rules, and more linguistically oriented branching diagrams.

In the following analyses, we used the Bengalese finch, a songbird, as a model to study the perceptual and motor aspects of song segmentation and chunking (Okanoya 2004b). Unlike other experimental birds, such as zebra finches, Bengalese finches sing complex songs that have a hierarchical structure that are learned via segmentation and chunking. The smallest unit of song production is the “note.” In most species, several notes are serially ordered to form a song, and the order becomes more or less fixed. However, Bengalese finches combine notes to constitute a “chunk,” and then further combine several chunks in multiple ways to constitute song phrases. Thus, Bengalese finch songs can be expressed by finite-state song syntax (Figure 22.3). Because of their hierarchical and syntactical nature, Bengalese finch songs provide a suitable model for studying segmentation and chunking in perception and behavior (Okanoya 2004a). Furthermore, because songbird songs are learned, Bengalese finch songs represent an interesting parallel to the acquisition of human language (Okanoya 2002).

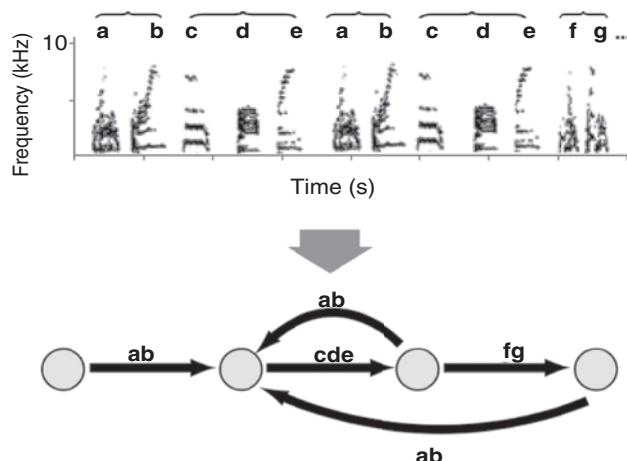


Figure 22.3 A sonogram and finite-state syntax extracted from a Bengalese finch.

Based on these studies, we can take a descriptive approach using finite-state syntax and an explanatory approach using hidden Markov models. At the conclusion, I suggest novel analysis schema using variable N-gram models for chunk extraction and k-reversible automata for transitional analysis.

22.3.1.1 Segmentation and chunking

In human language, multiple syllables are chunked to form a word, and these words are arranged to form a sentence. Words have a perceptual and motor reality for humans in that we can use discrete memories to both perceive and produce a particular word, and a focal brain lesion can obliterate the mental correlates of a specific category of words (Goodglass *et al.* 1986). Do birdsongs have such units of production and perception? An examination of the developmental process of birds constitutes one approach to this question. Juvenile birds with more than one tutor often learn parts of songs from different tutors and merge the parts to form one song. Two species of songbirds, the zebra finch and the Bengalese finch, have been examined in detail with regard to their development of song segmentation (Takahasi, Yamada, and Okano 2010; Williams and Staples 1992).

A second approach to answering this question involves studying real-time song production and perception. To examine the unit of production, one approach is to disrupt ongoing singing with a mild disturbance; the point at which a song terminates is considered to represent a boundary of motor chunking. If a motor programme actually chunks together several elements, the break points in songs might correspond to the boundaries between these chunks. When zebra finches were subjected to experiments focused on this issue, the minimum unit of song production in this species was found to correspond to song syllables (Cynx 1990).

For the perceptual side of segmentation, a study inspired by Chomsky's generative syntax (Chomsky 2002a) is very suggestive. When an extraneous stimulus was introduced while humans were processing a sentence, the part of the sentence that had perceptual unity could be processed prior to interruption by an extraneous stimulus (Fodor and Bever 1965). Indeed, humans can retain units that are of a higher order than words in their mental representations. Noun and verb phases are among these higher-order entities. The psychological existence of such abstract constructs was demonstrated with a behavioral experiment. Subjects were asked to identify the positions of clicks while listening to a sentence, and participants often displaced the position of the click in the direction of the boundaries of phrases. Similar procedures can be performed to examine perceptual chunking in birdsong (Suge and Okano 2009).

In the following sections, I present detailed accounts of a series of studies that addressed developmental, perceptual, and motor chunking in Bengalese finch songs.

22.3.1.1.1 Developmental segmentation

I hypothesized that the complexity of songs sung by Bengalese finches might have developed from perceptual and motor segmentation/chunking of available model songs by young birds. When chicks are raised by only one tutor, they learn the elements and order of songs primarily by copying the songs sung by that tutor. However, chicks with more than one tutor might learn pieces from each tutor and recombine them to form original songs. Our purpose was to clarify the developmental process by which Bengalese finches use chunking and segmentation to learn songs. The following is a short description of our recent work (Takahasi *et al.* 2010).

Eleven adult male and female Bengalese finches were kept in a large aviary with 11 nest pots, where they raised a total of 40 male chicks within 6 months. When the finches reached adulthood, we analyzed their songs and assessed their similarity to the songs sung by tutors by examining which part of the song came from which tutor. Using second-order transition analysis, we also analyzed the songs sung by tutors and chicks in terms of transitional structures; we measured the note-to-note transition probabilities as well as the inter-note intervals and jumps in pitch.

Bengalese finches raised with multiple tutors learned songs from several tutors. Approximately 80% of the birds learned from between two and four tutors, implying the operation of three underlying processes. First, juvenile finches segmented the continuous singing of an adult bird into smaller units. Second, these units were learned as chunks when the juveniles practiced singing. Third, juvenile birds recombined the chunks to create an original song. As a result, the chunks copied by juveniles had higher transition probabilities and shorter intervals of silence than those characterizing the boundaries of the original chunks. These processes suggest that Bengalese finches segmented songs by using both statistical and prosodic cues during learning.

22.3.1.1.2 Perceptual segmentation

Following the click experiment (Fodor and Bever 1965), we demonstrated the existence of a hierarchy characterizing song perception in Bengalese finches. The unit of song perception in this hierarchy is larger than the note, which is the smallest unit of song production.

Using operant conditioning, we trained male Bengalese finches to peck a key when they heard a short burst (10 ms) of white noise. The birds were trained to initiate the trial by pecking the observation key. After a random delay of 0 to 3 s, a short burst of noise was presented, and the birds were trained to peck the report key within a limited time interval (4 s initially, gradually decreasing to 1 s). Correct detection of the noise burst within the time interval was reinforced with a reward of yellow millet seeds. We gradually shortened the time available for an effective response to train the birds to respond as soon as they heard the burst of noise. We then introduced the subject bird's own song as a background stimulus. After the

bird responded to the observation key, we played a short segment (approximately 2 s) of the bird's song and superimposed a noise burst within the song, either inside or outside a "chunk" (two to five successive song notes that always appeared together). The song was played back either forward or in reverse.

We predicted that the reaction time for detecting the noise burst would be shorter when the noise burst occurred outside the chunk and longer when it was buried within the chunk. We also predicted that the reaction times would not differ when the song was reversed because a chunk would be processed before the noise burst within that chunk when the song was played forward. In addition, we hypothesized that reaction times to noise bursts that were outside a chunk would not differ from those to songs without a superimposed noise. Finally, we predicted that reaction times would not differ according to whether the noise burst was inside or outside the chunk when the song was reversed.

As predicted, the reaction times were significantly longer when the noise burst was within than when it was outside the chunk. We found no differences in reaction times when the song was reversed. Our results supported the notion that the perceptual unit of Bengalese finch songs is greater than an isolated note. The birds perceive songs by chunking notes and process the chunk as a unit.

22.3.1.1.3 Motor chunking

We next demonstrated the existence of the motor chunk, a higher-order unit of song production (Seki, Suzuki, Takahasi, and Okanaya 2008). We subjected singing Bengalese finches to a strobe light flash and determined the position in the song at which the bird stopped singing. Thirty song episodes were recorded for each of eight birds. We then used the recordings to count the note-to-note transitions and calculated the song note transition probability for each bird. When a note transitioned to the same specific note with a >90% probability (i.e. they tended to appear together), the notes were regarded as a "within-chunk" sequence. After the songs were recorded, each bird was placed in a small cage in a sound-attenuated box.

A light was flashed when the bird began a song, which usually resulted in the immediate termination of the song, implying that the fundamental unit of song production was the note. However, when the light interruption occurred within a chunk, the subsequent notes of that chunk tended to be produced, whereas interruptions presented between chunks tended to cause instantaneous song termination. This difference was statistically significant. These results suggest that the associations among sequences of song notes within chunks were more resistant to interruption than those between chunks. This confirmed that Bengalese finch songs are characterized by a motor hierarchy.

Higher-order song units might exist not only as a unit of perception, but also as a unit of motor production. Thus, to produce songs, the brain of the

bird might require a representation not only of each song note, but also of each group of song notes that serves as segmented parts of the song. Our results suggest that similar mechanisms of stimulus segmentation operate in Bengalese finches and humans, and that these result from complex interactions among perception, motor, segmentation, and chunking processes.

22.3.1.2 Procedures to extract song syntax

Two primary approaches have been proposed for extracting the syntax from a sequence of birdsong: the descriptive approach and the explanatory approach. The former attempts to produce a short-hand description of the birdsong sequence to enable comparisons among experimental manipulations or developmental processes, and the latter attempts to use certain mechanistic assumptions about a mental or brain representation of the birdsong to provide an account of why a particular sequence emerged.

Descriptive models use actual data on song strings and try to condense these into a set of rules or probabilities. Earlier attempts primarily utilized the transition diagram based on element-to-element transitional probabilities (also known as a first-order Markov model or bigram). The weaknesses of such probabilistic representations involve their sensitivity to subtle fluctuations in data sets and their inability to address relationships more complex than adjacency. The n-gram model (also called an $(n - 1)$ th-order Markov model) attempts to overcome the second weakness by addressing probabilistic relationships longer than immediate adjacency and predicting the nth elements based on the preceding $n - 1$ elements. A variant of this model involves changing the length of n according to the data so that n is always optimal. However, these modifications are not free of fluctuation (Okanoya 2004b).

The hidden Markov model (HMM) is among the most applicable models in terms of its explanatory power. This model assumes that actual elements of a song represent output from hidden states (Kogan and Margoliash 1998). The task is to estimate the number of these hidden states, the transitional relationships between the states, and the probability with which each song element emerges from each of the states. This task requires prior knowledge or assumptions about the nature of the states, as well as inferences about the transitional relationships between the states (Katahira, Okanoya, and Okada 2007). The established neuro-anatomical and neurophysiological characteristics of the song control system suggest that HMM represents one candidate for modeling the valid representations involved in the generative aspects of birdsong. The HVC probably stores hidden states (Fiete *et al.* 2010), and the RA is probably responsible for producing each song element (Leonardo and Fee 2005).

In order to overcome the drawbacks of the two approaches and to provide a model for both descriptive and explanatory purposes, we developed a set of procedures for automatically producing a deterministic finite-state

automaton (Kakishita *et al.* 2008). We first constructed an n-gram representation of the sequence data. Based on this representation, song elements were chunked to yield a hierarchically higher-order unit. We then developed a diagram that mapped transitions among these units and subjected this diagram to further processing for k-reversibility, where k-reversibility referred to the property of the resulting automaton that was able to determine the state that existed k steps back from the present state (Angluin 1982). Based on our experience with Bengalese finch songs, n was usually between 3 and 4, and k was often between 0 and 2. This set of procedures provided a robust estimation of automaton topography and has been useful in evaluating the effects of developmental or experimental manipulations of birdsong syntax. Furthermore, the resulting finite-state automaton can be used to guide further neurophysiological studies.

22.3.1.3 Discussion

Using analyses of Bengalese finch songs as examples, we demonstrated ways to advance studies in birdsong syntax and to clarify the relationship between the syntaxes of birdsong and human language. This approach offers both limitations and advantages. The limitations involve our original assumption that birdsong is a holistic signal with constituents that do not correspond to particular meanings; thus, birdsong is not characterized by compositionality. As a result, birdsong can serve as only a formal, rather than a detailed, model of human syntax. However, the absence of referential content in birdsong does not imply its inadequacy as a model of human language if we assume that the mechanisms underlying form and content are independent. Human inferior frontal areas have been shown to respond differently to strings derived from regular grammar than they do to those derived from context-free grammar. Area 45 was active for both types of grammar, whereas area 44 was active only in response to context-free grammar (Bahlmann, Schubotz, and Friederici 2008). Thus, satisfactory linguistic form, even without linguistic content, activated neural systems involved in language. In this regard, recent demonstrations that starlings and Bengalese finches could parse context-free grammar in a behavioral discrimination task (Gentner *et al.* 2006; Abe and Watanabe 2011) were somewhat unexpected and should be considered seriously. It follows that although no evidence for the use of recursive mechanisms to produce birdsong has been reported, birds might be able to perceptually process such structures. However, reanalyses of stimulus properties suggest alternative explanations: phonetic cues might be sufficient to account for such results (Beckers *et al.* 2012). Thus the issue remains unsolved and a breakthrough in conceptual and experimental design is expected to clarify this area of research (Berwick *et al.* 2011).

For the present purposes, birdsong syntax should serve as an adequate model for human language acquisition because key features are common to both systems. For example, the development of both birdsong and

human language is characterized by a period of babbling, premature vocalizations similar to adult phonology but less stereotyped. A recent neuro-anatomical study demonstrated that lesioning the higher-order vocal control nucleus (analogous to Broca's area) in zebra finches abolished normal song development; birds that underwent such treatment remained at the babbling stage of song production (Aronov, Andalman, and Fee 2008). Thus, the higher vocal area is absolutely necessary for the crystallization of song syntax in birds. Similarly, babbling in human infants might also be independent of Broca's area, and sensorimotor experimentation, in the absence of a higher-order area, might serve as a prerequisite for later linguistic sophistication.

More direct comparisons can be made from recent results in our own laboratory. Using artificial sound sequences that were segmented based solely on statistical cues, we found that when human adults continued listening to auditory streams, an area adjunctive to the anterior cingulate cortex was activated during the initial learning phase, and this activity gradually disappeared as learning advanced (Abla, Katahira, and Okano 2008). When the subject completely learned the segmentation task, inferior frontal areas, including Broca's area, were activated (Abla and Okano 2008). Determining whether juvenile birds with lesions created on a structure analogous to the anterior cingulate cortex (Okanoya 2007) and reared in a multi-tutor environment learn by segmentation and chunking will be interesting.

22.3.2 Neural mechanisms of birdsong syntax

Syntactical control in birdsong involves at least two distinct processes: chunking and branching. Chunking is a process that integrates multiple motor units into one. Branching is a process that selects one of several possible song notes (or chunks) to sing after one particular note (or chunk). Here, I discuss neural mechanisms of chunking and branching.

Because birdsong is a learned behavior, we need to know the mechanisms of sequential learning. Learning birdsongs depends on auditory feedback and error correction. I also discuss how these two processes affect the acquisition of song sequence.

22.3.2.1 Neural mechanisms for chunking and branching

How is a chunk of song notes coded in the bird brain? Neurons can be sensitive to a particular combination of events. If a set of temporal combination-sensitive neurons exist in the brain, such a system would code for temporal sequence in birdsong. For example, if the bird sang a song with a sequence of A-B-C, then the bird should have neurons sensitive to the combination of A-B or B-C. This assumption was examined by a single-unit electrophysiological technique (Lewicki and Konishi 1995). They indeed found neurons in the HVC that had specific combination-sensitivities

for the bird's own song. But how are such "useful" neural connections available in the nervous system? We felt that the nervous system is more robust, and thus used single-unit electrophysiology to show that note-to-note transitional information is population-coded in the HVC neurons of Bengalese finches (Nishikawa, Okada, and Okanoya 2008).

In male zebra finches, each of the HVC neurons that projects to the RA spikes at a particular time during a song sequence. Although firing patterns of these neurons are very sparse and bursting, this timing is highly precise (Hahnloser, Kozhevnikov, and Fee 2002). Such precision may be made possible by inhibitory interneurons and recurrent excitation of projection neurons (Mooney 2004). To produce variable song sequences, one must have a branching mechanism that controls which of a variety of possible syllable combinations are chosen each time the bird sings. Lesioning of the LMAN in adult Bengalese finches reduces the acoustic variability of each song element, but does not affect the sequence variability (Hampton, Sakata, and Brainard 2009). However, lesions of the NIf result in elimination of song branching and simplify songs in this species (Hosino and Okanoya 2000). These results suggest that the precise timing of a song sequence is organized in the HVC, but that branching of the sequence, and therefore chunking of song elements, may require other pathways.

In humans, clinical observations of speech sequence deficits are abundant, but these results hardly explain the mechanical details of sequence production because sequence deficits in human speech could arise from either psychological or peripheral factors. A functional MRI study attempted to depict the brain regions necessary for syllable sequence production (Bohland and Guenther 2006). In that study, comparisons were made between syllable and sequence complexity and between overt and covert speech production. The results showed that overt speech activated motor and auditory cortical areas related to speech production and perception, and subcortical structures including the insula, thalamus, basal ganglia, and the cerebellum. Increasing the sequential complexity additionally activated cortical and subcortical areas suggested to be involved in general motor sequencing, such as the left inferior sulcus, the frontal operculum, anterior insula, supplementary and pre-supplementary motor areas, and the basal ganglia. Although these results are promising, our present knowledge of human speech production does not allow for comparisons, at a functional level, with what is known in birdsong syntax.

22.3.2.2 Auditory feedback and reinforcement learning

Both birdsong and speech are motor behaviors adjusted by auditory feedback. The brain circuitry for speech and birdsong share a common feature in that the motor system also responds to auditory features, and the auditory perception of song or speech also depends on the proper functioning of the motor components within the "speech" of the birdsong

system. A clear demonstration occurred in birdsong motor components. Even the HMM showed a strong selectivity for species-typical songs (Williams and Nottebohm 1985), and this selectivity is found up to the highest motor nucleus, the HVC (Margoliash and Konishi 1985). The song motor system is also crucial for auditory discrimination of species-typical songs, as shown by lesion studies (Okanoya *et al.* 2001). Likewise, speech stimuli activate human vocal motor areas (Binder *et al.* 1997), and TMS stimulation of the motor areas disrupts auditory discrimination (Mottonen and Watkins 2009).

Motor performance is guided and maintained by auditory feedback of self-produced vocalizations both in speech and birdsong. In the song system, the anterior forebrain pathway provides an indirect, recursive connection to the direct motor pathway. This anatomical architecture stimulated computational neuroscientists who provided attractive models that explain functional and anatomical relationships in the song system. One of the earliest models assumed that reinforcement learning must be taking place in the song system and that the anterior forebrain pathway should provide an error signal to the direct motor pathway (Doya and Sejnowski 1995). Our current knowledge of the song system basically supports this notion, but further studies are needed to fully understand how the system can learn the song.

The reinforcement learning scenario occurs as follows. The anterior forebrain pathway connects to the nucleus of the direct motor pathway, the RA, through the output from the LMAN. Thus, the signal from the LMAN should be providing some sort of learning signal that modifies the connective strength between the HVC and the RA (Doya and Sejnowski 1995). One of the earliest experimental approaches attempted to validate this assumption (Brainard and Doupe 2000); when zebra finches were deafened, song acoustical deterioration occurred within 1 month, but when lesions were induced in the LMAN prior to deafening, this deterioration did not occur. This result could be interpreted to mean that the deafening-induced deterioration was due to an error signal from the LMAN, and when lesions are present in this nucleus, the error signal is no longer delivered to the direct motor pathway. However, this experiment alone could not determine the nature of the signal. This signal could be instructive, indicating how the connection between the HVC and RA should be modified, or permissive, allowing plastic changes between the two nuclei to occur but without specific information on how to modify the connection.

Advances in behavioral manipulations have radically expanded this view. A technique to detect a particular song syllable in real time and to give an auditory perturbation to that syllable allowed for the determination of whether birds had the ability to rapidly modify song acoustics (Andalman and Fee 2009). With this procedure, one could record real-time functioning of the LMAN in the maintenance of learned songs.

When the LMAN was active, birds were able to shift the song syllable to the direction opposite to that of the noise-masker, but when the LMAN was inactivated, this shift did not occur. This is strong evidence that the anterior forebrain pathway is providing at least a part of the instructive signal in addition to the permissive signal.

Feedback masking has also been used in human speech experiments, and this technique was recently combined with brain imaging to reveal brain mechanisms for real-time speech production (Christoffels, Formisano, and Schiller 2007). When overt speech was masked by pink noise, the activities of the insula and anterior cingulated cortex increased, suggesting that these areas are at the core of the speech adjustment system. The anterior cingulated cortex has been suggested to be involved in error monitoring (Magno 2006), and this structure receives direct or indirect projection from the basal ganglia. Thus, the anatomical position and functional role of the human anterior cingulated cortex could be comparable to that of the songbird LMAN.

22.3.2.3 Discussion

Taken together, both in human speech and birdsong, specialized motor areas provide a perceptual framework, and auditory feedback is crucial for the maintenance of learned vocalizations by providing error corrections to the motor system. Because of the functional and anatomical similarities between the two, birdsong will continue to serve as an excellent model to understand human speech production.

22.3.3 Evolution: Can we learn from birdsong?

The Bengalese finch is a domesticated Japanese strain of the white-rumped munia, a wild species endemic to South-East Asia. During 250 years of domestication, Bengalese finches have developed phonologically and syntactically complex courtship songs. Both string-pulling and phonotaxic responses showed that female Bengalese finches prefer complex songs, suggesting that females paired with complex singers were probably more successful in breeding. A cross-fostering study involving two strains revealed that certain genetic backgrounds are associated with song complexity in Bengalese finches. Munias demonstrated great rigidity in learning songs identified with their own strain, and they had difficulty learning songs associated with the domesticated strain. In contrast, the Bengalese strain showed equal competency in learning songs originating from both strains, although their accuracy was generally inferior. Our observations in wild habitats suggest that song complexity might be related to pressure for species identification, resulting in simpler songs in habitats with more sympatric species. Overall, sexual selection promoted song complexity by appealing to female preferences, and domestication exaggerated this tendency by allowing relaxation of species identification pressures. The

process proposed for song evolution in birds might have relevance to the process by which human language emerges and evolves.

Birdsong is a learned behavior that is culturally transmitted within a set of biological constraints. Birdsong can serve as an important biological model for human language with respect to interactions between culture and heredity. This study examined the differences between a wild and a domesticated strain of white-rumped munia (*Lonchura striata*) in terms of their songs. The comparison between the two strains revealed evolutionary factors affecting the acoustical and syntactical morphology of species-specific songs; these factors might also have relevance to the emergence of language in humans.

Wild white-rumped munias were originally imported from the Sappo port in Sekkou-shou, China, to Nagasaki, Japan, by a federal king of Kyusyu Prefecture in 1763 (Washio 1996). Since that time, they have frequently been imported from China to Japan, particularly from 1804 to 1829, when aviculture flourished in Japan. The white-rumped munia is generally brown with a white patch on the rump, as its name implies (Restall 1996). However, in 1856, birds with totally white plumage were distinguished from white-rumped munias and called Juushimatsu, or society finches. Although these birds were actually imported from China, European aviculturists believed that they came from India, and domesticated white-rumped munias imported from Japan to Europe were referred to as Bengalese finches (Buchan 1976). Thus, in what follows, the Japanese strain of wild white-rumped munias will be referred to as Bengalese finches. Bengalese finches were domesticated for their reproductive efficiency and their ability to foster other bird species, as well as for their plumage (Taka-Tsukasa 1917). During the approximately 250 years of their domestication, however, song characteristics have changed substantially from those observed in the wild strain, and the purpose of this chapter is to discuss the possible behavioral and evolutionary reasons behind these differences.

22.3.3.1 Song differences in wild and domesticated strains

Representative sonograms from a Bengalese finch and a white-rumped munia are shown in Figure 22.4. Brief inspection of the sonograms suggested that these two songs were very different in acoustical morphology and the order of elements. In general, the songs of the wild strain were noise-like and the notes were ordered simply and in stereotypic fashion, whereas the songs of the domesticated strain were more narrow-banded and had complex note-to-note transition patterns. We initially confirmed these impressions with acoustical analyses of song notes, and then by transition analysis of note sequences (Honda and Okanoya 1999).

Acoustical analyses revealed that the frequency of the maximum amplitude was higher in Bengalese finches than in white-rumped munias, and bandwidths 15 dB below the maximum amplitude were wider in

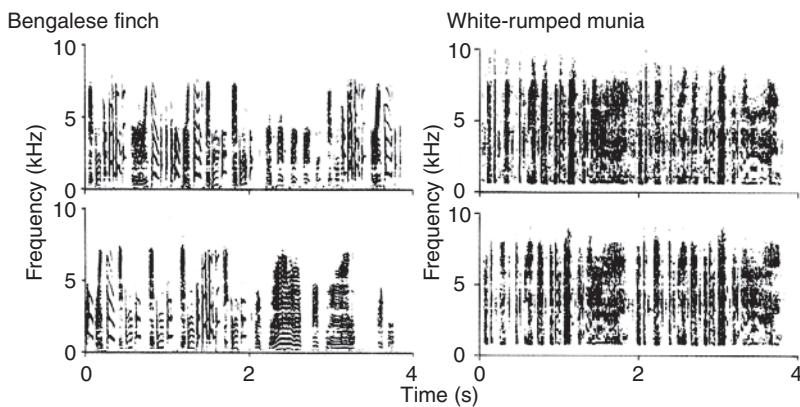


Figure 22.4 Sonograms from a Bengalese finch and a white-rumped munia.

white-rumped munias than in Bengalese finches. Furthermore, the sound density (root-mean-square value of 5 s of continuous singing) was, on average, 14 dB higher in Bengalese finches than in white-rumped munias when recordings were made with identical settings. However, no differences in the number of types of song elements were found between Bengalese finches (average 9.3) and white-rumped munias (average 8.4). Thus, Bengalese finch songs were higher-pitched, more narrow-banded and louder than white-rumped munia songs, but the strains did not differ with regard to repertoire size.

The sequential complexity of the songs was evaluated with the linearity index (Scharff and Nottebohm 1991), which is obtained by dividing the number of unique types of song notes by the number of observed transition patterns from one note type to another. This index is 1.0 (when N is the number of note types, then this will be $N/N = 1$) when the element sequence in the song is always identical, and it will approach 0 ($N/N^2 = 1/N$) when the element sequence is completely random. Results of this analysis showed that the average linearity index was significantly lower, signifying greater complexity, in Bengalese finches (0.33) than in white-rumped munias (0.61).

22.3.3.2 Female reactions to song complexity

What are the functions of song complexity in the Bengalese finch? Although this species is domesticated, I hypothesized that function evolved in part as a result of sexual selection by females (Anderson and Iwasa 1996; Catchpole and Slater 2003; Okanoya 2002). Because the Japanese avicultural literature does not contain evidence that songs were artificially selected by breeders (Washio 1996), we assumed that breeders selected only successful pairs and that this indirectly resulted in the selection of good singers. Therefore, I further hypothesized that males and females differed with regard to song perception and that song

complexity efficiently stimulated sexual behavior in females. I evaluated the former hypothesis using heart rate measurements, and the latter using several assays that supplemented one another (Searcy 1992). I first measured the reinforcing properties of a complex song using an operant task involving perch selection. Next, I measured the degree of nest-building behavior by female Bengalese finches as a function of stimulus songs. In addition, I measured the serum estradiol levels in females stimulated with complex versus simple songs.

22.3.3.2.1 Heart rate

Birdsong might be assessed and processed differently by each sex because its production and functional use are often sexually dimorphic. However, straightforward examination of this hypothesis has been difficult because different behavioral measures have been used to describe the process of song assessment in the two sexes. We analyzed changes in heart rate as an index of song assessment in the Bengalese finch (Ikebuchi and Okanoya 2003). In this species, only males sing, and song is used exclusively for mate attraction. Bengalese finches are not territorial, and the songs are not used in aggressive contexts. When a song stimulus was presented for the first time, the heart rate of the study participants increased. The duration of this increase in heart rate was defined as the period in which the heart rate increased by two standard deviations above that measured in the baseline interval, which was 10 s before song presentation. In both sexes, the repeated presentation of one song resulted in a reduction in the heart rate response. The presentation of heterospecific (zebra finch) songs did not increase the heart rate of Bengalese finches. When a novel conspecific song was presented, the heart rate increased only in females, and not in males, with each presentation of the stimulus. These findings confirmed the differential responses to songs by each sex in this species: males ignored the songs of other birds, whereas females were attentive. These patterns were not due to sex differences in memory capacity; operant conditioning studies have demonstrated that males and females do not differ in their memory capacity for songs (Ikebuchi and Okanoya 2003). Instead, the results suggested that syntactically complex songs might be more potent than simple songs in maintaining arousal in females.

22.3.3.2.2 Reinforcing properties of song complexity

To examine the preferences of female Bengalese finches with regard to song complexity, we employed an operant conditioning technique using the song as a reinforcer (Morisaka, Katahira, and Okanoya 2008). The protocol and apparatus used by Gentner and Hulse (2000) to test song preference in female European starlings were modified for Bengalese finches. I prepared a large metal cage and placed pot-shaped nests in the two upper corners. I also placed small speakers for song playback inside the nests and fastened a perch in front of each of the nest pots. A natural

song sung by a male Bengalese finch was used to prepare a simple (fixed order of song notes) and a complex (varied order of song notes according to a finite-state rule) song, both of which were played back from the relevant speaker when the bird sat on the perch. A female bird was placed inside this apparatus. Four of the eight birds tested chose the complex song, one chose the simple song and the remaining three chose both songs at random. These results suggest that the song preferences of female Bengalese finches varied depending on the individual, although more tended to prefer complex to simple songs. Because only one type of song was used in the experiment, the results should be interpreted with caution. Nevertheless, such female preferences could potentially contribute to sexual selection that facilitates the evolution of complex songs in male Bengalese finches (Morisaka *et al.* 2008).

22.3.3.2.3 Nest-building behavior

To further demonstrate the function of song complexity, I examined the nest-building behaviors of females (Eisner 1963) in response to songs with complex or simple syntax (Okanoya & Takashima 1997) using an approach first developed by Hinde and Steel (1976) and Kroodsma (1976). They demonstrated that female domesticated canaries engaged in more transportation of nest material when stimulated with conspecific songs than with songs of other species, and that female canaries performed more nest-building and laid more eggs when stimulated with large- compared to small-repertoire songs.

I analyzed the song recordings of a male Bengalese finch and identified four distinctive song phrases (Okanoya and Takashima 1997). The four phrases in this bird's song were organized such that phrases A or B were repeated several times and phrases C or D followed this repetition, but phrases C and D were never repeated. After phrase C or D was sung once, phrase A or B was repeated. I designed a computer program to produce this sequence of song phrases (complex syntax song) or one that repeated only phrase B (simple syntax song). Phrase B contained most of the song notes that occurred in phrases A, C, and D.

I examined three groups of four female Bengalese finches; each finch was kept in a separate cage, and they were kept together in a sound isolation box. The first group was stimulated with the song characterized by the complex syntax, the second group was stimulated with the song characterized by the simple syntax, and the third group was not stimulated with any song. The number of nesting items carried each day was counted and compared among the groups. Females stimulated with complex songs carried more nesting material. We further examined whether randomly generated note sequences were more effective than syntactically synthesized ones. Females that were stimulated with random note sequences were less responsive and carried comparable numbers of nest items compared with females stimulated with the simple sequence. Although

random sequences resulted in complex orderings of song notes, randomness did not generate the same female response as complexity produced by syntax (Okanoya 2004b).

22.3.3.2.4 Estradiol levels

Three groups of female Bengalese finches were used in this experiment; each group consisted of four separately caged females kept together in a sound isolation box (Okanoya 2004b). The first group was stimulated with the song characterized by complex syntax, the second group was stimulated with the song characterized by simple syntax, and the third group received no song stimulation. The levels of serum estradiol were compared among the groups before and after the experiment to determine baseline differences. Serum estradiol levels before and after the experiment were significantly higher in females stimulated with the complex song than in the females stimulated with the simple song or no song. Therefore, the complex song was more effective in stimulating female Bengalese finches into the reproductive condition.

22.3.3.3 Cross-fostering studies between the wild and domesticated strains

Bengalese finch songs are sequentially and phonologically complex, whereas white-rumped munia songs are simpler. To elucidate the degree to which environmental and genetic factors contributed to these differences in song structure, I cross-fostered white-rumped munia and Bengalese finch chicks (i.e. I used 7 pairs of Bengalese finches and 4 pairs of white-rumped munia and exchanged some of the eggs during incubation) (Takahasi and Okanoya 2010). As a result, I obtained 7 Bengalese finch-reared male white-rumped munias and 7 white-rumped munia-reared male Bengalese finches. For comparison, I also examined 12 normally reared male Bengalese finches and 14 normally reared male white-rumped munias. When the chicks had fully matured, their songs were recorded and phonological and syntactical comparisons were performed. Inspection of sonograms revealed that munia-fostered Bengalese finches were able to learn most of the songs sung by foster fathers, but Bengalese-fostered munias had some difficulty in learning the songs sung by foster fathers.

22.3.3.3.1 Constraints in phonological learning

The accuracy of song-note learning was measured as the percentage of song elements shared between the chick and the father. Detailed phonological analyses revealed that the accuracy of song-note learning was highest in white-rumped munia chicks reared by white-rumped munias (98%) and lowest in white-rumped munia chicks cross-fostered by Bengalese finches (82%). In contrast, Bengalese finch chicks exhibited an intermediate degree of learning accuracy, irrespective of whether they

were reared by white-rumped munias (92%) or conspecifics (94%). A two-way ANOVA detected a significant interaction between genetic background and rearing environment, indicating that white-rumped munias were sensitive to their rearing environments, whereas Bengalese finches were not. These results suggested that white-rumped munias are highly specialized to learning the phonology of their own songs, but are less adaptable to learning the phonology of Bengalese finch songs. In contrast, Bengalese finches are less specialized for learning the phonology of their own strain and more able to generalize their capacities to learn the songs sung by white-rumped munias. These findings suggested an innate bias toward species-specific phonology in white-rumped munias that might have been lost in Bengalese finches during domestication (Takahasi and Okanoya 2010).

22.3.3.3.2 Constraints in syntax learning

I used the same data set to test for a learning bias for song syntax. Similarities between the two types of song syntax were evaluated by first expressing the two songs under study as Markovian transition matrices (Takahasi and Okanoya 2010). In constructing these matrices, I considered the song notes shared by the songs of both tutor and pupil, as well as the song notes in songs sung only by tutors or only by pupils. The correlation coefficient calculated from the nonzero elements of the two matrices was used as an index for syntactical similarity. Using this method, I calculated average similarities between the songs of tutor and pupil in the four cross-fostered groups (Bengalese finches tutored by Bengalese finches, white-rumped munias tutored by white-rumped munias, Bengalese finches tutored by white-rumped munias, and white-rumped munias tutored by Bengalese finches).

Consistent with the results of the phonological learning experiment, the similarity between the songs of tutors and pupils was highest for white-rumped munias tutored by white-rumped munias (0.91) and lowest for white-rumped munias tutored by Bengalese finches (0.70). The similarities of Bengalese finches tutored by Bengalese finches (0.82) or by white-rumped munias (0.75) were intermediate in comparison to the two more extreme cases. Thus, when learning to sequence song elements, white-rumped munias were biased toward learning the linear syntax associated with their own strain and were far less adept at learning the complex syntax associated with Bengalese finches. These results supported my previous conclusion that white-rumped munias might have an innate bias for learning species-specific syntax and that this bias might have disappeared in Bengalese finches during domestication.

22.3.3.4 Field studies of wild white-rumped munia populations

I assumed that songs were kept simple in wild white-rumped munias because of specific pressures in the wild. Singing loud, complex songs in

the wild is costly for at least three reasons. First, such songs attract predators. Second, they draw on cognitive resources necessary for reacting to dangers, including predation. Third, the evolution and maintenance of the brain mechanisms underlying complex songs are costly. I began my fieldwork in Taiwan (Republic of China) to examine these hypotheses. However, the observation of predation in the wild is very rare, and this strategy did not allow for quantitative assessment of the hypotheses.

I examined the factors that might have accounted for the loss of the innate bias in Bengalese finches with regard to learning songs. One reason for the loss of this bias might involve an important function served by songs in the wild. Before a song can function as a mating signal to attract conspecific females, the singer must be identified as conspecific by the female. Toward this end, the song should possess species-specific characteristics. This function might degenerate as an identifying mechanism in a domestic environment because birds are paired by humans in these settings and thus do not need to seek mates on their own.

Several field studies might support this hypothesis. In the wild, white-rumped munias co-exist with various sympatric species, including spotted munias. A strong innate bias toward conspecific phonology should be adaptive for species of munia in avoiding futile attempts at hybridization. In contrast, Bengalese finches are domesticated and have been subject to controlled breeding. In such an environment, a species-specific bias would be neutral and might rapidly degenerate, perhaps allowing Bengalese finches to gain a more general ability to learn a wide-ranged phonology.

Preliminary data exist on the relationship between the degree of colony-mixing (with spotted munia) and song linearity in wild populations of white-rumped munia (Kagawa *et al.* 2012). When the level of heterogeneity in the colony was higher, the songs of white-rumped munia were more linear and exhibited less variable phonology. This might indicate that when more sympatric birds are present, species of munia must exaggerate their species-specific characteristics through their songs.

22.3.3.5 Discussion

I described acoustical and syntactical differences between wild white-rumped munias and Bengalese finches, female responses to song complexity, effects of cross-fostering, and fieldwork results to identify geographic variations in songs. Integrating these findings, I can now suggest a testable scenario for song evolution in Bengalese finches.

22.3.3.5.1 Domestication and sexual selection

The cross-fostering study revealed that white-rumped munias had a narrowly tuned learning mechanism for strain-specific phonology, whereas Bengalese finches had a more broadly tuned but less accurate learning mechanism. This finding should be considered in light of the

results of this fieldwork, which showed that higher sympatric ratios were associated with lower levels of song complexity.

Birdsong must initially operate as a species-identifier, and then it can function for sexual selection. In this regard, songs do not need to function as species markers in the absence of sympatric, closely related species. In environments characterized by the latter, however, songs should serve a sexual purpose. Domestication represents a special case in which no sympatric species exist. Because Bengalese finches no longer need to identify their species, they might have gradually lost the bias toward learning and producing species-specific characteristics in their songs. As a result of broadening the species-specific filter, Bengalese finches might have developed the ability to produce phonologically and syntactically complex songs. In this sense, song complexity might have arisen from a loss of species-specific bias rather than representing a gain in general learnability. Once constraints are weakened, female preferences might reinforce this tendency toward more complex songs. Indeed, male songs can be complex in syntactical and phonological domains to satisfy females' preference for variations.

22.3.3.5.2 A scenario for the evolution of song complexity

Based on the experimental results reviewed in this chapter, I suggest several steps that might underlie the evolution of complex song syntax in the Bengalese finch. In most estrildid finches, songs are used solely for the purpose of mating and not in male-to-male interactions. Thus, sexual selection is likely to have enhanced those song properties on which females base their choices, resulting in traits that are handicaps in the wild environment (Darwin 1871; Zahavi 1975). The following is one possible scenario that might explain the emergence of finite-state syntax in the Bengalese finch.

Complexity in song-note transitions became a sexually selected trait in white-rumped munias and was subject to individual variations due to genetic differences in neural capabilities and cultural differences in song traditions. However, the wild environment restricted the degree of possible song complexity in white-rumped munias due to the various costs associated with the maintenance of such traits, possibly including predation costs, foraging time, immunological costs associated with the production of testosterone, and metabolic costs associated with maintaining sufficient brain capacity to underpin the song system. Furthermore, songs needed to identify species in the wild, requiring that songs avoid phonological and syntactical complexity. Thus, mutations leading to greater song complexity would not have become fixed in a population of wild white-rumped munias, especially when sympatric species were living near them.

However, domestication eliminated many of these potential costs, especially those associated with predation and foraging time. Thus,

domestication relaxed the restrictions imposed on the evolution of song complexity (Okanoya 2002; Ritchie and Kirby 2007). Furthermore, it reduced the necessity for identifying members of the species via song. Therefore, mutations leading to song complexity through the loss of a rigid song structure were not fixed in the natural environment and were not eliminated in the domesticated environment. Changes in brain structure then allowed more elaborate songs to be learned and gave rise to the improvisation of song syntax. Genes that allowed for the learning of complex songs were selected because of the preferences of females.

22.3.3.5.3 Conclusion

Additional evidence is necessary to reinforce the above scenario. We need fieldwork data from a wider variety of geographical locations with different sympatric ratios. Data supporting female preferences for complexity should be examined at different levels of female receptivity, including approach, preference, acceptance, breeding behavior, breeding effort, and maternal effects (Soma *et al.* 2007). In the context of such reservations, I propose that explanations of the emergence of human language might benefit from observations of distantly related species, such as Bengalese finches (Deacon 2010).

22.4 General conclusion

Learning about birdsong is desirable because we believe that birdsong can be an excellent model in which to study biolinguistic inquiries. When we say A is a model for B, then A should provide at least analogical insights into the study of B when direct study of B is not possible because of cost, effort, the degree of technological advancement, the status of current knowledge, or ethical issues. Having reviewed the basics of birdsong and recent advancements in birdsong studies, our task is now to examine whether birdsong research may provide useful insights into biolinguistics. To examine this issue, I have established the following three assertions and examine each of these assertions in turn.

22.4.1 Birdsong is a possible model for the language evolutionary process

The premise has been held that language is so unique to us humans that no other biological system can possibly model the process of emergence and evolution of language. Of course, any system cannot be a model for any other system unless we assume a certain level of abstraction. In this regard, our research on wild white-rumped munias (Kagawa *et al.* 2012) has pointed to certain similarities in the evolutionary mechanisms of language, i.e. constrained and relaxed selection processes (Deacon 2010).

The strength of the birdsong system as a model of language evolution is that birdsong is learned, has social functions, and depends on certain innate knowledge to acquire it (Bolhuis *et al.* 2010). No system other than language itself seems to possess these characteristics.

22.4.2 Birdsong is an excellent model for vocal learning

Vocal learning, the ability to imitate a novel sound pattern, is a unique trait that is observed only in limited groups of animals, including birds, whales, bats, and elephants (Jarvis 2006). *Homo sapiens sapiens* is the only species of primate that obviously shows this trait (Braaten, Petzoldt, and Cybenko 2007; Jarvis 2006). One of the underlying anatomical features that differentiates vocal learners from non-learners is the direct cortex-to-medullar pathway for voluntary vocal control (Jurgens 2002). This pathway is clearly identified in songbirds, but not in non-passerines, and it is abundant in humans, but not in non-human primates. By this direct path, one can voluntarily regulate respiration and emit vocalizations that are not constrained to a specific behavioral situation (Deacon 1997). This pathway also allows modifications of innately programmed vocal gestures.

The cortex-to-medullar tract for voluntary vocalizations is but one of the several requirements for vocal learning. In addition, a system that enables matching between auditory and vocal information is crucial in establishing vocal mimicry. Mirror neurons are a class of motor neurons that are activated when the animal sees others doing the same action (Rizzolatti and Craighero 2004). A similar class of neurons for the auditory-vocal domain was found in the HVC of songbirds (Prather *et al.* 2008). These neurons show similar activity when the bird sings a particular song and when the same bird hears the same song. The HVC neurons can thus transform auditory patterns into motor gestures. The detailed mechanisms for this sensorimotor translation are not yet known, but a search for such mechanisms would be indispensable to understanding the human speech mirror system.

Taken together, birdsong provides basic mechanisms shared with human speech at two levels: corticobulbar connections for vocal plasticity and cortical mirror neurons for sensorimotor matching. In addition to these, we should also note the similarity in molecular mechanisms of birdsong and human speech, notably the *FoxP2* gene. Mutation of this gene resulted in language impairments in a specific family, and expression of this gene is pronounced during the sensorimotor learning phase in songbirds. Thus, birdsong should be a useful model for vocal learning.

22.4.3 Birdsong is a simple model for a subset of human syntax

I reviewed evidence that sequential structures in birdsong can be considered finite-state syntax and supplied behavioral and neural descriptions to

reinforce this assertion. I also reviewed the neural mechanisms underlying chunking and branching, both necessary mechanisms to support finite-state syntax. The remaining point is whether the finite-state syntax can be considered a simple form of human syntax versus having no relationship with any aspects of human syntax. Here, an important assay would be locating the exact level of the Chomsky hierarchy of birdsong because that would tell us whether birdsong is too distant from human syntax.

Berwick *et al.* used the concept of sub-regular language (Rogers *et al.* 2010) in analysing birdsong syntax (Berwick *et al.* 2011). They found that Bengalese finch songs and nightingale songs are at least more complex than a simple bigram, requiring us to consider more than just adjacent relationships between song elements (not strictly locally testable with window widths of two). One of the reasons why birdsong stays at this rather simple level might be related to its lack of semantics. This means that chunks in birdsong cannot be represented by labels or heads, disabling further hierarchical treatment of the chunks. A more proper linguistic level with which to compare birdsong is not sentence syntax, but construction of syllable phonology; finite-state syntax is enough for that purpose (Johnson 1972). Birdsong syntax therefore should more correctly be identified as phonological syntax as Marler (2000) suggested.

Despite the simplicity of birdsong phonological syntax, it requires at least a memory storage, a branching mechanism, and a chunking mechanism to construct the level of complexity birdsong shows. Birdsong should therefore remain an indispensable biological model with which to study brain mechanisms for human syntax (Bolhuis *et al.* 2010)

23

Language, culture, and computation: An adaptive systems approach to biolinguistics

Simon Kirby

23.1 Introduction: Why is language the way it is?

A common response to the very idea of a scientific approach to the origins and evolution of language is bewilderment about how it is even possible. How can we study the emergence of a trait that is apparently unique to a single species, and leaves no first-hand fossil remains? There are, of course, many answers to this question. For example, it is becoming increasingly clear that there is a lot that we can actually learn from homologies and convergent evolution in other species if we conceive of the faculty of language in a broad perspective (Hauser, Chomsky, and Fitch 2002), and techniques from fields such as genetics provide new sources of data on the origins of features of our species-unique capacities (Fisher and Marcus 2006). In this chapter, I will survey another approach that has become particularly important in evolutionary linguistics, which arose out of a concern with the challenge of finding a scientific methodology for language evolution: computational modeling. Although our story begins from a purely methodological perspective, it ends with a new way of thinking about the role of cultural transmission in an explanatory biolinguistics.

Before setting out, it is worth being clear about what the goal is for an evolutionary approach to language. On the one hand, we might be interested in an “evolutionary story” for humans that involves language. Although this is a significant topic of interest to many in the field it is not our main target here. Rather, we are interested in the fundamental explanatory challenge for linguistics: explaining why language has the particular structural properties that it does, and not some other possible set. The evolutionary approach to this challenge is one that attempts to explain universal properties of language as arising from the complex adaptive systems that underpin it.

Complex systems whose behavior emerges out of a multitude of interactions between large numbers of simple elements are notoriously resistant to reliable intuitions. For example, it is not immediately obvious why the interactions between individual drivers can, in certain circumstances, cause a traffic jam; or why sudden avalanches down the side of a sand-pile should follow power laws (e.g. Bak, Tang, and Wiesenfeld 1988). In order to understand and make reliable predictions about these kinds of systems, we need models that make explicit the role of individual elements, and the interactions between these elements. Computer simulation in particular is a valuable modeling methodology in these cases because once we have a model of the individual (be it a car driver, or a grain of sand) then it is a relatively simple matter to create a population of such individuals and then run virtual “experiments” on that population.

The motivation behind much computer simulation in biolinguistics is the belief that language, like the traffic jam, exhibits the properties it does because of the interaction between many individuals. Furthermore, the observed structural properties of language are believed to be emergent, in the sense that it may not be possible to simply read-off the universal structure of language from the shared properties of individuals. In this sense, the methodology of computer simulation provides an alternative approach to the more familiar Chomskyan idealization of the “ideal speaker-listener, in a completely homogenous speech community” (Chomsky 1965). Rather than abstract away details about population structure or patterns of interaction, computational modellers will typically retain these complexities, but come up with simplifying idealizations about the properties of the individual or the details of linguistic structure. It is important to realize that these different choices in creating models of language are complementary, and it would be a mistake to think that one were right and the other wrong. A complete theory of the evolution of language will require a suite of models, each with simplifying idealizations, and each illuminating a different aspect of the processes that give rise to the universal structural properties of language.

In this chapter, I will not attempt a complete review of the literature on the computational simulation of language evolution (see, e.g., Jaeger *et al.* 2009; Steels 2003; Kirby 2003; Cangelosi and Parisi 2002, for more comprehensive reviews). In particular, I won’t touch on any of the impressive work looking at the evolution of phonological systems (e.g. de Boer 2001; Oudeyer 2006) or grounding of models in physical robots (e.g. Steels 2010), nor will the focus be on formal mathematical models of language change and evolution (e.g. Niyogi 2006). Instead, I will work through a handful of case studies briefly that take us from the biological evolution of innate signaling, through the cultural transmission of vocabularies, and the emergence of simple syntactic structure, to co-evolutionary models that consider the interaction between learning, culture, and biological evolution along with its implications for innateness.

23.2 The evolution of simple innate signaling

Our first example of a computational simulation is not actually linguistic at all, but nevertheless it provides a nice foundation for the later models we'll be looking at. Oliphant (1996) considers what selection pressures need to be in place for reliable innate signaling to evolve. Simplifying drastically, we can consider this to be akin to the signaling we see in the alarm calls of many species (e.g. the vervet monkeys, Cheney and Seyfarth 1990).¹

Oliphant sets out an extremely simple model of an individual with an innate mapping between signals and meaning, which he glosses as "observable behaviours" and "environmental states/responses to those states" respectively. This simulated individual, called an *agent* in the modeling literature, has a set of genes that specify which observable behavior an agent will produce in a particular environmental state and which response an agent will give when it observes a particular behavior in another. To put it another way, these genes specify two mappings for each agent: one from meanings to signals, and another from signals to meanings.

Specifically, the genome of each agent is an ordered string of signals that the agent can produce (one for each meaning), and an ordered string of meanings (one for each signal). These genes specify a transmission function for sending signals, and a reception function for understanding them. The agents play a series of communication games in which one agent chooses a random meaning to produce and another tries to decode that signal back into a meaning.

The genes of each generation of agents can be passed on to their offspring, possibly with some mutations. In combination with a procedure for differentially selecting some agents and not others for reproduction on the basis of success at the communication game, this model thus implements a simple process of biological evolution by natural selection for signaling systems. The key question Oliphant asks is: *under what conditions does optimal signaling evolve?*

The answer, it turns out, depends on two key parameters of the simulation: precisely how agents are rewarded with greater chances of reproduction on the basis of communication; and how agents are selected to become partners in a communication game.

Let's consider the first of these. In a single interaction in Oliphant's model, an agent is chosen to be the sender of a signal and a second agent is chosen to be the receiver. The success of the interaction depends on the meaning for which the sender produced the signal being the same as the meaning the receiver understands that signal to correspond to. In the case of success, who should benefit?

There are two obvious possibilities that Oliphant considers. Firstly, we could imagine that only the receiver benefits from successful communication (in the scenario of the alarm calls, this seems plausible). In this case, for

every successful interaction, the receiving agent scores a point. After some number of interactions, the agents with the higher scores are selected to pass on their genes (with a chance of mutation) to a new generation of agents. Oliphant shows that with the simulation set up in this way, the average scores of the population do not increase over generations, despite the fact that agents are being selected on the basis of those scores. Specifically, optimal communication does not evolve. The signals that are used by the population do not reliably discriminate between meanings.

What has happened here? The issue is a familiar one to researchers looking at the evolution of altruism. If only receivers benefit from an interaction, there is nothing providing differential fitness advantage to the genes providing good production behavior, and this is required for optimal signaling to evolve. This is confirmed by Oliphant's second simulation, in which he gives a point to both sender and receiver for each successful interaction. In this case, optimal signaling evolves. In other words, the population share distinct signals for each meaning.

The other parameter Oliphant looks at is how agents are picked to take part in an interaction. For the results described above, the sender and receiver are picked randomly from the population, but an alternative is to imagine the population being organized spatially with two agents being picked to engage in an interaction if they are close together. Surprisingly, if the population has spatial structure, optimal signaling can evolve even when only receivers benefit from successful interactions.

This result demonstrates the way in which simulations can reveal important factors in the evolution of emergent behaviors that we might not otherwise expect purely from intuitive introspection about theories. In this case, the spatial structure of the population gives rise to a form of kin selection (Hamilton 1964). Altruistic signaling behavior can evolve because the recipients are likely to share genes with the sender by virtue of being born close to those that share recent ancestors.²

23.3 Learning simple signal systems

In the previous section, we looked at a model of the evolution of signal systems. Even with a very simple model, we were able to make interesting and potentially relevant observations about the importance of structure in populations on the likelihood of the emergence of communication. However, these kinds of results have limited relevance for linguistics because the communication system is entirely innate, leaving no role at all for learning. Whilst this may be a reasonable first approximation for much animal communication, it is clear that one of the strikingly unusual features of human communication is the extent to which the details of a particular language are acquired through observation of the language being used by others.

What we need, therefore, is a way of extending Oliphant's model to capture learning within the lifetime of the agents. Smith (2002) does just this. Rather than treat the signal system of an agent as a genome encoding specific signals to produce for each meaning and specific meanings to understand for each signal received, Smith collapses sending and receiving into a single matrix of weights. The weights record the strength of association between signals and meanings. A typical matrix is shown below:

1	1	3
0	2	1
4	0	2

Here, the rows correspond to meanings and the columns to signals. Given a matrix like this, the agents need a procedure for generating a signal given a meaning, and vice versa. The simplest procedure, and the one Smith (2002) uses, is called *winner take all*. In production of a particular meaning, the signal with the highest weight on that row is generated (if there is a tie for highest weight, one is chosen randomly from the winners). Similarly, in reception, the meaning is chosen that corresponds to the highest weight in the column for the received signal.

So, an agent with our example matrix will produce (and understand) the following language:

$$\begin{aligned} m1 &\leftrightarrow s3 \\ m2 &\leftrightarrow s2 \\ m3 &\leftrightarrow s1 \end{aligned}$$

This language turns out to be optimal assuming a population of agents that all possess it, because the set of signals differentiates unambiguously between meanings.

Given this model of the representation of meaning-signal mappings, an obvious question arises: how does an agent come to acquire the particular set of weights that characterize a language? Smith (2002) makes the simplifying assumption that learners are exposed to meaning-signal pairs, although clearly the real situation is not as clear cut as this and in real language acquisition considerable inference needs to take place for learners to extract meanings from context (A. Smith 2005). Given a set of meaning-signal pairs, the model must take a matrix whose weights are initially all zeros and adjust those weights in such a way that ensures the agent adapts its behavior to fit the signaling system it is exposed to.

There are a number of reasonable ways in which the weights in the matrix could be adjusted. Perhaps the most obvious algorithm results in a weight being incremented by one every time its corresponding meaning and signal are seen together in the input. In this case, the matrix ends up being a frequency table for the language with each cell recording the

number of co-occurrences of signal and meaning. Alternative methods include those that additionally reduce the weights between the observed signal and incorrect meanings, and between the observed meaning and incorrect signals – a process called *lateral inhibition*.

Smith explores a large space of possible algorithms for updating an agent's matrix, and finds that there are many different ones that are capable of successfully acquiring an optimal signaling system. The intriguing question is whether there are any important differences between these different algorithms, or whether they are all equivalent. In order to answer this question, we need to move beyond individual learning to consider the effects of the interactions between learners in a population context.

23.4 Iterated learning

When considering the validity of a particular model of learning, a typical approach is to test the efficacy of that learning model on a data set that tests the relevant feature of language being investigated. Even with the extremely simple model we discussed in the previous section – akin merely to a lexicon of fixed size – this is the strategy that was employed. Different ways of updating weights in the lexical matrix were tested against a data set drawn from an optimal pairing of meanings and signals. In this case, optimal simply means unambiguous, or in the lexical sense, having no homonymy.

However, when we are considering evolution, it is reasonable to ask where this language that we are testing the agents with comes from. For the evolutionary simulations of Oliphant (1996) and others (e.g., Noble 1999), the answer is clear: it is the result of evolution by natural selection of the genes encoding the mapping between meanings and signals. But for our learners, this can't be the case, because they are born not knowing which signals pair with which meanings.

The answer is that the language must come from other agents. We acquire language by observing utterances produced by other individuals who acquired language in the same way. In other words, the language we speak is the result of a process not just of learning in one generation, but a repeated cycle of learning and production over multiple generations (Hurford 1990). We call this process iterated learning (Kirby 2001; Kirby and Hurford 2002; Brighton, Smith, and Kirby 2005; Griffiths and Kalish 2007):

Iterated learning: a behavior is the result of iterated learning if it is acquired by observation of that behavior in an individual who themselves acquired the behavior in the same way.

Simulations of iterated learning focus on the social coordination of language among agents in a population, and the cultural transmission

of that language over generations, rather than the biological evolution of the genes that underpin agents' behavior. The key motivating question is this: *what is the relationship between the model of learning, and the outcome in terms of behavior of agents with that model of learning interacting in a population?*

In terms of Smith's (2002) model, he runs a number of simulations where a population of learners with a particular method of updating the weights in their lexical matrix learn from the utterances produced by the previous generation of agents. The basic setup of the population model is set out in Figure 23.1, although there are many other alternatives that could be considered (see e.g. Scott-Phillips and Kirby 2010, for an overview of different possibilities for modeling populations). In these simulations, the agents all have identical learning algorithms (for example, the frequency counting method outlined above), and are all born with no knowledge of language. The population can be initialized with a particular language (or no language at all) and then the process of cultural transmission by iterated learning can be left to run. Each agent learns from a sample of meaning–signal pairs produced by existing agents in the population and then joins the population when an older agent is removed. The new agents will eventually be called on to produce their own meaning–signal pairs for the next generation, and so on.

Using this iterated learning model, Smith returns to the consideration of different learning algorithms. Despite appearing equally capable of acquiring an optimal system, is their behavior different when considering the evolution of such systems?

Smith finds very distinct behaviors when a learning algorithm is iterated in this way. He defines a hierarchy of three types:

Acquisition: a learning algorithm is capable of acquisition if it can learn an optimal system.

Maintenance: a learning algorithm that is capable of acquisition is also capable of maintenance if a population of such learners can maintain an optimal system over generations in the presence of some noise on transmission.

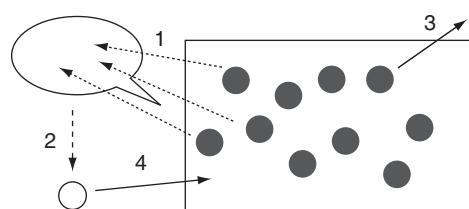


Figure 23.1 Smith's (2002) populations model. Adult agents shown as filled circles, a child agent shown as white circle. 1. Adults produce utterances. 2. Child learns from the utterances. 3. One adult is chosen at random and is removed from the population. 4. Child is added to the population as a new adult. The sequence then repeats with a new child.

Construction: a learning algorithm that is capable of maintenance is also capable of construction if a population of such learners can create an optimal system over generations from a random starting condition.

The frequency-counting method for acquiring a lexical matrix turns out to be capable only of acquisition. In the presence of noise, errors creep into the language which introduce ambiguity and lead to the eventual collapse of an optimal system for communication. On the other hand, methods that introduce lateral inhibition can eventually construct an optimal system in a population over generations through cultural evolution even from an initial state where every agent is simply producing signals at random.

Although what we see in these models is simply the cultural evolution of a lexicon – hardly a satisfactory model of language in all its complexity – the implications are nevertheless quite general. Each of the learning procedures for updating the lexical matrix that we might come up with represents a different hypothesis regarding the nature of the human language faculty. Several of these hypotheses prove perfectly adequate in terms of acquiring the target. If all we are concerned with as biolinguists is constructing a model of our biological endowment that can account for our ability to acquire language then we might mistakenly accept a hypothesis that is too low on Smith's hierarchy. We can appear to explain how humans *acquire* language, without giving any plausible answer to how a structured language is *maintained* over generations or is *constructed* in a population in the first place. In other words, in order to have a complete picture of the biological foundations for language, we need to consider the process of cultural evolution, and specifically how different learning mechanisms influence the emergence of structure in a population of individuals through iterated learning.

23.5 The emergence of compositionality

Results like those described in the previous section have convinced computational modellers of the value of considering the interaction between individual learning and the cultural transmission of behavior in populations in explanation of the origins of linguistic behavior. Nevertheless, it is clear that much of what is interesting about human language as a natural phenomenon cannot be satisfactorily explored if we restrict ourselves to modeling lexical matrices. We need to move beyond models that treat signals and meanings as atomic entities in order that we can consider what happens when signals and meanings have internal structure. In this way, we can start to look at phenomena that might be described as syntactic.

The general approach that the field of computational modeling takes is one of stepwise refinement of previous models, with each change adding

the minimum possible to extend the explanatory reach of what has gone before. In other words, at every stage, there is a tendency among modelers in this area to try and keep models as simple as possible (see Di Paolo, Noble, and Bullock 2000, for further discussion of the methodology of computational simulation of living systems in general). This often leads to puzzlement among linguists at the choices that modelers make. Typically, the central desideratum of linguists is coverage of data, and as discussed in the introduction, this leads to particular kinds of idealizations in the theories constructed. For the modeler simulating the evolution of language, the motivation is rather to try and understand the ways in which complex systems interact and how this might impact on the structure of language in the abstract first in order to construct general theories that may be applied eventually to explain language data. As such, the approach is rarely to try and build a model that looks exactly like the real thing – even if this were practically possible, it isn't obvious that we would be left with a clearer understanding. Emergent phenomena in a complex simulated system may end up just as mysterious as emergent phenomena in the real world.

The move from a simulation of language as a mapping between atomic signals and atomic meanings to one where we can start to talk about syntax, is just such a small, stepwise modification. By treating signals as sequences of symbols, and meanings as feature structures (for example), we can start to ask questions about the precise nature of the mappings between these two structured spaces. By doing this, we are not necessarily saying that this is the best representation of signals, nor especially the best representation of meaning, but rather we can consider the impact that introduction of some kind of structure has on the dynamics of iterated learning.

A number of papers have looked at precisely this kind of problem (e.g. Batali 1998; Hurford 2000; Batali 2002; Kirby 2001), but here we will focus on one particular model by Brighton (2002). This model considers the class of finite state transducers that map between strings and simple feature vectors. This can be thought of as a minimal extension to the lexical model. Clearly, such a model cannot deal with the full complexity of human language, not least because it considers only finite state languages. Nevertheless it is interesting to see what phenomena even a simple addition like this will do to the dynamics of cultural transmission.

Brighton uses a standard technique in machine learning called Minimum Description Length (MDL) learning (Li and Vitanyi 1997) to model an agent's strategy for acquiring a language from examples of string-meaning pairs. Given a set of utterances like the ones shown in [Figure 23.2\(a\)](#), the first step is to construct a transducer that accounts for these, and only these, examples (see [Figure 23.2\(b\)](#)). This grammar clearly fits the data very well, but has the problem that it is overly specific, and cannot generalize. MDL treats the learning problem as one of finding a trade-off between fit to data and generality.

A key observation is that *smaller* grammars will tend to be more general. In other words, an important quantity to consider when thinking about learning is the amount of space the representation of a grammar takes up. Equally, we can think of the amount of space taken up by a representation of the *data* that the learner is exposed to. This can change depending on what grammar we have, because the grammar itself can be used to encode the data. So, for example, the kind of maximally specific grammar that Brighton's learner constructs as a first step encodes the data very efficiently. We can essentially reconstruct the data heard exactly simply by generating all the string–meaning mappings allowed by the grammar. We say the coding length of the data given the grammar is minimized by this transducer. On the other hand, the coding length of the grammar itself is very large.

In Brighton's model, learning proceeds by looking for ways to compress the grammar, for example by merging nodes in the transducer. By doing this, the coding length of the grammar decreases, but the coding length of the data might increase, because the precise data that the learner has heard might not be specified explicitly by the grammar itself, and so extra information is needed to reconstruct it. Using the principles of MDL learning, many possible compressions of the original grammar are considered in terms of the sum of the coding length of the grammar and the coding length of the data given that grammar. The target grammar is the one that minimizes this sum and hence considers both generality and fit to data (see [Figure 23.2\(c\)\(d\)](#)).

We now have the necessary machinery to consider a simple extension of the lexicon-only model to tackle the origin of one of the fundamental design features of language: compositionality (i.e. the fact that the meaning of utterances is typically composed from the meaning of parts of those utterances). It is a rather surprising fact that out of the huge variety of communication systems in nature, there are none that utilize compositionality as part of a learned mapping between meanings and signals. This is surprising because it is precisely this feature that gives language its open-ended expressivity. It is such an obviously reasonable way in which to construct a signaling system, enabling individuals to construct a wide range of messages having only learned a much smaller set of more basic elements, that it is rather mysterious why we do not see it elsewhere in nature.

Given its uniqueness to human language, and its apparent adaptive benefits, the evolution of compositionality is an important topic for evolutionary linguistics (see, for example, Wray 1998 for a theory of language evolution that places the transition to compositionality at its heart, and subsequent debates about “holistic protolanguage,” Tallerman 2007; Smith 2008). In the context of Brighton's model, the learner can acquire a compositional mapping between meanings and strings (as in [Figure 23.2](#)), and it can also acquire quite straightforwardly a “holistic,” non-compositional one.

(a) $L_{\text{comp}} = \{\{\{1,2,2\}, \text{adf}\}, \{\{1,1,1\}, \text{ace}\}, \{\{2,2,2\}, \text{bdf}\}, \{\{2,1,1\}, \text{bce}\}, \{\{1,2,1\}, \text{ade}\}, \{\{1,1,2\}, \text{acf}\}\}$

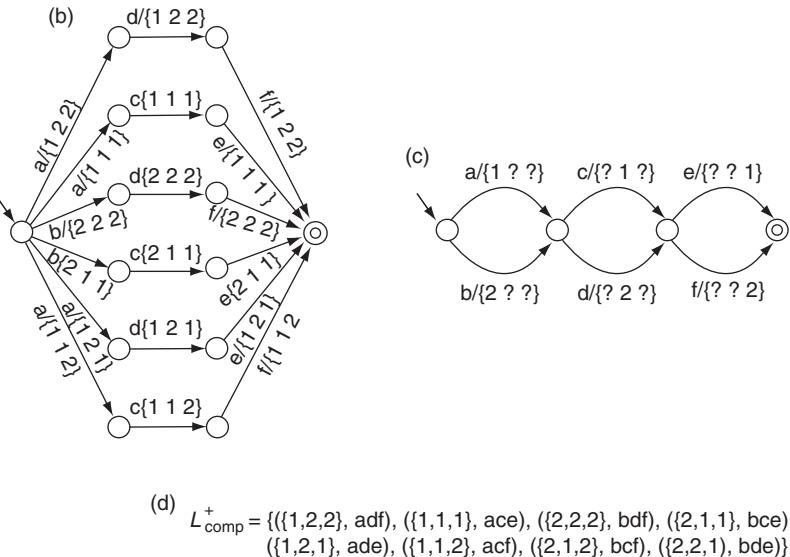


Figure 23.2 How a language can be compressed in such a way as to optimally trade off fit to data and generality using MDL. A toy compositional language is shown in (a). Here, meanings are simply triples of numbers and strings are triples of characters. This language is compositional because there is a one-to-one correspondence between numbers and characters. The transducer in (b) expresses this language precisely (i.e. with no generalization to novel string-meaning pairs). The transducer in (c) is the result of compression guided by the MDL algorithm. In this transducer, wild-cards in meanings are expressed using "?". This transducer generates a slightly larger compositional language shown in (d). Figure adapted from Brighton, Smith, and Kirby (2005).

The question is whether there is something about iterated learning that will lead a population of learners like this one to *construct* a compositional language from scratch.

To test this, Brighton gives his agents an ability to “invent” random strings for meanings that cannot be expressed by an agent’s current grammar, and then places them in a simple iterated learning setup whereby each agent is called upon to produce strings for a randomly selected subsample of meanings. These meaning-string pairs are then the training data for a new agent and the cycle repeats.

By virtue of the random invention mechanism, the first languages are inevitably non-compositional and unstructured. Because of this, the expressivity of the language is limited in the early stages. In other words, agents’ grammars typically only express the meanings that agents are exposed to – there is no generalization to novel items. However, over a number of generations, this gradually changes. Structure appears in the mapping between meanings and strings and compositionality emerges.

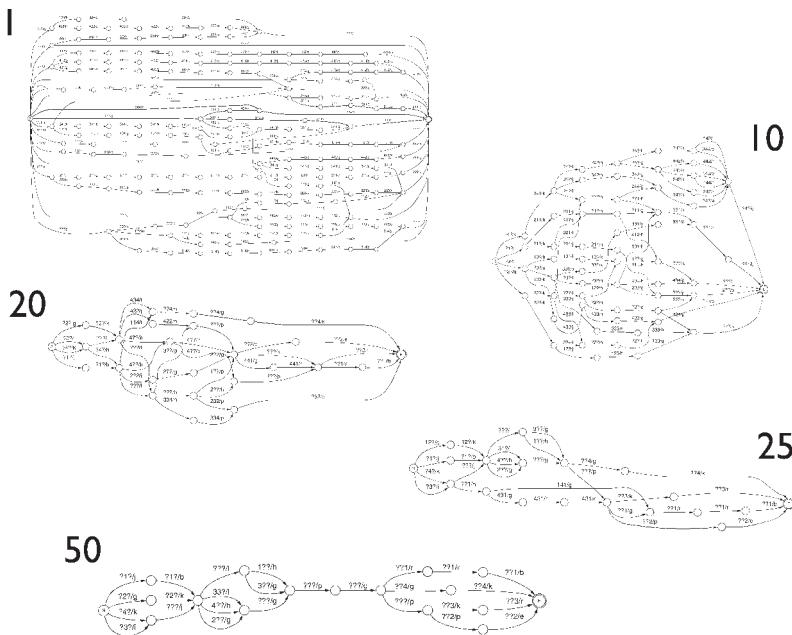


Figure 23.3 An example of the evolution of grammars in Brighton's simulation, numbers indicate the generation in the simulation the grammar is taken from. Over time, languages become more and more compressible and consequently more learnable and more structured. Figure adapted from Brighton, Smith, and Kirby (2005).

Eventually the languages are maximally expressive, and agents can reliably produce the same string as their “parents” for all meanings, even if they were not exposed to them during training. (See Figure 23.3 for an example of the evolution of grammars in this system.)

So, what we are seeing is a gradual evolution, purely through cultural transmission by iterated learning, of a holistic protolanguage to a compositional one. The result is a language that learners can reliably generalize from a sub-sample. It turns out that this generalization from a sub-sample is not merely a *result* of the evolution of structure in the model, but actually the cause of it. Brighton (2002) demonstrates this by varying the amount of data a learner sees in the iterated learning model, while retaining the same meaning space size. He argues that the limitations placed on the amount of data a learner is exposed to (one form of *stimulus poverty*, Zuidema 2003) acts as a bottleneck on cultural transmission. The tighter this bottleneck, the more pressure there is on language to adapt so that it can pass unscathed from one generation to the next. With a wide bottleneck (many data items for the learner), languages remain random, unstructured, holistic mappings between meanings and signals. It is only as the bottleneck narrows that compositional structure emerges.

The very fact that languages are transmitted by iterated learning under conditions of stimulus poverty leads to the emergence of structure as an

adaptation that maximizes transmission fidelity. Hurford (2000) expresses this pithily with the maxim that “social transmission favours linguistic generalization.” Note that this perspective sees compositionality as an adaptation, but not one that arises from biological evolution. At every stage, the agents in Brighton’s simulations are identical at “birth.” All that changes here are the languages in the simulation. The particular structures that emerge depend on what might reasonably be termed environmental conditions that impinge on the language itself (such as the number of utterances an agent is exposed to), and it is the language itself that adapts to those conditions. Languages adapt to maximize their transmissibility, and they do so by innovating structural properties such as compositionality that leverage structure in meanings in order to increase learnability.

Since the publication of Brighton’s simulations, and others that converge on the same results with strikingly different assumptions (e.g., Batali 1998; Hurford 2000; Batali 2002; Kirby 2001), the emergence of compositionality through iterated learning has also been demonstrated in human participants (Kirby, Cornish, and Smith 2008, and Scott-Phillips and Kirby 2010, for review). This shows the generality of the basic idea of structure as an adaptation to transmissibility.

23.6 Interactions between learning, culture, and evolution

One of the most challenging aspects of language from an evolutionary perspective is the unique degree to which it is the result of not just one complex adaptive system, but three – all of which interact at various timescales (see Figure 23.4). We have already seen the importance of looking not only at individual learning, but also how the particular nature of the learner results in different dynamics at the population level through

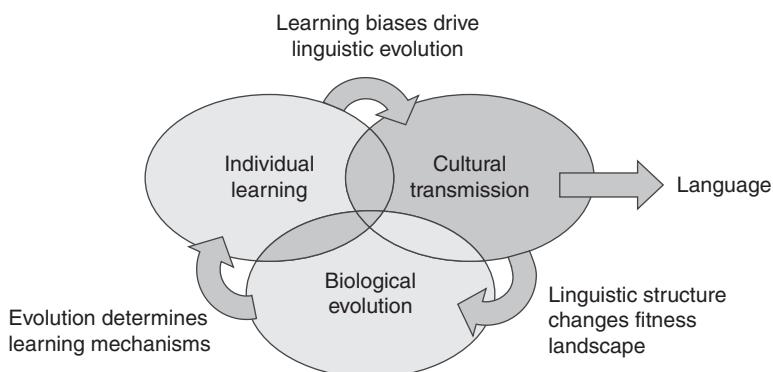


Figure 23.4 The three complex adaptive systems that underpin language, and their interactions. From Christiansen and Kirby (2003b).

cultural transmission. We have also seen that the cultural process can deliver up adaptive structure that previously we may have suspected could only be explained through biological evolution by natural selection.

Does this leave biological evolution out in the cold, with no part to play in the explanation of why languages are the way they are? Not necessarily. After all, the particular learning mechanisms that we bring to bear on the task of acquiring language are assuredly part of our biology. The key questions to ask about this aspect of the biology of language are: what is the nature of our biological endowment that relates to language learning? and to what extent is this endowment specific to language? These questions essentially define the biolinguistics enterprise, and their answer depends on an understanding of the relationships between learning, cultural transmission, and biological evolution.

Since the early days of computational modeling, as a result of simulations of the interaction between learning and evolution (Hinton and Nowlan, 1987), a number of scholars have suggested that a relatively obscure mechanism from the evolutionary literature is implicated in language evolution: the so-called *Baldwin Effect* (see Belew and Mitchell 1996 for review). For example, Jackendoff (2002: 237) has remarked on the evolution of language, “I agree with practically everyone that the Baldwin Effect had something to do with it.” What most people are actually referring to when they discuss the Baldwin Effect is better described as *genetic assimilation* (Briscoe 2000). What genetic assimilation does is provide a mechanism that is entirely compatible with standard Darwinian natural selection whereby behaviors that are learnt may, over evolutionary time, become innately encoded.

Put simply, this is possible if we assume that there are behaviors that could potentially arise from learning or be genetically determined to a greater or lesser extent. This means that, even if an organism does not have some optimal fitness-enhancing behavior completely innately encoded, they can nevertheless potentially exhibit that behavior through learning. As long as learning has some cost then a population in which there is variation in the extent to which a behavior is innate will exhibit differences in fitness that favor increasingly nativized variants. Figure 23.5 contrasts two scenarios from Hinton and Nowlan’s (1987) model: one in which there is a single genetically determined behavior that gives fitness advantage; and another in which the same is true but there is also the possibility of learning. The first scenario presents evolution with a “needle in a haystack” search problem. No matter how close to the optimal solution an organism is, they receive no fitness advantage, so the best evolution can do is simply to chance upon the perfect solution through a lucky combination of mutations. In the second scenario, however, individuals can reach the optimal behavior by learning even if they do not have it innately specified. This means that organisms who are born with some of the behavior innately specified are better off than those who are born with none.

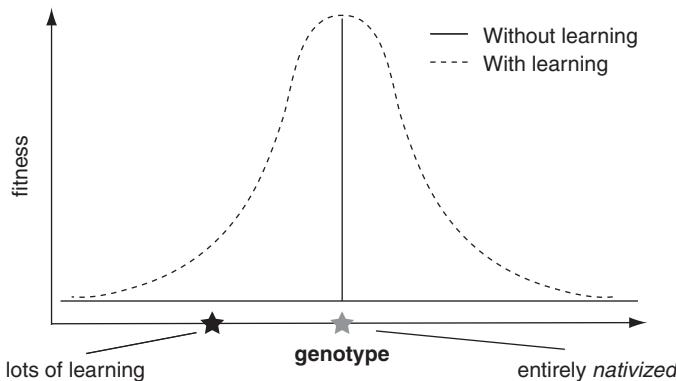


Figure 23.5 Two “fitness landscapes” corresponding to a similar adaptive challenge with and without learning. In this scenario, without learning, the only difference in fitness is felt by an individual with a very specific genotype. With the addition of the possibility of learning, individuals born with nearly the “correct” genotype are born with some fitness advantage over those that are born far away from the fitness peak. In this second scenario, we expect an evolutionary trajectory from individuals with lots of learning to those that are born with the trait entirely nativized. However, see main text for issues with applying this logic to language.

However, since learning is assumed to carry some cost, the optimal organism is still one in which the behavior is entirely innately specified.

The introduction of learning to the model thus radically alters the fitness landscape that evolution must search. We have moved from a needle in a haystack, to a smooth hill for evolution to climb. The outcome is an evolutionary trajectory that moves from behavior that is supported entirely by learning to one that is increasingly innately coded.

It is clear why, given models like this, that the so-called Baldwin Effect is of great interest to biolinguistics. It appears to provide support for the evolution of a language faculty in which linguistic behavior is to a great extent constrained by a domain-specific genetic endowment. However, it is important to realize that the Hinton and Nowlan (1987) results cannot be directly applied to the linguistic case.³ Looking again at Figure 23.4, it should be clear that this model of the Baldwin Effect only considers the interaction between learning and evolution, and misses out cultural transmission.

The most obvious effect of removing cultural transmission from a model of the interaction between learning and biological evolution is that the target for learning (i.e. the peak in the fitness landscape) is fixed. If language is transmitted culturally, and as a result is subject to change, then there is no longer a fixed target for the Baldwin Effect to nativize. Chater, Reali, and Christiansen (2009) have argued that this is a serious problem if we assume that constraints on the ways languages change are arbitrary, and that the rate of language change is higher than that of biological evolution.

In addition to the fact that cultural transmission implies change, we also need to consider the results from the work we reviewed earlier on iterated

learning. We have already shown that there is not a straightforward relationship between our biologically specified learning machinery and the resulting structure of language. What does that mean for evolutionary models in which fitness is determined by the behavior in question (specifically, language) but what is selected are genes that shape the nature of the learner?

To answer this question, we need a model of learning which cleanly separates the influence of the genes from the influence of data observed through cultural transmission. Because of this, a recent trend in work on iterated learning has been to use Bayesian models of the learning process (Griffiths and Kalish 2007; Kirby, Dowman, and Griffiths 2008; and see Griffiths, Kemp, and Tenenbaum 2008 for a tutorial introduction to Bayesian approaches to cognition). In a Bayesian model, the influence of the data and the influence of factors independent of the data (e.g. the learner's genes) are explicitly combined to compute the probability of various hypotheses about the target language given the utterances observed. This is possible using Bayes' rule:

$$P(h|d) = \frac{P(d|h)P(h)}{\sum_{h'} P(d|h')P(h')}$$

where $P(h|d)$ is the *posterior probability*, the learner's confidence a particular grammar h accounts for the linguistic data, d ; $P(h)$ is the prior belief in each grammar; and $P(d|h)$ gives the probability that grammar h could have generated the observed data. (The denominator here simply normalizes the probabilities by summing over all possible grammars.)

So, we can use the prior as our model of innateness – it is what the learner knows about language independent of any utterances heard and it is provided, perhaps indirectly, by the genes. Kirby *et al.* (2008) following Griffiths and Kalish (2007) place this model of learning within an iterated context by allowing the product of one instance of Bayesian inference to determine the data for the next generation. It is possible to compute from this a “transition matrix” indicating the probability that any one language will change into any other language after one “generation” of learning. The particular probabilities in this matrix will depend primarily on the prior and the number of utterances an individual hears during learning (the bottleneck). Finally, this transition matrix can be used to compute the “stationary distribution” arising from iterated learning (see also Komarova and Nowak 2003). This is the expected distribution of languages after a sufficient period of cultural evolution has taken place. In other words, the stationary distribution corresponds to the distribution of language described by the set of all language universals.

In a nutshell, what the Bayesian iterated learning model buys us is a straightforward way to compute the language universals predicted to emerge given a particular innate prior bias and a particular bottleneck.

In other words, it gives us a particularly general view of what cultural transmission adds over and above any innate contribution.

What this means is that we can move beyond simply equating cultural transmission with *change* in our co-evolutionary models of the biological evolution of the language faculty as Chater *et al.* (2009) do. We now have a principled way of relating changes in the genotype (the language faculty) to the ultimate fitness-relevant phenotype (the language an individual is likely to speak). Smith and Kirby (2008) utilize this to consider a simple question: under what conditions should we expect biological evolution to favor strong innate constraints on the form languages can take?

The classic results from the Baldwin Effect models seem to suggest that such constraints will be favored as long as there is a cost to learning. However, Smith and Kirby (2008) suggest that the addition of cultural transmission, in the form of iterated learning, radically changes things. Specifically, it acts to neutralize any variation in the strength of prior biases. This is because – as Dediu and Ladd (2007) predict – culture can take a weak preference and amplify its influence in such a way as to create a strong bias in the distribution of languages. Kirby *et al* (2008) show that the ultimate strength of a cross-linguistic bias in language distribution is dependent on the bottleneck but not the strength of the prior bias. The distinction between a strong innate constraint and a weak innate bias is therefore effectively “masked” from the view of natural selection (see also Wiles *et al.* 2005).

The surprising conclusion is that we should not expect natural selection to favor strong innate constraints in the case of a culturally transmitted trait like language. Weak nudges from our biology suffice. Smith and Kirby (2008) conclude by saying:

If we believe that stronger prior biases (more restrictive innate machinery) cost, then selection can favour the weakest possible prior bias in favour of a particular type of linguistic structure. This result ... leads us to suspect that a domain-specific language faculty, selected for its linguistic consequences, is unlikely to be strongly constraining – such a language faculty is the least likely to evolve. Any strong constraints from the language faculty are likely to be domain general, not domain specific, and owe their strength to selection for alinguistic, non-cultural functions. (p. 3601)

23.7 Conclusions

In this chapter, we have seen some examples of how computational modeling has been used to tackle the complex adaptive systems that underpin the emergence of language structure: individual learning, cultural transmission, and biological evolution. Our intuitions about the behaviors of these kinds of systems are often confounded, as I hope these simulation

models have demonstrated. Nowhere is this more true than when we consider the interactions between the different timescales that are involved. What might make intuitive sense when considering the biological evolution of the language faculty alone, for example, must be reconsidered when the masking influence of cultural transmission is included in the models.

Of course, these models are by their very nature extraordinarily simplified. But this, to many who work in the field of computational modeling, is in fact their strength. Idealized models give us insight into how the fundamental mechanisms involved behave in general. They allow us to develop new theories that build on a more solid understanding of what to expect when, for example, mechanisms for learning a cultural system evolve. Conversely, they allow us to reject hypotheses based on faulty understanding of what is or is not likely when adaptive systems come together. For instance, I would argue that we can reject the hypothesis that natural selection is the only possible source for the appearance of design in language structure (e.g., Pinker and Bloom 1990), now that we know that iterated learning provides an additional possible adaptive mechanism.

Ultimately, however, this kind of work can only grant us a new toolkit for building theories and suggest new places to look for hypotheses. They must be paired with work from the other branches of this interdisciplinary enquiry of biolinguistics. This will require bridge-building between different approaches that take very different stances on the kinds of idealizations required to carry out research in linguistics. Idealizing away from the complexities of linguistic phenomenon in order to focus on the role of populations, for example, needs somehow to be reconciled with the more traditional idealization away from the complexities of population dynamics in order to focus on details of the representation of syntactic structure. This kind of rapprochement between distinct methodological approaches is likely to be one of the foremost challenges in the coming years of our discipline.

24

Language and natural selection

Derek Bickerton

24.1 Introduction

Did language evolve through natural selection? At first sight, this looks like a question that would take a straight yes-or-no answer, one to which almost all biologists (if not necessarily most linguists) would respond with an unhesitating affirmative. Unfortunately things aren't so simple. In the constantly expanding literature on language evolution, one can distinguish at least the following points of view:

- (1) Language began through natural selection and every subsequent step in its development was guided by natural selection (Pinker 1994; Pinker and Bloom 1990).
- (2) Language began through natural selection and was subsequently adaptive as a whole, but particular steps in its development were not selected for per se (Jackendoff 2002).
- (3) Particular prerequisites for language were selected for non-linguistic reasons, and language itself was a resulting emergent property, but language itself was not directly selected for (Jenkins 2000; Hauser *et al.* 2002).
- (4) A suite of cognitive characteristics was selected for, having nothing to do with language, that made possible the cultural invention of language (Donald 1991; Fauconnier and Turner 2008).

Since this chapter deals only with natural selection, it will not cover (3), and (4), properly speaking, lies outside the purlieu of biolinguistics altogether, although it may be necessary from time to time to remind readers of factors that make the invention of language by clever humans a highly unlikely development. Accordingly I deal only with variants of the first two positions.

The first point to emphasize is that, as the differences between (1) and (2) suggest, we cannot treat the question "Did language evolve through natural

selection?” as a monolith. We must distinguish between two questions of narrower scope: “Was the initial emergence of language due to natural selection?” and “Were formal characteristics of language a consistent and specific target for natural selection during subsequent development?”

For any trait to be selected, two conditions have to be fulfilled. There has to be some kind of genetic variation for selection to choose from, and there has to be some kind of selective pressure as a result of which the target genes lead to increased reproduction for those organisms whose genotype includes them. (As an example of variation, take say a species with varying genetic instructions for density of fur, and as an example of a selective pressure, a downturn in average temperature – organisms bearing genes that code for denser fur will enjoy longer life and better health than organisms that do not, and consequently will increase their representation in the gene pool.) When we turn to the evolution of language we must accordingly ask what the relevant variation might have consisted of and what pressure(s) might have selected from that variation. Since the type of variation and the selection pressures involved are by no means necessarily the same in both the original emergence of language and its subsequent development, these two stages will be dealt with separately in what follows.

24.2 Pre-linguistic variation

When change in an existing trait is involved, the answers to questions about variation and selective pressure are usually straightforward. When an evolutionary novelty such as language is involved, problems arise. If a behavior is truly novel, then by definition there cannot already exist variation in that behavior; variation must therefore be sought in some other type of behavior, and suggestions made as to how that variation could have been co-opted for some role in generating language.

What is perhaps still the most explicit exposition of position (1) – Pinker and Bloom (1990) – clearly shows awareness that genetic variation must be shown to exist if an account based on natural selection is to be supported. But the only variation Pinker and Bloom consider (*ibid.*, Section 5.1) is that which is found in contemporary language. Extrapolating backwards from this, it is not unreasonable to suppose that some degree of variation has existed since the early days of language. But to project things further we would have to suppose that linguistic variation preexisted language! Pinker and Bloom fail to mention this problem.

24.2.1 Continuism and the “ladder to language”

The question therefore remains: what kind of variation in human ancestors could have been selected from in order for language to begin? Since

language is used (among other things) for communicating between individuals, the communicative behaviors of ancestral species seemed the likeliest place to look, and that area has indeed been, historically speaking, where most adherents of natural-selection theories of language have looked. Perhaps the commonest assumption, from early studies such as Hockett (1960) to recent ones such as Hurford (2007) – though one not shared by Pinker and Bloom or, so far as I know, by many if any generativists – has been that language was somehow on a continuum with forms of animal communication. Let us call this position “naïve continuism.”

Plausible though naïve continuism might initially sound, it suffers from serious problems. Some of these were pointed out in the first chapter of Bickerton (1990). The properties of language and animal communication differ radically from one another: animal communication is stimulus-bound, finite, and trapped in the here-and-now, whereas language is stimulus-free, open-ended, and can roam freely over past and future, over regions remote in space or existing solely in the imagination. An even more fundamental argument has recently been provided by Balari and Lorenzo (2009b, 2010b). They point out that systems of animal communication do not constitute a natural class; “communication” is itself no more than a convenient abstraction covering a range of behaviors as diverse in their means of expression as they are in their functions. Moreover, it is natural and inevitable that this should be so, since the functions and structures that any species employs to communicate between members are tailored by evolution to fit the needs of that particular species.

It follows that it is senseless to talk about language evolving out of pre-linguistic communication, since pre-linguistic communication itself did not evolve – at least not in the sense that organs like eyes or wings evolved, by a stepwise and ratcheted progression in which each stage built on a prior stage over a series of related organisms. But even the more thoughtful continuists (e.g. Hurford 2007) seem to assume that animal communication constitutes some kind of “ladder to language,” the rungs of which constitute precursors of words, syntactic combinations, and so on. For that matter, even some non-continuists seem to partially endorse the notion; as Balari and Lorenzo (2009b, 2010b) point out, Hauser *et al.* (2002) treat many pre-linguistic behaviors, including some communicative behaviors, as giving rise to the Broad Faculty of Language, and hence as being legitimate objects of study for language evolutionists.

However, many facts bear strongly against any kind of consistent evolutionary development in communication. Take for example what is frequently referred to as “functional reference,” found in monkeys (Cheney and Seyfarth 1990), chickens (Evans *et al.* 1993), and other species: that is, calls (usually warnings) that are taken by some continuists as directly referring to members of other species, even as “proto-words” for

those species. Systems that include “functionally referential” calls are often regarded as more complex (and are certainly richer) than systems that do not.

If indeed there were a “ladder to language,” one would predict that the communication systems of great apes would, among all such systems, most closely resemble language, hence would show more richness and complexity than the systems of animals further from the hominid line. If this were the case, great ape systems would include calls with functional reference. But in fact they do not, even though functional reference is found among monkeys, chickens, and other species much more distantly related to humans.

The fact of the matter is that there is no “ladder to language.” A species communicates about things if and only if its members thereby obtain some selective advantage; its communication therefore directly reflects its adaptive needs. All sexual species must reproduce, so all such species will display some signs expressing receptivity to sex, preparedness for sex, and the like. But not all species need signs to indicate level of aggression or willingness to defend territory, since not all species are territorial or engage in aggressive confrontations. Species that are under heavy predation benefit from having warning calls – calls with “functional reference” – so they have them; the larger primates, on the other hand, have relatively little to fear from predators, therefore such calls have little or no adaptive value. Yet such is the attraction of the “ladder to language” model that these facts, even when directly confronted, can still be rejected: “The apparently greater complexity of monkey calls *must be an illusion*, one that simply reflects our limited understanding of ape calls” (Mithen 2005: 113; emphasis added).

One of the most striking differences between language and the communication of other animals is the difference in their relationship to fitness. All units of all animal communication systems were originally selected for, and continue to exist because, expression of those units under the appropriate circumstances confers inclusive fitness. This applies even for communications that result in the death of the communicator, for even if the animal that gives a warning call falls victim to a predator, close kin who carry the genes that underlie calling behavior will survive and continue to reproduce. However, there is no unit of language (word, phrase, sentence ...) that can, in and of itself, in any way affect fitness, whether individual or inclusive. Language as a whole may appear adaptive in the long run – at any rate, the one species possessing it has achieved a position of unparalleled dominance in nature – but it is too large and vague a category for statements like “language increases fitness” or “language is adaptive” to have much sense; one might almost as well say “communication increases fitness” or “behavior is adaptive.”

24.2.2 Possible candidates for variation

If belief in straightforward continuity between animal communication and language cannot be maintained, are there any aspects of the great ape behavioral repertoire that might have been co-opted for linguistic purposes? Let us consider the possibility that animal communication might have provided some kind of input that could have been utilized for phonetics, for semantics, or for syntax.

Could natural selection have honed the vocalizations of primate ancestors to produce at least the phonetic material that a nascent language (if spoken) might employ? The profound differences between the modern human phonetic inventory and the vocalizations of apes does not argue against this possibility, since we cannot assume that sounds used in the earliest stages of language bore any relationship to modern speech sounds. More telling, if far from conclusive in itself, is the fact that speech sounds and ape vocalizations are produced under the control of quite distinct brain regions (Deacon 1997).

Perhaps most relevant here is that while on the one hand there is no overlap in the phonetic properties of human and ape speech (Lieberman 1968), on the other there are clear continuities between ape sounds and features of the human non-linguistic communication system, such as laughter (Ross *et al.* 2009). It would appear that the latter system is indeed homologous with earlier communication systems, while language represents a genuine evolutionary novelty.

With regard to the meaningful units (words) that speech sounds are used to express, it has been suggested that the predator-warning signals of vervet monkeys (Cheney and Seyfarth 1990), which vary according to whether the predator concerned is an eagle, a leopard, or a snake, are in some sense precursors for human words. For instance, Donaldson *et al.* (2007: 232), while admitting the possibility that “the kind of functional reference we have described here bears only superficial resemblance to the type of referential meaning that words in human language have,” claim that “even a superficial resemblance to a referential system could have provided the conditions necessary for a truly referential system to develop.” But if this is so, it is strange that such signals are found in a range of species (from vervets to chickens) that are phylogenetically remote from humans, but are absent from all of our closest relatives.

Moreover, the assumption that vervet signals refer to particular species such as eagles or leopards is no more than a back-projection of our own language-saturated view of the world. The main functions of these signals are (a) to make others aware of danger and its likely location, and (b) to get others to take the appropriate evasive action. Thus the so-called “eagle” and “leopard” signals are probably interpreted by vervets as things along the line of “Danger from the air!,” “Danger from the ground!,” “Get down from the trees,” “Get up into the trees,” etc., with the identification of the

predatory species being marginal, if an issue at all – in other words, vervet calls are indexical and manipulative, but in no sense referential.

This leaves only syntax as an area of language that might (in some sense) have been anticipated by other species. Any kind of concatenation in animal signals is notoriously rare, but recent research has uncovered a few cases, and these have been hailed by some as “precursors of syntax.” For instance, according to Zuberbühler (2002, 2005), Campbell’s monkeys sometimes preface warning calls with a “boom” vocalization, and this is interpreted by a sympatric species (Diana monkeys) who attend to and repeat Campbell’s monkey calls as indicating that the danger is remote and can safely be ignored. This is a bizarre case, since it involves two species rather than one; more telling evidence comes from studies of putty-nosed monkeys (Arnold and Zuberbühler 2006). This species has two calls, a “pyow” call that warns of the presence of eagles, and a “hack” call that signals the presence of leopards. However, a combination of two to three “pyow” calls followed by up to four “hack” calls led in many cases to subsequent movement of the group.

Note however that (in contrast with concatenated words) the coupling of these signals produces a meaning that has no connection with the meanings of the two calls in isolation. In contrast, what characterizes linguistic utterances of only two units is the fact that they stand in a subject-predicate relationship with one another, so that while one focuses on a particular topic, the other provides information about that topic (*dogs bark, John agreed, temperatures rose*). Moreover, it is surely significant that these behaviors, like warning calls, are found in relatively remote phyla rather than among our more immediate relatives. While such phenomena are of considerable interest in themselves, they show no cumulative increase across species, and can therefore hardly be regarded as steps on the road to syntax. Indeed, since animal signals are complete in themselves, and since each is modeled for use in some very specific situation, it would seem quite pointless to concatenate them in any way that could be properly called “syntactic.”

The foregoing suggests that language could not have been initiated by natural selection working on any of the variations – phonetic, semantic, or quasi-syntactic – found in other species. This should, accordingly, strongly discourage researchers from seeking antecedents to language among the communicative behaviors of other living organisms. It does not, of course, rule out the possibility that natural selection worked upon variation in some species now extinct that was ancestral to modern humans, a point to which I return later.

24.2.3 Possible candidates for selective pressure

As noted above, natural selection requires two components. In addition to variation, there must be some form of selective pressure such that

possessors of some variant(s) increase their representation in the gene pool. In variation, as we saw, there is a dearth of plausible possibilities, but when it comes to selective pressure, the reverse is the case. Over the years, numerous and diverse selective pressures have been proposed. However, the very number of these proposals is enough to excite suspicion. None has shown itself to be more plausible than any of the others, and in every case serious doubts can be raised.

Suggested pressures for language fall into two major classes, those that involved interaction with the physical environment and those that were purely social (for a full list see Johansson 2005). Environmental explanations were most favored until the 1990s; language was seen as being selected for by activities such as tool-making or hunting. An influential essay by Humphrey (1976) shifted the emphasis to social-interaction theories; in an intelligent species, coping with conspecifics was seen as more challenging than coping with environmental factors (Dunbar 1998; Worden 1998).

One important factor that led to this shift was an exponential increase in knowledge and understanding of the great apes. This increase was two-sided. On the one hand, it was found that activities like hunting and tool-making, once thought to be exclusively human, were also carried on by chimpanzees, showing that language was by no means necessary for their performance (Boesch and Boesch 1990; Goodall and von Lawick 2000). On the other, numerous studies showed that ape societies were highly complex and that apes actively sought to determine the intentions and manipulate the behavior of others, demonstrating a well-developed social intelligence (Byrne 1997; de Waal 1995). In consequence, much work since 1990 has seen gossip (as a grooming substitute) (Dunbar 1993), theory of mind (Dunbar 1998, Kwisthout 2006), female choice (Miller 1997), ritual (Deacon 1997; Knight 1998) or other aspects of social interaction as motivating the earliest appearance of language.

One difficulty is that most of these supposed pressures apply equally to a range of other species. Other species make and use tools, engage in female choice, and so on. Any adequate theory of language evolution must explain not only how and why humans got language, but also why no other species has even set out on that particular road. Another difficulty applies to any theory based on social intelligence. The complexity of primate social relations, which initially seemed to favor social-intelligence pressures, turns out on closer examination to do the very opposite. If interactions were already so complex, how would language have facilitated them? If human societies are more complex than those of other primates, this would seem to be due to complexities that result from the possession of language, rather than things that language might have made more manageable. In the absence of any detailed hypothesis as to exactly how any added layer of complexity could have selected for a referential, information-exchanging form of communication – rather

than a meaningless repertoire of pleasing sounds, which would surely equally well satisfy the requirements of a grooming substitute or sexual display – there is really nothing in social-pressure theories that can be helpfully discussed.

Other objections to selective-pressure stories relate to circumstances surrounding the initial use of symbolic units. The first handful of linguistic symbols must have fulfilled some substantive function, otherwise there would have been no motivation to develop more. But for most of the proposed pressures, the utility of such a handful is dubious at best. Moreover, other problems would have had to be overcome, including those of selfishness – why would an individual give away information, a valuable commodity, with no sure prospect of reward? – and “cheap tokens” (Zahavi 1977) – why would a species capable of deception trust things as easy to produce as words? (For a fuller discussion of these problems see Bickerton 2009.)

We can conclude from the foregoing sections that it is by no means easy to explain the emergence of language in terms of natural selection. The situation can be summed up as follows: whatever variation was selected from could not have been found outside the hominid line, and must accordingly have been produced by some species now extinct, while even given the existence of appropriate variation, none of the proposed selective pressures either stands out as superior to others or overcomes all the objections that can be raised. To this last generalization there is one possible exception, which will be discussed in the final section of this chapter.

24.3 Post-origin variations and pressures

There have been a number of arguments against the possibility that, once it had originated, language could have continued to evolve biologically. Deacon (1997) saw language, post-origin, as changing so rapidly that it could not provide a stable target for selection. Lieberman (1989) suggested that the uniformity of grammatical attainment among all mentally normal speakers is such that selection would not have enough variation to work with. However, Deacon’s argument seems to depend on the assumption that language was somehow separate from the minds/brains that developed it, rather than simply reflecting the current capacities of those minds/brains, while Lieberman does not consider the point that, although all normal speakers today might have achieved a similar level of competence, this was by no means necessarily so during the countless millennia that it may have taken language to evolve to its present state.

But if variation did indeed exist during the post-origin development of language, what would have been the variables? Pinker and Bloom (1990) suggest that a parent with a rule of grammar n might (through

recombination or mutation) have had an offspring with a rule $n + 1$ (a similar approach is found in Newmeyer 1991). Quite apart from the fact that this reflects a concept of a grammar as a list of specific grammatical rules, one already outdated in 1990, there are serious problems with this formulation. First, the fact that some variants of grammar might crop up does not entail that they will become fixed, and the authors do not propose any selective mechanism that might have served to fix them. But more importantly, the notion that selection involved particular components of formal grammars seems to lack any empirical or even intuitive support. David Lightfoot (1991: 69) drily remarked that “the Subjacency Condition has many virtues, but I am not sure that it would have increased the chances of having fruitful sex.”

Variation in the expressive power of speakers is a different matter. In a little-known article that preceded the 1990 explosion of interest in language evolution, Burling (1986) put forward two arguments that linked language skills with leadership and increased reproduction. Burling noted that political power very frequently went to the most copious and fluent speakers, and that since, once in leadership positions, such speakers would most likely enjoy longer life and better mating opportunities, they would thereby increase the representation of their language-adapted genes in the population at large. This presumes a type of linguistic variation that, unlike the Pinker/Bloom version, abstracts away from specific formal structures in favor of overall proficiency (which could, though it would not necessarily, involve increased complexity in syntax).

Rather than search for elusive links between formal grammar and evolutionary processes, one should consider what the brain was most likely to do once it had been colonized by words. Brains have evolved for eons to organize increasingly complex behavior. For instance, as sight improved and more complex objects could be distinguished, areas of the brain devoted to vision grew more complex to deal with them. Comparable developments occurred for the other senses. That a similar process occurred with human language, only indirectly helped by the kind of selection that Burling envisaged, seems more plausible, in light of present knowledge, than the Pinker/Bloom model’s more simplistic links between selection and formal structures.

24.4 Is any natural-selection-based account of language potentially viable?

Although the Pinker/Bloom model of language as a direct product of natural selection was initially plausible, two decades of inquiry have shown that any model based on natural selection runs into serious problems. Consequently many, especially in linguistics, have adopted partly or wholly non-selectionist models of language. Since non-selectionist

arguments are dealt with elsewhere, I shall not discuss their pros and cons here, but instead suggest a way – perhaps the only way – in which at least some of the objections to selectionist models could be overcome.

In light of what has been said above, natural selection is unlikely to have led directly to the emergence of language. It is more likely that instead, some particular property of language was selected for, possession of which gave humans a starter menu of quasi-symbolic signs that triggered brain processes for storing, analyzing, and building structure – processes similar to those that had already been used to store, analyze, and build structure from data collected by the various senses. If, instead of seeking homologies, we turn to the analogous developments of convergent evolution and niche construction (Conway Morris 2003, Odling-Smee *et al.* 2003) and look (as recommended by Hauser *et al.* 2002, and as is standard procedure for biologists researching non-linguistic adaptations) for such phenomena in the natural world even beyond vertebrates, we find that, so far as is presently known, only three species have ever incorporated any property usually treated as unique to language into their communication systems. Those three are ants, bees, and ravens, the property in all three cases is displacement – objective reference to things beyond either sender's or receiver's sensory range at the time of communication – and the selective pressure in each case is the same: recruitment for exploitation of food sources too large and/or transient to be exploited by the individual(s) who first located them.

Bees indicate by specialized dances the locations, distances, and qualities of remote food sources (von Frisch 1967, Gould 1976). Ants regurgitate food samples, lay chemical trails, and physically manipulate nest-mates (Holldobler 1971, 1978). Immature ravens inform roost-mates of carcasses guarded by bonded pairs of mature ravens, and the next day a cohort of roost-mates flies to the site and drives off the bonded pair (Heinrich 1991). Intriguingly, though much less is known about the latter case, it incorporates an element – the elimination of scavenging competition – that becomes critical in the case of human ancestors.

Around two million years ago, evidence from changes in foraging patterns (Binford 1985), the sequence of cut-marks and bite-marks on bones (Monahan 1996, Dominguez-Ramiro *et al.* 2005) and practical experiments in using prehistoric tools for butchering megafauna (Schick and Toth 1993), as well as the implications of optimal foraging theory (Stephens and Krebs 1986), indicates that human ancestors began to engage in confrontational scavenging. There existed a brief window of opportunity between the death of a large herbivore and the rupture of its hide by expansion of internal gases, a period during which no competing scavenger could access the vast bonanza of meat that would subsequently become available. However, since stone flakes and hand-axes could be used to open and segment the carcass, pre-humans could exploit this window, provided they could, within a short space of time, summon

sufficient numbers to drive off other scavengers. This would have required the transfer of information about the nature and location of a carcass between individuals who might be removed from it by some miles in distance and an hour or more in time, adding the crucial element of displacement to an otherwise standard mammalian communication system (for a much fuller account of the factors involved, see Bickerton 2009: Chapters 5–8).

Note that this is not a case of natural selection directly selecting for language, but rather of a constructed niche (Odling-Smeet *et al.* 2003) – confrontational scavenging of large carcasses – selecting for a particular type of behavior that involved a major pre-requisite for language – displacement. But this is only to be expected, and indeed should have been suggested earlier, given the problems that attend all proposals for more direct selection of language. Note also that the target achieved by the process might be better regarded as an enhanced communication system than as a primitive form of language. However, once the capacity for displacement, a crucial pre-requisite for symbolic units (words or manual signs as we presently know them) had been implanted in a primate brain – a brain orders of magnitude larger than the brains of ants or bees – it became highly likely that it would be developed and exploited for other purposes besides scavenging megafauna, and that (in ways that remain to be worked out in detail) a true language would eventually emerge from this source. While the particular account above may prove incomplete or unsatisfactory, it seems highly likely that if any viable selective theory of language evolution is to be developed, it must take a similarly circuitous route.

24.5 Conclusion

Given the extent to which Darwinian thought has affected the behavioral sciences over the last quarter-century, it is hardly surprising that theories of language evolution have favored natural selection as a major, if not the only agency. Unfortunately, and perhaps at least in part because of this generational mind-set, selectionist theories have been much too uncritically accepted. Even those who reject them usually do so outright, on principle, rather than providing a detailed analysis of their difficulties from a biological perspective.

At the same time, natural selection remains central to evolutionary theory. Rather than reject selectionist approaches entirely, researchers should at least consider the possibility that, while language may not have been directly selected for in its origin, and while formal features of modern language form unlikely targets for any known selective process at any time, natural selection has indeed worked in a more indirect fashion to bring about the circumstances in which the modern human language faculty could emerge and develop.

25

The fossils of language: What are they? Who has them? How did they evolve?

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25.1 Introduction

Unraveling the origin of language is not an easy task, for even establishing what can reasonably be deemed as a “fossil of language” is a very controversial matter. Some non-trivial issues concerning the nature of homological relations and the form/function problem are especially hard to come to terms with when dealing with anatomical fossils or, more recently, with fossil DNA sequences. When studying language, on whose anatomical and genetic foundations there is no clear consensus, these issues become almost intractable. However, we contend in this chapter that a reasonable picture about when and how language evolved can be attained by putting together (1) the archaeological record and the cognitive capabilities underlying it, (2) cranial fossils and some independently motivated principles governing the organization of brains, and (3) some recently accessible data regarding the evolutionary history of some human DNA sequences. These are, in our opinion, the most reliable fossils of the human faculty of language.

The chapter is organized as follows. [Section 25.2](#) is devoted to the analysis of fossil remains that have been classified in relation with hominid articulatory and auditory abilities. We argue that these data do not touch on core linguistic properties and that they are therefore a poor source of evidence concerning the evolution of modern forms of

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language. We also observe that a certain imbalance between articulation and audition, which is apparent in the analysis of these fossils, could mean that even speech was probably not adjusted for language in species other than modern humans. However, we contend that it is possible to find in the archaeological record rich information about the cognitive capabilities of different hominid species that can be put in relation with the symbolic and computational requirements of the faculty of language. In this respect, the most significant fact is the asymmetry between the remnants of modern humans and other hominids, including those of the Neanderthals. [Section 25.3](#) concentrates on the anatomic correlates of those mental abilities, discussing how they evolved in the brain of early humans. We contend that the incorporation of the “grammar of the basal ganglia” model into the “evo-devo” paradigm and the “size-driven view of vertebrate brain evolution” opens a way to a promissory answer which, we further defend, fits well with the pattern of brain size increase observed throughout the evolution of humans. [Section 25.4](#) explains that such an answer also matches with recent findings concerning both the evolutionary history of certain genomic sequences and the general evolutionary pattern of DNA fragments directly related with the development of the human central nervous system. Some concluding remarks are offered in [Section 25.5](#).

25.2 Fossil evidence of language origins

Language does not fossilize. To study its evolution we must therefore rely on indirect evidence coming from different sources, and one of these sources is the archaeological record and the fossil remains of our ancestors. However, the record does not itself speak ([Mithen 2007a](#)). Thus, its interpretation crucially relies on inferences ([Binford 1989](#): 3; [Fitch 2009a](#): 115), which sometimes may not be entirely well grounded ([Botha 2009, 2010](#)). This section concentrates on the main types of “fossil” evidence (the term “fossil” being used in a broader sense than usually) which have been claimed to help unravel language evolution, and on the shortcomings of many of them. Two core ideas are defended: (1) the types of fossils which may more reliably be linked to language (symbolic or cultural fossils) are absent before *Homo sapiens* (Anatomically Modern Humans; henceforth, AMH); (2) evidence deriving from most of the other kinds of fossils is inconclusive.

25.2.1 Fossil evidence of (full) language

Making “Jespersen’s dream” ([Lorenzo and Longa 2009](#)) true would mean characterizing the prehistory of language through the laws or tendencies

of linguistic change (Jespersen 1922: Chapter XXI). Unfortunately, this is unrealistic: historical linguistics and its methods of internal reconstruction have important shortcomings. Those methods cannot shed light upon language's origins or its evolution (d'Errico *et al.* 2003: 49). In fact, historical linguists (even those who consider long-range comparisons to be attainable; see Ruhlen 1994a, 1994b) are highly skeptical in this respect (Campbell and Poser 2008: Chapter 12; Nichols 1997: 365).

This skepticism is justified for two reasons. Firstly, internal reconstruction suffers from methodological constraints that hinder our look into the past (see Gray 2005). If disagreement exists about current linguistic families and their genetic relationships, it is pretty clear that the hope of explaining language evolution by means of historical linguistics is an illusion. There are several problems involved here: (1) The rate at which languages change is neither constant nor predictable (Dixon 1997: 9; Nichols 1997: 365); (2) according to scholars like Lightfoot (1999), language change is unpredictable, thus lacking directionality; (3) it is often not possible to know whether a resemblance among languages is due to an areal (i.e. borrowing) or a genetic origin (Dixon 1997: 141), or even to a chance resemblance; (4) the search for similarities among languages is usually based on the identification of cognates (lexical similarities due to common descent). Cognates, however, do not inform us about grammatical structure, this level being the most relevant in determining the "shape" of prehistoric languages. Although Nichols (1992) extended the notion of genetic marker to features of language structure, the same temporal restrictions apply, and accordingly, descent and reconstruction are not traceable beyond 10,000 years (Dixon 1997: 2; Nichols 1997: 365), nor reliable beyond 6,000 years (but see Ruhlen 1994a, 1994b). Secondly, reconstruction is a valid procedure only when linguistic change entails a process of divergence from a common (proto)language. However, according to Dixon (1997), this situation is perhaps exceptional: linguistic change would seem usually to take place through borrowing and spreading.

In short, the comparative method is incapable of illuminating language evolution. For language phylogeny to be reconstructed, we need to take into account other kinds of evidence, but see Heine and Kuteva (2007) for a recent attempt at applying the methodology of grammaticalization to the reconstruction of early stages of grammars.

Leaving aside speculative proposals about ancient stages of language in present-day brains (Bickerton 1990) or protolinguistic fossils in today's language (of the type "Agent first," "Focus last"; see Jackendoff 2002: Chapter 8), direct evidence for language is very recent: the advent of writing, which dates back about 5 ka - where 1ka = 1,000 years - (see Daniels and Bright 1996), or perhaps a bit more (Haarmann 1996 claims that writing's origin lies in the pre-Indo-European Vinça culture, near Belgrade, VI millennium B.P.).

According to Donald (1991), three main transitions characterize human cognitive evolution, each of them providing new representational systems. Writing belongs to the third transition, “external symbolic storage,” which enabled external memory to develop through material symbols. Donald does not identify these symbols with writing: he rather points to Upper Palaeolithic visual symbols (some of them much older) as the origin of that stage. It is thus licit to include within such a stage a wider range of objects made of antler, bone or ivory and engraved with marks. They appear to be (abstract) notational systems (Marshack 1991; d’Errico 1999; d’Errico *et al.* 2003; Mellars 2005) used to store and recover information.

D’Errico *et al.* (2003: 31) contend that the geometric patterns engraved on pieces of ochre found at Blombos Cave (Henshilwood *et al.* 2002) are the earliest samples of external memory (while Lewis-Williams 2002 takes them to be the first evidence of art). Later on, since the beginning of the Upper Palaeolithic, true notational systems were developed. It is worth noting that, according to d’Errico *et al.* (2003: 31), these systems clearly demonstrate the existence of modern language, “because modern language is the only communication system with a built-in metalanguage that allows for the creation of symbolic codes.” These scholars point out that notational systems become very complex at the end of the Upper Palaeolithic (see d’Errico 1995), and they go on to suggest a hierarchical organization of information (d’Errico *et al.* 2003: 33). No evidence is found for notational systems before AMH, and this absence fits in well with the absence of symbolic evidence in earlier hominids (see Section 25.2.3).

25.2.2 Fossil evidence of sensorimotor systems

The evidence discussed so far either does not provide any data on language evolution (historical linguistics) or it does so in a very indirect way (notational systems). Other kinds of alleged evidence are also inconclusive. The reconstruction of the Articulatory-Perceptual System from the fossil record is the most obvious case. Two examples are brought to the fore which illustrate pretty well that inconclusiveness.

25.2.2.1 Articulatory system

Much ink has been spilt in discussing the position of the hominid larynx (especially, the Neanderthal larynx) and the shape of the supralaryngeal vocal tract (henceforth, SVT). This discussion rests on the assumption that “speech is special” (but see below), and it takes for granted that AMH underwent a lowering of the larynx (and further re-arrangement of the SVT) which enhanced their articulatory capacities (Lieberman and Crelin 1971; Lieberman *et al.* 1972). Accordingly, many efforts have been devoted to the reconstruction of the Neanderthal vocal tract, in order to know whether this species had a larynx low or high in the throat. The results are completely unclear, though. The vocal tract is made up of soft tissue

and does not fossilize; therefore, reconstructions rely on very indirect inferences, from which conflicting conclusions can arise.

Several reconstructions of the vocal tract of Neanderthal fossils (Negus 1949, Lieberman and Crelin 1971) have made the point that the Neanderthal larynx was positioned higher than in AMH. For example, Lieberman and Crelin (1971) attempted to reconstruct the SVT of the Neanderthal fossil from La Chapelle-aux-Saints. According to them, the Neanderthal airway was similar to that of human newborns and primates, with a high larynx. Thus, Neanderthals lacked an adult human SVT, this limitation preventing them from producing the quantal vowels [a], [i], and [u] (for a modern treatment, see Lieberman 2006, 2007a, and Martínez and Arsuaga 2009, who extend Lieberman's conclusions to Middle Pleistocene fossils in Europe and Africa). Following Lieberman (1991, 2006, 2007a), the human vocal tract shows a 1:1 oral-to-pharyngeal ratio, the horizontal oral cavity (SVTh) and the vertical suprpharyngeal cavity (SVTv) thus being proportional. Because of the length of the Neanderthal SVTh, the SVTv should be as long as the SVTh for the system to be a functionally human SVT (Lieberman assumes that the 1:1 ratio also holds for Neanderthals). Two consequences arise: (1) a long mouth would prevent a human-like tongue (Lieberman 2006: 294), and (2) given that a long oral cavity requires a long pharynx, the Neanderthal larynx should be in the chest, but "no mammal has its larynx in its chest" (Lieberman 1991: 68).

Lieberman and Crelin's (1971) conclusion has been highly controversial (Falk 1975; Le May 1975; DuBrul 1977). Recently, Boë *et al.* (1999, 2002) refuted it on biometric and acoustic grounds: the Neanderthal larynx would not be as high as assumed by Lieberman and Crelin. According to Boë *et al.*, the position of the larynx and the length ratio of the pharyngeal cavity to the oral cavity may be estimated from the cranial-facial geometry. They calculate length ratios from anthropological data, and discover that those ratios show a great variation, although with expected age and gender trends. If such results are applied to Neanderthals, the length ratio and the position of the larynx are within the range of variation of a modern female or a child. Moreover, by using an articulatory model, they conclude that the Neanderthal vowel space was not smaller than that of modern humans. The approach of Boë *et al.* is purely morphological, as they themselves highlight. This means that they cannot answer the question whether Neanderthals really had a modern speech. In any event, their conclusions suggest that Neanderthals were not morphologically handicapped for speech (for further discussion see Lieberman 2007b and Boë *et al.* 2007).

Other scholars agreed with Lieberman and Crelin: Laitman and Heimbuch (1982) or Laitman *et al.* (1978, 1979), relying on correlations between skeletal traits (especially, the basicranium) and the position of the larynx. These papers (and before them Lieberman and Crelin themselves) assumed a correlation between the flexure of the basicranium and the

position of the larynx. Human newborns (and primates) show a basicranium that is not much flexed, and a high larynx; when the larynx descends in the ontogeny, the flexure of the basicranium increases. Accordingly, a Neanderthal shallow basicranium (i.e. not flexed) would mean that the specimen was not able to speak. However, X-ray studies of children (Lieberman and McCarthy 1999; Lieberman *et al.* 2001) showed that the correlation between the lowering of the larynx and the flexure of the basicranium does not hold (as recognized by Lieberman 2007a: 46). Moreover, Heim (1989) considered that the reconstruction of the specimen from La Chapelle-aux-Saints assumed a basicranial flexure that is too small.

The Neanderthal hyoid from Kebara is not informative either. This bone supports the larynx and the tongue root. Because its morphology and size are quite similar to those of a modern hyoid, Arensburg *et al.* (1989, 1990) concluded that the vocal tract of the Neanderthal specimen was modern as well. This statement was strongly criticized (Lieberman *et al.* 1989; Laitman *et al.* 1990): the position of the hyoid bone is not fixed but “moves upward and forward” (Lieberman 2007a: 47) because it is attached to soft tissues. Therefore, its morphology cannot show the shape of the tract or the position of the larynx. That is, reconstructions suggest one thing ... or quite the opposite.

However, this is not the whole story. If reconstructions have been very controversial, the serious application of the comparative method weakens them even more (Fitch 2000a, 2000b, 2002, 2005, 2009a). Traditionally, two models for the position of the mammal larynx were sustained (Negus 1949; Lieberman *et al.* 1969; Lieberman 1984: 273–274): in non-human primates and human newborns the larynx (and the tongue root) is positioned high, whereas it undergoes a lowering in the child. Therefore, the position of the human larynx was taken to be special, in agreement with the special nature of speech (see Lieberman 2003: 258). Accordingly, “The beliefs that a descended larynx is uniquely human and that it is diagnostic of speech have played a central role in modern theorizing about the evolution of speech and language” (Fitch and Reby 2001: 1669). This idea has pervaded Linguistics (Lieberman 1984, 1991; Carstairs McCarthy 1998, 1999; Studdert-Kennedy 1998) and Paleoanthropology (Arsuaga and Martínez 1998: Chapter 16; Chaline 1994: Chapter 12).

However, the idea that “The non-human larynx is positioned high” (Lieberman 2003: 261) has been shown to be false. Recent research (Fitch 2000b; Fitch and Reby 2001) demonstrates that the position of our larynx is not special. The mammal vocal tract is highly flexible and mobile, and the larynx descends during the vocalizations of animals like pigs, dogs, goats, or monkeys, sometimes to a very extensive degree (dog barking). Besides, a permanently descended larynx is not specifically human either: animals like red deer, fallow deer (Fitch and Reby 2001), Mongolian gazelle (Frey and Riede 2003), or several species of the genus *Panthera* (Weissengruber

et al. 2002) also have it. Because these animals do not speak, “a descended larynx is not indicative of speech” (Hauser and Fitch 2003: 165).

Such considerations make it more difficult to reconstruct the morphology of the hominid vocal tract and its speech capabilities (Fitch 2009a: 119). To summarize, according to Lieberman (2006: 292), “Barring the discovery of a frozen Neanderthal preserved in a glacier, this issue may never be fully resolved.” However, as claimed by Fitch (2009a), this discovery would not clarify the issue. On the one hand, because some mammals have a descended larynx but are unable to speak, “even if we discovered (say) a frozen Neanderthal specimen with a descended larynx, we could not necessarily conclude its species possessed spoken language” (Fitch 2009a: 119). On the other hand, “Nor, given the flexibility of the mammalian vocal tract, would a high laryngeal position demonstrate that the Neanderthal didn’t speak: He or she might have lowered the larynx and tongue root dynamically during vocalization” (Fitch 2009a: 133).

The take-home message is clear: reconstructions of the vocal tract face insurmountable problems.

25.2.2.2 Perceptual system

The example discussed in this section (linguistic significance of auditory capacities) shares the same indeterminacy. Martínez *et al.* (2004, 2008) and Martínez and Arsuaga (2009) acknowledge that reconstructions of the vocal tract of human fossils have not offered clear results. Thus, they suggest a new approach: to analyze the hominid auditory capacities (especially, sound power transmission) from a 3D reconstruction of the outer and middle ear of *Homo heidelbergensis* fossils of more than 500 ka found at Sima de los Huesos, Atapuerca.

The results derived from the reconstruction of the audiogram suggest that those hominids shared the U-shaped curve which is usual for modern humans, and which greatly departs from the primate W-shaped curve. This means that both modern humans and *Heidelbergensis* fossils show high sensitivity within the 1 kHz to 5 kHz range of frequencies. On the other hand, chimpanzees have two peaks of high sensitivity (at 1 kHz and at 8 kHz), and a loss of sensitivity between 2 kHz and 4 kHz. From their results, Martínez *et al.* (2004: 9980) conclude that “the skeletal anatomy of the outer and middle ear that support the perception of human spoken language were already present in the SH [Sima de los Huesos] hominids.” That discovery suggests “an ancient origin for human spoken language” (Martínez *et al.* 2008: 4178).

The results attained by Martínez *et al.* are undoubtedly relevant: it is the first time that an aspect linked to hominid sensory perception has been measured. But caution is in order: to have the same auditory capacities as those of modern humans cannot be said to be identical to possessing a human-like language. Although we cannot know what kinds of sounds *Heidelbergensis* (or Neanderthals) actually produced, according to the

audiogram we could suppose that they were able to easily discriminate between [i] and [u], or to distinguish relevant acoustic properties of sibilant sounds like [s]. Unfortunately for this kind of approach to the reconstruction of the speech capabilities of our ancestors, these results raise more questions than they provide answers. Indeed, recall from our discussion above that, as repeatedly pointed out by Lieberman and his colleagues, most evidence appears to suggest that Neanderthals lacked a fully human SVT, a conclusion recently supported by new evidence presented by Martínez and Arsuaga (2009), who extend this conclusion to other Middle Pleistocene fossils in both Europe and Africa. Now, this basically means that these hominids would have had problems with the production of the vowels [a], [i], and [u], which, taken now together with the perceptual data, reveals a fairly peculiar situation indeed, since Neanderthals (and other hominid species, according to Martínez and Arsuaga 2009) would have been able to discriminate sounds ([i] and [u]) that they were not able to produce accurately. As we said, this is certainly strange, since a balance between emission and perception capabilities is the rule rather than the exception in other primates (Cheney and Seyfarth 1990; Hauser 1996). For example, chimpanzee vocalizations contain sounds comparable to [a], [o], and [u], but, as one would expect given their perceptual capabilities, they lack any sounds resembling [i] or [e], since their acoustic features sit within the range of frequencies to which the chimpanzee ear is less sensitive (Kojima and Kiritani 1989).

The key question, however, is whether auditory capacities tell us anything about language. We do not think so. The anatomy of the middle ear is hardly informative about the perception of sounds, and even less about their temporal perception while they are processed. This was already clear for the theoretical musicians of the eighteenth century, who knew that such a capacity lies in the internal ear and the auditory nerve (Rameau 1737). Both the cochlea and the auditory nerve are central for perceiving musical or speech sounds (Moore 1989; Pickles 1988), and, in general, every complex sound. Primates share very similar capabilities in this respect. For that reason, in spite of the sensitivity differences between humans and primates, both show a very similar behavior in categorical perception experiments (Kuhl and Padden 1983; Kojima and Kiritani 1989; Kojima *et al.* 1989; Kuhl 1989; Hauser 1996; for a review, see Hauser and Fitch 2003). Differences derive from the differences in sensitivity, not from how sounds are processed.

The real question is, are the syllables [ba] and [pa] two syllables for a chimpanzee? Undoubtedly, the answer is no: syllables (the same applies to vowels and consonants) are linguistic units (although they may derive from ancient mammal capacities, like chewing; see MacNeilage 1998, 2008). Syllables, vowels and consonants are for chimpanzees and humans different sounds (i.e. different categories), but there is no basis for considering that those categories are “syllable,” “vowel,” or “consonant”

for a chimpanzee. The same reasoning applies to the case under discussion: do the acoustic analyses mean that [ba] and [pa] were actually two different syllables for Heidelbergensis or Neanderthals? They certainly were different sounds, because categorical perception is shared within the primate lineage (and also by birds). But more evidence is needed in order to determine whether what are syllables for us were also syllables for those hominids. Auditory capacities are moot regarding linguistic capacities, and consequently the reconstruction of auditory capacities from fossil remains does not constitute informative evidence for language evolution.

25.2.2.3 Methodological caveats of fossil evidence: The form–function problem

The two examples discussed in Sections 25.2.2.1. and 25.2.2.2. show that the fossil record cannot shed much light on the linguistic or communicative skills of a given species. Certainly, some conclusions derived from the formal analysis of the design of an organ are reliable. For example, that is the case of Weaver and Hublin's (2009) analysis of birth in Neanderthals from the reconstruction of a female pelvis found at Tabun. In this case, inferences about the birth derive from mechanical considerations which have a universal validity.

However, the reliability of inferences is dramatically reduced, as we have shown, when we try to attribute mental or behavioral capacities to a given species from its anatomy or morphology. The reason is that morphological considerations are opaque with regard to the functionality of the structures involved. We shall call this the “form–function problem,” which can be formulated as follows: a species S could have a morphological structure which is related to a function F in other species, but this does not necessarily mean that such a structure is related to the same function in species S.

Let us develop the argument further. The absence of an anatomical support could be used as a proof of the absence of a specific biological function, as formalized in A:

- (A) If an extant species ES has an anatomical structure S related to a function F, and we find a fossil species FS lacking A, we can infer that FS did not have F.

However, even (A) should be taken with caution, as shown by the discussion about Neanderthal speech; let us remember that, according to Fitch (2009a), Neanderthals could have spoken even with a high larynx, in such a way that it dynamically descended during the vocalizations.

But what cannot be maintained is the reverse:

- (B) If we find a fossil species FS with an anatomical structure S similar or equal to S in extant species ES, and S is related to function F in ES, we can assume that FS had F.

To show why (B) is untenable, let us take up again the discussion about the perceptual system, but now from the articulatory perspective. Even assuming the morphological sameness between a Neanderthal vocal tract and a modern one, this does not answer the question whether this tract actually externalized utterances with the properties of linguistic sequences. Assuming that Neanderthals could pronounce [ba] or [pa], the key issue is whether these elements were true syllables for them. It is not possible, therefore, to attribute functions to structures on the sole basis that those structures are shared.

Another example may clarify our claim. Anatomical evidence suggests that pterosaurs had wings. Because the majority of vertebrates with wings fly, we could be tempted to assume that pterosaurs flew. However, wings are not enough for an animal to be able to fly; many other requirements (physical, mechanical, etc.) must be fulfilled. Therefore, pterosaurs could have the relevant anatomical structure for flight and even so they may not have been able to fly: interestingly, this is what Sato *et al.* (2008, 2009) suggest (but see Claessens *et al.* 2009 for the opposite conclusion, although their answer is based on evidence linked to the respiratory system). Be that as it may in this particular case, a morphological trait cannot be adduced as proof for a capacity. Inferences based on arguments like (B) do not have a general validity, and this follows even for seemingly clear cases.

In view of the indeterminacy of the fossil record, Fitch (2009a: 133) suggests that “approaches based on DNA analysis offer far more promise for dating key events in the evolution of speech and language.” However, we believe that the conclusions arising from DNA analysis are not free from the “form-function problem” either. We can illustrate this with an example. Fitch (2009a: 133) quotes a paper by Krause *et al.* (2007) to support his claim. From the analysis of two Neanderthals from El Sidrón Cave (Spain), this paper discovered that the two mutations of FOXP2 believed to be specific to modern humans were shared by Neanderthals. This discovery suggests according to Krause *et al.* (2007: 1911) that “Whatever function the two aminoacid substitutions might have for human language ability, it was present not only in modern humans but also in late Neanderthals.” This rationale is shared by Trinkaus (2007), d’Errico (2008), or Martínez *et al.* (2008): if Neanderthals had the modern FOXP2 version, then they had language (this inference, by the way, relies on the unwarranted assumption that FOXP2 is the unique and direct gene responsible for language). However, Krause *et al.*’s (2007) finding does not tell us anything about the linguistic capacities of that species. In fact, this inference is based on the same type of problematic assumption raised by (B).

According to Benítez Burraco *et al.* (2008), the two aminoacid substitutions could have been selected in Neanderthals, as argued for by Krause *et al.* (2007), but its fixation could be related to selective causes different from those which operated in AMH, that is, to causes unrelated to complex language, a possibility also envisaged by Ptak *et al.* (2009). The key to settle

the function of *FOXP2* in that species is not, as Krause *et al.* (2007: 1911) claim, to get the complete sequence of *FOXP2* in Neanderthals, but rather to obtain information about the genetic context in which the gene displayed its regulatory function in this species (Spiteri *et al.* 2007 for modern humans). Within a different genetic context, it could have helped regulate the development and/or execution of a non-syntactic protolanguage (Mellars 1996a, 1998) or some other form of quasi-musical vocalizations (Mithen 2005), among many other conceivable possibilities.

Relevant with regard to this idea is the fact that different *FOXP2* orthologues have been described for several species and associated with a distinctive ability in each case: bird song (Haesler *et al.* 2004, 2007), ultrasonic vocalization in mice (Shu *et al.* 2005; Fujita *et al.* 2008), or eco-location in bats (Li *et al.* 2007). It also is worth noting the high degree of conservation of this gene (Enard *et al.* 2002), which has experienced very few evolutionary changes within vertebrates. Thus, the ability with which each *FOXP2* orthologue relates in each species is a function of the molecular context in which the protein coded by the gene integrates in each particular case, and not of minor structural modifications experienced by its different variants. Accordingly, the finding of Krause *et al.* (2007) is compatible with alternative interpretations to that of complex language in Neanderthals. In fact, Konopka *et al.* (2009) show that the two aminoacids which differentiate the human *FOXP2* gene from the chimpanzee version change the function of the gene by conferring differential transcriptional regulation in vitro. This paper identified 61 genes upregulated and 55 downregulated by *FOXP2*, compared to the chimpanzee version of the corresponding regulatory cascade. The key would be to know whether in the Neanderthal version the direct *FOXP2* targets were the same, but this information is wanting.

25.2.3 Fossil evidence of conceptualization systems

Although not every aspect of thought requires language (Carruthers 2002), a consensus exists in Paleoanthropology concerning the impossibility of developing complex symbolic practices in the absence of a complex language: “Abstract and symbolic behaviors imply language” (McBrearty and Brooks 2000: 486; see also d’Errico *et al.* 2005: 19–20; d’Errico and Henshilwood 2011; Henshilwood *et al.* 2002: 1279; Henshilwood and Dubreuil 2009; and Renfrew and Morley 2009). We will not question here the validity of this claim (but see Balari *et al.* 2010, 2011 for some critical remarks) and we will limit ourselves to accept and apply the logic of this assumption as it is traditionally elaborated in the fields of paleoanthropology and archaeology. The reasons that are traditionally adduced are that symbolic practices must be supported by a powerful representational (and communicative) system like language, which makes it possible to transcend the here and now (Bickerton 1990; Dennett 1995, 1996, Jerison

1985). Language enables not only displaced representations but also representations lacking any correlate in the world: therianthrope beings, the Beyond, etc. This power is greatly improved by syntax, which generates complex representations by combining simpler ones. Thus, language seems to stress the evolutionary meaning of encephalization: the capacity of processing and interpreting the information an organism has access to, in order to build a mental model of the world (Jerison 1973, 1985). As Jerison (1985: 31) put it, “We create worlds with language.”

Thus, the search for symbolism in the hominid archaeological record could be informative about language. In this sense, given (1) the wide range of symbolic objects associated with AMH, many of them having no correlate in the empirical world (and attested in Africa much earlier than in Europe), and (2) the multiple indicators of modern behavior (McBrearty and Brooks 2000: 492; Henshilwood and Marean 2003: 628; Klein 2009: 742; Tattersall 1998: Chapter 1), also associated to AMH, all scholars agree that AMH possessed a complex (i.e. syntactic) language from very early stages.

AMH's symbolic behavior, which dates back 100 ka or even more, comprises different kinds of objects and behaviors. From Mellars's (2002) typology (see also Bar-Yosef 2002: 365 ff.; Klein 2009: 673; Mellars 2005: 13), we will restrict ourselves to the following ones:

- symbolic burials, as attested by funerary offerings
- symbolic use of ochre and pigments
- personal ornaments (shell beads, perforated animal teeth, pendants, etc.)
- musical instruments
- several forms of art

What about earlier hominids? Are there traces of symbolism in the archaeological remains associated with them? More specifically, what about Neanderthals, who coexisted with AMH in Europe at least for 10 ka? Despite controversial claims to the contrary (Trinkaus 2007; d'Errico 2009; see Balari *et al.* 2008 for a critical appraisal), symbolic evidence is conspicuously absent. We will briefly discuss these in turn.

BURIALS Neanderthals practiced intentional burials, some of them older than 100 ka (Grün and Stringer 2000). However, unlike AMH burials, Neanderthal burials do not include any remains which can be unequivocally interpreted as offerings (Ayala and Cela Conde 2006: 108; Arsuaga 1999: 344; Davidson 2003: 154; Mellars 1996a). Although d'Errico (2003: 196) takes this difference to be “the only difference,” such a difference is crucial: it is the only one which could be the testimony of true complex symbolic behavior, the Beyond (life after death). In the absence of mortuary offerings, an intentional burial is compatible with interpretations lacking a true symbolic meaning, like hygienic reasons, to avoid the presence of carnivores in the cave (Mithen 1996; Klein and Edgar 2002;

Neanderthal burials have always been found within caves), social or emotional links (Mellars 1996a), or the importance of the ancestors (Mithen 1996).

OCHRE AND PIGMENTS The utilization of ochre and pigments was widespread more than 200 ka ago in South and Central Africa (McBrearty and Brooks 2000). However, there are no hints of their symbolic use earlier than 100–90 ka ago (Hovers *et al.* 2003), by AMH at Qafzeh (Israel). d'Errico *et al.* (2003: 55) point out that the first use of pigments where “symbolic behavior can be inferred” is more recent, dating back to 77 ka at Blombos Cave (see Henshilwood *et al.* 2002). Before, ochre and pigments in Africa and Europe “do not, in themselves, signify symbolic behavior” (d'Errico *et al.* 2003: 55). Neanderthals used pigments (for a recent discovery, see Zilhão *et al.* 2009), even intensively in some Mousterian sites (Pech-de-l'Azé, 60–50 ka; see d'Errico 2003); but they were much less selective with the raw materials from which pigments were extracted, and selectiveness is taken to be a key indicator for the symbolic use of pigments to be inferred. Furthermore, Neanderthal pigments were generally black, whereas in Africa the red color is abundant, and this color is taken to be a clear hint of the symbolic utilization of the pigment (Mithen 2005).

ORNAMENTS A wide consensus exists in Paleoanthropology in considering personal ornaments (perforated marine shells or teeth, ostrich egg-shell beads, pendants; for a technical survey of shell objects, see Claasen 1998) as a sign of symbolism and modern cognitive capacity (Henshilwood and Dubreuil 2009: 50, d'Errico *et al.* 2003: 17). The oldest ornaments were made by AMH; for example, the perforated shells found at Skhul, Israel (135–100 ka; Vanhaeren *et al.* 2006), Grotte des Pigeons, Morocco (82 ka; Bouzouggar *et al.* 2007) or Blombos (78–75 ka; d'Errico *et al.* 2005). As for Neanderthals, they did not produce any ornaments before the end of the Mousterian (Arsuaga 1999: 371; d'Errico 2003, 1999; d'Errico *et al.* 2005: 20; McBrearty and Brooks 2000: 521); see, however, Zilhão *et al.* (2009) for an opposite view. Many earlier Neanderthal objects which had been described as ornaments “are actually the result of natural phenomena” (d'Errico 2003: 198). Neanderthal ornaments are restricted to a few late sites, especially Chatelperronian sites (Grotte-du-Renne or Quinçay, France). According to the great majority of scholars, these ornaments are the product of an acculturation scenario, through contact with AMH populations already present in Europe.

MUSICAL INSTRUMENTS Another relevant proxy for symbolic activity is music. There exists a substantial archaeological record of musical instruments made by AMH in the Upper Palaeolithic (Cross *et al.* 2002, d'Errico *et al.* 2003, Fitch 2006), especially bone (and ivory) flutes, some of them dating back to more than 35 ka (Conard *et al.* 2009: 737). This shows “a well-established musical tradition at the time when modern humans colonized

Europe" (Conard *et al.* 2009: 737). No evidence exists for Neanderthal instruments (nor instruments of other species). Many perforated animal bones (especially phalanges) at Middle Palaeolithic sites were considered flutes, but subsequent analyses (see Chase 1990) showed that the perforations were carnivore punctures. The same applies to the putative Neanderthal flute at Divje Babe, Slovenia (50–35 ka; Turk 1997): its holes were pierced by the teeth of perhaps a cave bear (d'Errico *et al.* 1998a; Chase and Nowell 1998). To sum up, evidence of Neanderthal musical instruments "is lacking" (Conard *et al.* 2009: 737; see also Mithen 2005 and d'Errico *et al.* 2003).

ART The origin of art is also clearly associated to AMH. There is no evidence for art in earlier hominids, but with AMH this trait becomes general first in Africa and later in Europe. Many, and very sophisticated, forms of art appear, both of the parietal and mobile kinds (on mobile art, see Barandiarán 2006). Two pieces of ochre engraved with geometric designs found at Blombos Cave, which imply "conventions unrelated to reality-based cognition" (Henshilwood *et al.* 2002: 1279) are considered the oldest art samples (Lewis-Williams 2002; Mellars 2002; Davidson 2003: 155), and the oldest symbols (Mithen 2005, d'Errico *et al.* 2003; but see Henshilwood *et al.* 2009 for some purportedly older engraved pieces of ochre). It is worth noting that art appears in other continents as the African populations reach them (Sanchidrián 2001: 40). This applies even for traditionally unattended archaeological records, such as the Australian one (see Balme *et al.* 2009; Brumm and Moore 2005).

Therefore, symbolic activity (and modern behavior) is linked to AMH and arises in the African Middle Stone Age. This contrast is clearly perceived when comparing Neanderthals and AMH in Europe. Differences between European Middle and Upper Palaeolithic are well known (Bar-Yosef 1998, 2002; Gamble 1999; Klein 1995, 2000, 2009; Klein and Edgar 2002; Knecht *et al.* 2003; Kuhn and Stiner 2006; Mellars 1989, 1994, 1996a, 1996b, 1998, 2002, 2005; Mellars and Stringer 1989; Mithen 1996, 2005; Pfeiffer 1982; Tattersall 1998; White 1982; Wynn and Coolidge 2004, etc.). Behavioral modernity (be it technological, symbolic or social) concurs in Europe with the first appearance of AMH in this area, but it is absent in the many Mousterian sites of the Middle Palaeolithic. With regard to symbols, Mithen (2007b: 321) claims that although some Neanderthal objects have been argued to have symbolic meaning, "these are so rare, so varied in nature, and so unconvincing that they provide an insufficient basis for the inference of symbolic thought." All in all, this suggests a world-view and a cognitive make-up lacking complex language.

Some papers (d'Errico 2003; d'Errico *et al.* 1998b; Zilhão *et al.* 2006) have argued that several late Neanderthal cultures were modern: Chatelperronian (south of France and north of Spain) and its equivalents in other European areas, like Uluzzian (Italy) or Szeletian and Bohunician

(Central Europe). For instance, Chatelperronian sites show elements which were unknown in the Mousterian (blade technology, perforated or grooved personal ornaments, etc.). The aforementioned papers defend an independent invention of such objects, thus assuming that late Neanderthals were both cognitively and behaviorally modern. However, the majority of scholars reject that view (Klein 2003; Mellars 2005; see Balari *et al.* 2008 for a review). It would certainly be odd that Neanderthals, characterized by a static culture for more than 200 ka, became suddenly innovative at almost exactly the point when AMH (among which those objects are perfectly documented) were expanding across Europe. This “impossible coincidence” (Mellars 2005) makes that thesis quite improbable. In addition, doubts have recently been raised about the association between Neanderthals and both Chatelperronian (Higham *et al.* 2010; Mellars 2010) and Uluzzian (Benazzi *et al.* 2011).

When analyzing the hominid archaeological record, it is certain that a methodological caveat should be taken into account: “absence of evidence is not evidence of absence” (Proctor 2007: 752; see also O’Connell and Allen 2007: 405; Guthrie 2005: 23; Mithen 2007b: 320). However, different kinds of proxies indicate a generalized symbolism in AMH but its absence in previous species. That converging evidence seems to suggest that the most parsimonious hypothesis is that modern language (1) is a recent evolutionary outcome linked to AMH, and (2) it appeared in the African Middle Stone Age, perhaps coinciding with the speciation process that gave rise to our species. This means that the archaeological record does not support the gradualist scenario of language evolution argued for by Pinker and Bloom (1990); see Bickerton (2002). If language evolution was gradual, with many intermediate stages of increasing complexity selected according to the fitness they conferred, it would also be expected that the archaeological record would show a gradual emergence of modern behavior and symbolism in different species. However, there are no hints of that before AMH.

It is our opinion that the hypothesis that complex language was absent before AMH, suggested by the analysis of symbolic evidence, could be further strengthened by an examination of the technological domain (Balari *et al.* 2008; Longa 2009; Balari *et al.* 2011). Although the possibility of inferring cognitive and linguistic development from the analysis of the technological record is controversial (Gibson and Ingold 1993: Part V), it is well established that the prehistoric techno-complexes previous to AMH show a strong stasis, i.e. lack of innovations for huge temporal periods. Culture follows a continued and feedback-driven model (Dennett 1995, Dunbar *et al.* 1999, Levinson and Jaisson 2006) as opposed to a static perspective. According to Dennett (1995) language is the key for such a model of change, because (1) it permits us to virtually explore new options, and (2) it is a highly efficient tool for passing on and spreading innovations. As Dennett (1995) puts it, language is the crane of culture. For those

reasons, a complex (i.e. syntactical) language seems a key requirement for non-static cultures. However, all the technological traditions before AMH are very static in overall terms, and this is consistent with the lack of complex language in them. A brief overview will serve as an illustration (for a review of prehistoric technology, see Schick and Toth 1993; Toth and Schick 1993).

The first techno-complex, Oldowan (2.6 my, million years; Lower Palaeolithic and Early Stone Age) is based on a chopper industry which does not shape the stone cores. The Oldowan shows a great uniformity in space and time, such that it hardly shows any relevant improvements for more than 1 my (Leakey 1981; Klein 2009: 256). The same applies to the Acheulean techno-complex (Lower Palaeolithic and Early Stone Age). It appears some 1.65 my ago, and, unlike the Oldowan, cores are carefully carved and symmetrically shaped, as evidenced by bifaces. Acheulean extended for more than 1 my; during this vast period of time, its basic design remained nearly unchanged (Ingold 1993; Mithen 1996; Klein 2000; Klein and Edgar 2002), in such a way that “assemblages separated by tens or hundreds of thousand of years commonly differ little if at all” (Klein 2009: 378). In fact, bifaces are, according to Wynn (1996: 273), one of the greatest prehistoric enigmas, due to the technique’s basic uniformity. Recent remarks about two Acheulean periods for biface carving (bifaces of the second period being more symmetric and thinner; see Klein and Edgard 2002: 108–109 and 141–142) do not contradict the “remarkably conservative” nature of such a complex (Klein and Edgar 2002: 107). Therefore, “artifactual change during the long Acheulean time span was extremely slow, and overall, Acheulean people seem to have been nearly as conservative as their Oldowan predecessors” (Klein 2000: 23).

The Mousterian complex (Middle Palaeolithic and Middle Stone Age) appears around 300–250 ka ago. Its distinctive feature is that core carving (biface) is in general abandoned, and lithic industry becomes based on flakes detached from the core. Mousterian is mainly characterized by the very complex Levallois reduction technique, by which flakes of pre-determined size and shape are obtained through a meticulous previous preparation of the core (Dibble and Bar-Yosef 1995). After its origin (it sporadically appears in the Acheulean, but it only generalizes in the Mousterian) stasis prevails again for 200 ka, in such a way that this complex “is remarkably uniform through time and space” (Klein 2009: 500). To put it in Mithen’s (2007b: 323) words, “The tools they [Neanderthals] made ... 250,000 years ago were effectively no different than those at the moment of their extinction, just under 30,000 years ago” (see Coolidge and Wynn 2004: 61). That is, Neanderthals lack “conscious experimentation and creativity” (Wynn and Coolidge 2004: 476), and it is perhaps not difficult to associate this aspect with the absence of complex language.

However, this picture changes dramatically with AMH. Although they first use Mousterian techniques, they develop in the Middle Stone Age an

unprecedented technology, as testified by South African sites like Klasies River Mouth (elongated blades of the Howieson's Poort industries) or Blombos (microliths). In addition, new raw materials are used to produce tools. The main point is that, contrary to the static nature of prior complexes, with AMH a very quick succession of industries is observed; for example, in the Nile valley six clearly differentiated lithic industries follow one another between 40–17 ka. A quite similar pattern is seen in Europe between 40–11 ka: each stage (Aurignacian, Gravettian, Solutrean, Magdalenian) possesses tools which are absent or only rarely attested in the other stages, and even specific subcultures exist within them (Klein 2009: 662). That is, the rhythm of technological invention is unprecedented (Tattersall 1998: Chapter 1).

Summarizing then, it seems to us that the archaeological evidence makes it difficult to infer the existence of complex (i.e. syntactic) language before AMH.

25.2.4 Fossil evidence of computational capabilities

Archaeological remains have usually been regarded from the perspective of the behavior they may be associated with (symbolic, technological, etc.). It is also possible to consider them from the perspective of the computational processes and capabilities required for their production. Chomsky's hierarchy (Chomsky 1956, 1959b) may be a useful tool for that perspective to be developed (Piattelli-Palmarini and Uriagereka 2005; Camps and Uriagereka 2006; see O'Donnell *et al.* 2005 for its application to non-human species). The hierarchy establishes four levels of increasing complexity: type 3 (finite state systems), type 2 (context-free systems), type 1 (context-sensitive systems, natural language being within this type) and type 0 (unrestricted systems). Given that each level of complexity involves more computational power, “By finding the fossilized remains of such behaviors we can at least surmise the computational complexity in the minds of their executors” (Piattelli-Palmarini and Uriagereka 2005: 41).

Piattelli-Palmarini and Uriagereka (2005) and Camps and Uriagereka (2006) propose an intriguing hypothesis, which could help determine those computational capabilities: to analyze evidence for knotting in the archaeological record from the view of Chomsky's hierarchy. Knots are very complex cognitive operations; consequently, whoever made them could think in context-sensitive terms (and, plausibly, could also externalize that kind of thought). Thus, knotting may be an indirect evidence for fully modern syntactic behavior. According to both papers, a knot requires applying an operation on previously computed operations, which must be kept in the working memory until the overlapping needed for knotting is made (see Camps and Uriagereka 2006: 46–51). That means that a sophisticated working memory is needed for the derivational record to

be kept (see Balari and Lorenzo 2009a for the phylogenetic relationship between working memory and context-sensitivity). Such a procedure, of the type [A₁ |B |C D₁]], implies context-sensitivity in the Chomsky hierarchy sense. The capacity to process that kind of long-distance relationship is a formal property which pervades language (agreement, coreference, displacement, operator-variable relationships, and so on). Therefore, “evidence for knot-tying is computationally equivalent to modern linguistic evidence” (Camps and Uriagereka 2006: 46).

Knots are not attested until 27 ka BP, by means of weaving, but they can be inferred long before (Piattelli-Palmarini and Uriagereka 2005: 41; Camps and Uriagereka 2006: 35). Both papers concentrate on two domains which presuppose knots: (1) purposefully perforated ornaments (beads, teeth, shells, etc.) to be hung to the body or to be sown, and (2) small projectile technology (arrow heads, arches, harpoons). The oldest evidence for those objects is about 90–75 ka (but see Vanhaeren *et al.* 2006). This suggests that AMH “had achieved modern language by that time, with its full-blown context-sensitive potential” (Piattelli-Palmarini and Uriagereka 2005: 42). However, no evidence for knots is found among Neanderthals: Mousterian projectile technology implies hafting, not knotting, and Neanderthals did not make personal ornaments until a later stage, when AMH had reached Europe (Zilhão *et al.* 2009 report the discovery of Neanderthal perforated shells of more than 50 ka found at Avones site, Spain, but the fact that perforations are due to natural causes does not invite us to associate these ornaments with capabilities of such high computational complexity). According to both papers, the absence of knotting may suggest that the Neanderthal mind lacked context-sensitivity, a key property of language. Such a proposal fits in well with other proposals (d’Errico *et al.*, 2005, Henshilwood *et al.* 2002; Henshilwood and Dubreuil 2009) that, for different reasons, sustain the existence of a fully syntactical language 80–70 ka ago.

In addition, the knotting hypothesis converges with frameworks which emphasize an enhancement of the working memory in AMH, like Balari and Lorenzo (2009a), Russell (1996), Donald (1991), or especially, Coolidge and Wynn (2004, 2005, 2009), and Wynn and Coolidge (2004, 2007). Coolidge and Wynn (2004) analyze some objects of the Chatelperronian culture (see above) which are unknown in the Mousterian stage (perforated or grooved ornaments, blade technology, etc.). Many scholars consider that Neanderthals imitated these objects from AMH, who already inhabited Europe. However, Coolidge and Wynn qualify this view with their claim that Neanderthal thought was based on an expert (skilled) cognition, which implies “schemata encoded into long-term memory and activated easily with minimal on-line processing” (Coolidge and Wynn 2004: 62). They go on to argue that “expert cognition lacks the dynamic, on-the-spot problem solving that is enabled by enhanced working memory” (*ibid.*).

According to them, these objects were not imitated by Neanderthals but emulated, meaning that “the subject understands the goal but applies his or her procedure for achieving it” (Coolidge and Wynn 2004: 66). Emulation is the expected procedure if Neanderthals actually had a reduced working memory: they would try to produce these objects according to their own techniques and procedures (see Coolidge and Wynn 2004 on the different technique for the blades, and Balari *et al.* 2008 on the meaning of the abundance of grooved – not perforated – teeth). That is, these objects could be the result of adjusting the pieces to the Neanderthal technical intelligence. One is thus tempted to equate Coolidge and Wynn’s proposal to the absence of the appropriate computational capabilities in the relevant domains as argued for by Piattelli-Palmarini and Uriagereka (2005) and Camps and Uriagereka (2006).

25.3 Brain evolution and the evolution of language

Brain tissue does not fossilize, but cranial fossils provide valuable information related both with the size and structural organization of hominid brains. However, the interpretation of these data is not self-evident without certain assumptions on the significance of the homological relations that can be established between brain structures in different species and on the nature of the form/function relation that holds in any anatomical structure including, of course, brain structures. In this section, we try to make explicit some of these assumptions. We further argue that independently established principles concerning the reorganization of brain structure as a function of its development constitutes another key piece in order to elucidate the evolutionary origins of language. We conclude that once all these pieces are put together a picture of language evolution emerges in which the evidence derived from cranial fossils, archaeological remains, and genomic deciphering and clock techniques fits coherently.

25.3.1 Homologous structures of the neuronal substrate of language

The notion of homology refers to comparable organic characters on the basis of their common development, composition, localization, or connections (*identification criteria*) in species within the same monophyletic group (*homology criterion*) (Hall 1994; Owen 1843, 1848; Wagner 1989a, 1989b). From this definition, one can easily appreciate that this is an “adjustable” concept allowing one to apply either a strict perspective, focusing on phyletic groups with a relatively close common ancestor (e.g., hominids or primates), or a wider perspective, focusing on groups whose common ancestor may be relatively distant (e.g., mammals or vertebrates). Now, research on homologous structures of the neuronal

substrate of language is often based on a strict application of the homology concept, perhaps on the grounds that the human specificity of this biological character does not call for an extension of the comparison beyond the last common ancestor of hominids or, at most, primates. In what follows, however, we will try to show that the quest for homologies of the neuronal substrate of human language may actually benefit from the application of the wider perspective we referred to above (*deep homology*; Shubin *et al.* 1997, 2009; also Shubin 2008 and Gould 2002, Chapter 10, for an overview).

From the point of view of the strictest application of the notion, the search for homologies of the neuronal substrate of language has been centered on the attribution of equivalences to the classical areas for language (Broca's and Wernicke's) to different hominid species, on the basis of data derived from the inspection of prints left by these brain areas in the interior of fossil crania. From this analysis, some authors have felt inclined to attribute to some Australopithecine species the presence of clear homologues of Broca's area, while also finding evidence suggesting a reorganization in *H. habilis* of the parieto-temporal area where Wernicke's area is localized (Holloway 1983; Tobias 1983, 1987). It is not at all clear, however, what the real significance of these findings would be. On the one hand, and applying the "adjusting" principle of homology to the primate lineage, these prints may also be related to homologous areas with no linguistic specialization, such as cortical areas 44 and 45 (Broca) and the temporoparietal auditory area (Wernicke) in monkeys (Preuss and Goldman-Rakic 1991a, 1991b; Pandya 1995; Rizzolatti and Arbib 1998; Petrides and Pandya 1999; Wu *et al.* 2000; Petrides and Pandya 2001; also Striedter 2005, Chapter 8, for an overview). On the other hand, as pointed out for example by Striedter (2005), a much more informative datum concerning the linguistic specialization of these areas would be their interconnection, which, unfortunately, cannot leave any fossil trace, as they are interconnected by non-superficial cortical fibers, and only recently have we come to know that even the most commonly accepted hypothesis about their interconnection is false (Bernal and Ardila 2009).

A possible strategy to avoid this empirical cul-de-sac would consist in paying attention to a different datum, well-attested in the hominid fossil record, despite the fact that its significance is even more debated than that of the putative homologues of the language centers. The evidence in question concerns the successive increments of brain size observed in hominid evolution, both in absolute terms and in relation to body size (*encephalization quotient*; Jerison 1973), a distinction that nevertheless loses much of its force once a comparative approach is applied to species with close evolutionary origins (Striedter 2005).

25.3.2 A (plausible) evolutionary scenario for the neuronal substrate of language

What, then, is revealed by a sequence of fossil crania belonging to different hominid species that either coexisted or succeeded each other in time? Essentially, a series of bursts that affect brain size in the hominid lineage (Collard 2002; Kappelman 1996). The first of these bursts took place during the evolution of *H. habilis*, a species that differs little from Australopithecines in the remainder of its anatomical features. Similar bursts are observed to correlate with the evolution of *H. erectus* (but not with *H. ergaster*, its putative African ancestor), and with that of the common ancestor to *H. neanderthalensis* and *H. sapiens sapiens*. Although it is often assumed that brain size constitutes too rough a kind of evidence (Ayala and Cela Conde 2006), and that its actual correlation with the evolution of some particular aspect of cognition (or with cognition as a whole; Falk 1990) is dubious, our understanding is nevertheless that brain size is highly significative in the context of the “size-driven view of vertebrate brain evolution” (Striedter 2005). This framework makes it possible to delineate a plausible scenario for the evolution of language in such terms that the data presented in Section 25.2, far from being disproved, may instead be of some significance.

This point of view of brain evolution is based on two fundamental theses: (1) all brains follow a highly conservative basic structural type; and (2) brains tend to reorganize themselves internally in characteristic ways and according to variation in terms of size. As for the second thesis, some principles of brain reorganization that have been proposed are, for example: that brains tend to be subdivided into more areas or activity nuclei as they grow in size (Ebbesson 1980), and that each of these areas tends to show a higher degree of lamination than the one that precedes it (Bullock and Horridge 1965; Strausfeld et al. 2000); that brain growth tends to foster the mutual invasion of areas and nuclei of activity as these acquire larger proportions (Deacon 1990a, 1990b); or that late-maturing brain areas tend to grow larger (Finlay and Darlington 1995). As for the first thesis, and for the purposes of this section, it is particularly relevant to emphasize the distinction between the basal and the cortical components of the brain, while keeping in mind that the equivalent in terms of activity of the latter (see below) often is a highly hypertrophied component of the pallium with which basal ganglia establish most of their connectional pathways – thus, for example, the dorsal ventricular ridge in birds, which is anatomically homologous to the amygdala and the pallial claustrum in mammals, of which the neocortex is an integral part (Striedter 2005).

The preceding discussion suggests an evolutionary scenario for the neuronal substrate of language that may easily be summarized by saying that language is an effect of increase in brain size at a critical point during

hominid evolution. This critical point meant, on the one hand, the habilitation of the cortical mass required for executing the necessary computations (Section 25.2.4), and, on the other hand, the overlapping (via *connectional invasion*) of such greatly disjoint brain areas. From this time on, these areas were capable of performing a qualitatively new kind of mental activity, giving rise to new and unique forms of behavior in the organic world (Section 25.2.3) (Balari and Lorenzo 2008, 2009a). The data discussed in Section 25.2 suggest that this whole process of brain reorganization with the mentioned consequences culminated no earlier than the emergence of AMH.

25.3.3 The evo-devo paradigm, the evolution of the brain, and the evolution of language

The scenario sketched above fits naturally in the scientific paradigm of what nowadays is known as evolutionary developmental biology (evo-devo; Carroll 2005b; Hall 1999; Minelli 2007). The basic premises of this framework may be summarized by the idea that evolution cannot be seen as a constant diversification process affecting adult phenotypes only, but as a process of diversification and substitution of the developmental patterns leading to these phenotypes. Paraphrasing the words of William Bateson (1894), phylogeny is not a sequence of adult organisms, but a series of complete ontogenies succeeding each other in time. More concretely, nowadays we are in a position to state that the idea that the phylogeny of the human brain may be explained as the result of a number of perturbations affecting its developmental pattern is far more than speculation.

In the development of the human brain we observe a general pattern of late offset of the process (*peramorphosis by late offset* or *hypermorphosis*; Gould 1977; McKinney 2000; Parker and McKinney 1999), which can be held responsible for its relatively large size with respect to other, closely related species, and which triggers a series of reorganizational events like those described in Section 25.3.2 (Balari and Lorenzo 2009a). Indeed, we first appreciate a late offset of the symmetric cellular division phase of neuronal precursors, which gives rise to an overproliferation of these precursors (Rakic and Kornack 2001); we observe, moreover, a late offset of cylogenesis, the phase starting with the onset of precursor production and ending with the phase where a maximum number of divisions obtains and cells do not divide further but migrate to the forming cortical structure (Kaskan and Finlay 2001). This late offset of cylogenesis causes an exponential growth of the cortical structure, while, at the same time, basal structures and the medulla only grow linearly. Concerning the developmental pattern of human brains, other phenomena have also been described associated with this hypermorphic tendency, such as the extension of fetal growth, of cortex myelinization (especially in the frontal

cortex), and of dendritic growth (Deacon 2000a). Taken together, these data suggest that during the evolutionary configuration of the human brain, some specific mutations affecting genes involved in the regulation during development of such processes as proliferation, division, migration, and growth of neural cells must have been crucial. In Section 25.4.1 we point out some possible candidates.

It is also important to assess the role of the reorganizational processes that, inevitably, accompanied this growth, such as the subdivision of the cortex in well-differentiated areas, a streamlining of lamination, area overlapping, and the cortical invasion of basal and medullar structures, because it cannot have been the consequence of natural selection acting over randomly generated variation, but a direct and spontaneous effect of the critical cortical mass attained at some point in brain growth. This is not to exclude that such effects may later have been the target of selection of the genetic variants capable of stabilizing and consolidating these results, independently of those attained through phenotypic plasticity (West-Eberhard 2003). In the light of this discussion, one is tempted to attribute the remarkable difference between cognition and behavior in *H. neanderthalensis* and in *H. sapiens sapiens* (see Section 25.2) precisely to these genetic accommodation processes, given the fact that such differences are not expected on the basis of brain-size alone (Collard 2002). In contrast, some recently unveiled disparities in subtle anatomical details (Peña-Meilán *et al.* 2011) and in the early developmental path (Gunz *et al.* 2010) of the organ in both species seem more congenial with our approach.

25.3.4 The evolution of the neuronal substrate of language: the form–function problem revisited

Even with all that has been said above, we are still in the dark as to how a complex linguistic faculty actually emerged in the human brain, although we are now in a much better position to point at some factors that may have played a crucial role in this process.

Firstly, the growth of the cerebral cortex may have provided the necessary working memory space to carry out the complex computations required for language, such as the hierarchical structuring of expressions and the introduction of long-distance and crossed dependencies between elements (Balari *et al.* 2011; Balari and Lorenzo 2008, 2009a; Uriagereka 2008). Essentially, the preceding discussion would imply that, once a critical point was attained, the sequences generated in the basal ganglia (a relatively conservative component; Cummings 1993) could have been subject to mental manipulations well beyond a strictly lineal computational regime. This idea, deeply indebted to Lieberman's (2006) model of the grammar of the basal ganglia, is not at odds with similar events in other species, and it is thus entirely compatible with the possibility that a hypertrophy of the brain, not necessarily affecting cortical areas (see

[Section 25.3.2](#)), could have given rise to an extended working memory space. In fact, this could be the case, for example, with some avian species (Balari and Lorenzo 2008, 2009a) with respect to their construction behavior (Hansell 2000).

Secondly, the stabilization and consolidation of the overlapping of certain structurally and functionally independent areas brought about by brain growth could have had the effect of transforming the described computational system into a properly linguistic system, capable of acting as an internal communication pathway between sensorimotor specialized areas and those areas devoted to conceptualization and intention fixation. It is this latter aspect that endows the system of linguistic computations with an unequivocally linguistic character, in the sense that other similar systems, such as the one underlying building or nesting abilities in some bird species, although showing comparable computational capabilities, appear to instantiate a much more localized and specific kind of connectivity with the overall brain architecture (Balari and Lorenzo 2008, 2010b).

The ideas we have just sketched out have some additional and important consequences taking us back to [Section 25.3.1](#). Even though no clear fossil evidence exists, it appears nevertheless to be true that language is not a true exception in the natural world, and that its homologues may be found in the most unexpected places within the animal lineage. The key to accepting this contention presupposes not paying too much attention to what other animals actually do when it comes to understanding where language might have come from, but rather to how they are structured and how development structured and restructured their brains (Balari and Lorenzo 2010a, 2010b). Once this point of view is accepted, we immediately see deep homologies across the whole organic world, homologies that we analyze from a molecular perspective in the following section.

25.4 Molecular aspects of language evolution

The evidence discussed in [Sections 25.3.2](#) and [25.3.3](#) points to the need to determine whether the changes that took place in our evolutionary lineage with respect to brain size and the interconnection patterns in specific cortical and subcortical regions – presumably as a consequence of modifications affecting the brain's developmental itineraries, with all of the crucial implications of these changes for the emergence of language ([Section 25.3.4](#)) – may eventually be associated to specific alterations in the molecular mechanisms involved in the regulation of the brain's ontogeny, and particularly to the positive selection of concrete changes in the sequence of some specific genes.

25.4.1 The evolution of (some) genes and the evolution of language

25.4.1.1 Genes involved in brain size regulation

There exists good evidence suggesting that genes may play an important role in the regulation of brain size, in particular: (1) the remarkable heritability that characterizes this parameter, which may be up to 90% in the case of the whole brain and well beyond 80% for neocortical grey matter (Baaré *et al.* 2001); (2) the presence of recurring patterns in cortical histogenesis (Felleman and Van Essen 1991); and especially, as we discuss below, (3) the confirmation that the mutation of certain genes gives rise to a reduction of brain size.

During embryonic development, neurons are generated from precursors located in the pseudostratified epithelium. As pointed out in Section 25.3.3, their population grows exponentially through successive cycles of symmetrical divisions; a small alteration of the existing balance between the factors determining the precursor's polarity (and hence that of the pattern of mitotic division) causes a change towards an asymmetric cellular division, which regenerates the precursor but also originates a neuron (Woods 2004). A number of genes have been characterized as involved in this phase of the process of neurogenesis, whose mutation brings about a reduction of brain size known as microcephaly. But, whereas in some cases the mutation of the gene affects only the proliferation of neuroblasts, and therefore its phenotypic manifestation is essentially constrained to microcephaly, in other cases as well certain additional neuronal developmental programs are altered, such that not only is the brain reduced in size, but its folding pattern also becomes modified (Barkovich *et al.* 1998; Sztriha *et al.* 1999; ten Donkelaar 2000). Contrary to what has been the case with the genes in this second group, some specific mutations occurring in the sequences of genes in the first group have been the focus of positive selection within our lineage (Gilbert *et al.* 2005). Current evidence concerning two of the better characterized genes of the last type, *MCPH1* and *ASPM*, suggests that this process could be characterized as an instance of "mosaic evolution," since some of the functional domains in the corresponding proteins have been subject to a significative evolution, while others remained virtually unaltered (Evans *et al.* 2004a; Evans *et al.* 2004b; Kouprina *et al.* 2004; Wang and Su 2004).

MCPH1 appears to be involved in DNA repair (Xu *et al.* 2004) and in the regulation of chromosome condensation during the cellular cycle (Trimborn *et al.* 2004), such that its mutation would have effects on brain size through an increase in the rate of cell death during neurogenesis (Gao *et al.* 1998) and/or a reduction of the proliferation of neuronal precursors during embryonic development (Hakem *et al.* 1996). This gene has undergone positive selection, which has been particularly strong within the primate lineage that gave rise to Old World monkeys and higher apes (Evans *et al.* 2004b; Wang and Su 2004). To this circumstance we should

add the additional particularities that the most frequent haplotype in human populations (D) has undergone a very recent positive selection and that it could have arisen in an evolutionary line of hominids that diverged from the one that eventually led to *Homo sapiens* around 1.1 my ago; accordingly, its current presence in our genetic pool could be the result of a later introgression that occurred some 40 ka ago (Evans *et al.* 2006). In any case, there does not seem to exist in modern human populations any remarkable correlation between the presence of this allele and specific relevant cerebral features, either morphological or cognitive, such as brain size (Woods *et al.* 2006) or intellectual quotient (Mekel-Bobrov *et al.* 2007).

ASPM is involved in the maintenance of a symmetrical division pattern of neuronal precursors (Woods 2004), plausibly due to its direct implication with the regulation of cytogenesis, as is the case in *Drosophila* (González *et al.* 1990; Do Carmo Avides and Glover 1999; Riparbelli *et al.* 2002). Certain modifications that occurred in its sequence appear to have been selected after the human lineage branched from the evolutionary line leading to higher primates (Bond *et al.* 2002), although probably before the out of Africa migration of *Homo sapiens* (Zhang 2003). For this reason, some have speculated about the possibility that the modification of this gene might correlate with some of the acceleration phases of brain growth referred to in Section 25.3.2. As is the case with MCPH1, this gene appears to be still evolving today (Mekel-Bobrov *et al.* 2005; Yu *et al.* 2007), although in this case also there seems to be no positive correlation between certain haplotypes and certain parameters linked to brain function and structure (Woods *et al.* 2006; Mekel-Bobrov *et al.* 2007).

Two more genes have also been subject to positive selection in the human lineage, i.e. CDK5RAP2 and CENPJ (Dorus *et al.* 2004), with the particularity that, at least in the latter, the highest frequency of non-synonymous substitutions has been detected in the terminal lineages of humans and chimpanzees (Evans *et al.* 2006). While the function of the first remains unknown (Ching *et al.* 2000), the second seems to encode a protein involved in microtubule nucleation (Hung *et al.* 2000; Hung *et al.* 2004).

These four genes are responsible for a specific subtype of congenital microcephaly known as primary microcephaly or *microcephalia vera*, where severe neurological alterations or dysmorphic features are characteristically absent (Dobyns 2002), and in which the abnormal reduction in cortical volume is specifically due to a decrease in the number of neurons (Woods 2004). The atavistic character of the phenotype associated with their mutation is unquestionable. Moreover, the fact that this mutation is linked to a noteworthy impairment of the subject's cognitive capacities (Dolk 1991; Cox *et al.* 2006) and that the reduction of brain size does not substantially affect the histological organization of the cortex make these genes ideal candidates at the time of inferring the first hominids' cerebral

characteristics and how these may have been modified within our evolutionary lineage (Ponting and Jackson 2005).

The case of *SHH* is, however, slightly different, since individuals carrying point mutations in this gene present a much more severe brain malformation than microcephaly, known as holoprosencephaly, which is plausibly caused by an abnormal development of the ventral portion of the neural tube (Roessler *et al.* 1996). Although the gene is highly conserved (Marigo *et al.* 1995), it has nevertheless also been subject to positive selection within the primate group (Dorus *et al.* 2004). The gene encodes one of the inductive signals responsible for regulating embryonic organization and morphology during the earlier stages of development, including, significantly, the morphogenesis of the ventral region of the neural tube (Echelard *et al.* 1993; Roelink *et al.* 1994). The gradient of the *SHH* protein modulates the expression pattern of several other genes that encode homeotic transcription factors, eventually contributing to the differentiation and the specification of the identity of several types of neuronal precursors (Ericson *et al.* 1996; Wilson and Maden 2005). This gene also seems to participate in the regulation of axonal growth and synaptogenic processes involved in the establishment of certain specific neuronal circuits (Bourikas *et al.* 2005; Salie *et al.* 2005).

Other genes exist associated with neuronal proliferation which display an accelerated evolution in the human lineage, in particular (i) *CASP3*, which encodes a protease involved in the activation of apoptosis during the multiplication of neuronal precursors (Kuida *et al.* 1996), and (ii) *ADCYAP1*, which appears to contribute to the modulation of these precursors' transition to differentiated neurons (Dicicco-Bloom *et al.* 1998; Mercer *et al.* 2004) and whose mutation impairs some specific psychomotor capacities, at least in certain model organisms (Hashimoto *et al.* 2001). The rate of non-synonymous substitutions in the coding sequence of the latter gene appears to have increased significantly after the branching of the evolutionary lines leading respectively to our species and to chimpanzees (Wang *et al.* 2005).

It has been suggested that the increase in brain size that has gone on throughout our evolutionary history would not have been due solely to the mutation of genes specifically involved in the regulation of brain development, like the ones so far described, but probably also to other kinds of processes that would have fostered and/or caused it, in particular to a hypotrophy of the masticatory muscles with the net effect of altering the cranio-facial morphology and reducing the tension exerted by the muscle fibers on the cranial sutures, which coincide with the growth points of the immature neurocranium (Warren *et al.* 2003). The inactivation of *MYH16*, which in the other primate species is expressed in the muscles derived from the first pharyngeal arch, that is, the masticatory muscles (Stedman *et al.* 2004), has been pointed out as a possible cause of this hypotrophy. *MYH16* encodes the heavy-chain of the predominant myosin in these muscles, such that its inactivation would bring about a notable reduction in the thickness of the

muscle fibers and, consequently, in the size of the muscle itself (Stedman *et al.* 2004). The mutation that inactivated the gene occurred some 2.4 my ago, simultaneously with the appearance of the genus *Homo*, in which the facial and cranial features associated with a reduced masticatory musculature can already be observed (Walker and Leakey 1993).

25.4.1.2 “Genes for language”

The complex process responsible for the structural and functional development of the brain, and which starts with the proliferation of neuronal precursors, is completed by the migration and the morphological and functional specialization of the neurons that make it up, as well as with the ultimate establishment of the necessary connections among them. There are many genes involved in the regulation of such processes (see Benítez Burraco 2009 for a review). Simultaneously, the genetic analysis of hereditary cognitive disorders affecting language has made possible the identification of numerous genes that participate in a more or less direct manner in the organization and functioning of the neuronal circuits devoted to linguistic processing. Some specific modifications observed in the sequences of some of these genes have also been subject to positive selection during the recent evolutionary history of our species, such that it may well be possible that these changes have played an important role in the evolution of the neural substrate of language. Moreover, in many cases the homologous and orthologous genes are involved in the development and functioning of neuronal structures presenting an unmistakable evolutionary continuity with specific components of language and which therefore have received special attention in deep homology studies.

In this connection, obviously those genes whose mutation appears to cause specific language impairments (in the sense that they are not linked to neurological dysfunctions, mental retardation, broad cognitive deficits, hearing deficits, or an inadequate or insufficient exposition to linguistic stimuli during development) are of particular interest, in spite of the fact that such specificity may be questionable at different levels of biological complexity. Among them, the evolutionary path followed by *FOXP2*, considered the “gene of language” *par excellence*, is the one which has recently deserved most attention. The gene encodes a transcription factor that appears to be involved in the regulation of the necessary neuronal differentiation for the organization and/or the functioning of certain cortico-thalamic-striatal circuits associated with motor planning, sequential behavior, and procedural learning, and whose mutation gives rise to deficits of a varied nature and range, but with both a motor and a linguistic character at the same time, affecting equally the receptive and expressive components of language (for a review, see Marcus and Fisher 2003; Vargha-Khadem *et al.* 2005).

In spite of being highly conserved in evolutionary terms, within the line leading to the human species the gene has suffered two mutations that

have given rise to two non-synonymous changes in the sequence of the protein it encodes. As already noted in [Section 25.2.2.3.](#), the timing and the evolutionary meaning of these mutations are not at all clear. However, consideration of the structural and functional properties of the corresponding orthologous structure present in other (model) organisms is contributing decisively to elucidate the function and the evolution of this gene, and in particular to determine which of the previously discussed scenarios is the most plausible one. In this respect, certain songbirds are of a particular interest. Altogether, this kind of evidence appears to confirm: (1) the phylogenetic conservation of the structures and neuronal circuits with which the activity of the *FOXP2* protein appears to be related; (2) the fact that the gene would not only be involved in the development of these circuits during the embryonic stage, but also in their functioning in the adult organism; (3) the hypothesis that the gene's mutation (and the decrease in its expression levels) gives rise to dysfunctions in basic neurobiological processes (neuronal migration, synaptic plasticity, synaptic integration); and (4) the hypothesis that these dysfunctions eventually bring about disorders that are not just of a motor but also of a behavioral/cognitive nature ([Benítez Burraco 2008](#), for a review). The data we have to date jointly suggest that the *FOXP2* transcription factor must be very old in evolutionary terms and that it plays a crucial (and substantially similar) role in all vertebrates with respect to the regulation of both the development of brain architecture (already at the initial phases of embryogenesis) and the functioning of certain neuronal circuits in the adult stage ([Bonkowsky and Chien 2005](#)). However, these data also suggest that, simultaneously, this regulatory network might have been reused (with slight modifications) in some species with different purposes. If *FOXP2* has played a relevant role in the emergence of language (and that could be the case), this role would not only be connected to a modification of the mechanisms associated with the regulation of the development of certain brain regions (and their structural and functional compartmentalization), but also the regulation of those involved in the modulation of neuronal plasticity associated with the learning of motor tasks ([Fisher and Scharff 2009](#); [White 2010](#)).

For reasons similar to those adduced in the case of *FOXP2*, the analysis of the evolutionary history of genes linked or associated to dyslexia is also particularly interesting, specifically that of *DYX1C1* and *ROBO1*. The former has accumulated a significant number of mutations after the separation of the evolutionary lines leading, respectively, to humans and the rest of the higher primates ([Taipale et al. 2003](#)), whereas the latter appears to have been subject to positive selection between 12 and 16 my ago, that is, around the time that the branch of orangutans separated from the lineage of gorillas, chimpanzees, and humans ([Hannula-Jouppi et al. 2005](#)). The case of *ROBO1* is particularly significative in connection with our discussion in [Section 25.4.1](#), as in this gene also some differences are observed

between humans and chimpanzees with respect to the splicing patterns of mRNA (Calarco *et al.* 2007). It is worth mentioning that other genes involved in the regulation of axonal growth seem to have been positively selected in the human lineage, as in the case of *AH11*, whose product appears to participate in the redirection of axons from the brain to the spinal cord and whose modification might be related to the evolution of specific motor programs in our species (Ferland *et al.* 2004).

Several pieces of evidence point to the need to also consider, in any genetic characterization of language, those genes implied in the development and functioning of the brain whose mutation simultaneously impairs linguistic competence and other cognitive capacities. This considerably larger group encompasses numerous genes that have also experienced a positive selection within the human lineage. Among them, the case of *LIS1* (*PAFAH1B1*) is particularly relevant. This gene is involved in the regulation of neuronal migration and axonal growth (Smith *et al.* 2000; Toyo-oka *et al.* 2003; Shu *et al.* 2004), and its mutation gives rise to a reduction of brain size and, in particular, to different types of lissencephalies (Reiner *et al.* 1993; Lo Nigro *et al.* 1997) with different degrees of impact on linguistic abilities (Leventer *et al.* 2001).

Another particularly remarkable case is that of different typologies of neurotransmitter receptors. Thus, there is evidence of an evolutionary acceleration in the sequences of genes such as *CHRM2*, which encodes an acetylcholine receptor of the muscarinic type; *DRD2*, which encodes a member of the dopamine receptor family, and *GRIK4*, which encodes a member of the kainate subtype of glutamate-gated ion channels (Dorus *et al.* 2004). The mutation of similar genes significantly affects the development and the functioning of language, and causes cognitive deficits in which the impairment of linguistic competence is a relevant symptom. Thus, for example, *CHRNE*, which encodes the e subunit of acetylcholine receptor at mature mammalian neuromuscular junctions, is a possible candidate for Asperger syndrome (*locus ASPG2*) (Tentler *et al.* 2003). *DRD4*, on the other hand, which encodes the dopamine D4 receptor, is also one of the loci for dyslexia (DYX7) (Hsiung *et al.* 2004) and for the attention-deficit-hyperactivity disorder (McCracken *et al.* 2000; Schmidt *et al.* 2001). Similarly, *GRIK2*, which encodes the GluR6 subunit of the kainate-type glutamate receptor, is one of the candidate genes for autism (Jamain *et al.* 2002), while certain polymorphisms of the gene have been correlated with the early onset of Huntington's disease (Rubinsztein *et al.* 1997), a pathology that, as is also the case with the phenotype associated with mutation of the *FOXP2* gene, carries linguistic and motor impairments (Cheney *et al.* 2002; Teichmann *et al.* 2005; Gusella and MacDonald 2006). Significantly, *GRIK1*, another gene similar to *GRIK4*, is one of the targets of the *FOXP2* factor whose expression pattern has been modified throughout the evolution of our species (Spiteri *et al.* 2007).

25.4.1.3 Other genes of interest

In addition to what we have discussed so far with respect to the increase in brain size and the (re)configuration of the circuits implied in linguistic processing, during human speciation other processes have taken place that could have played a significative role in the evolution of language, in particular (1) the modification of brain metabolism and homeostasis, and (2) the strengthening of the brain's functional lateralization.

As for the first point, on the one hand one must take into account that a more complex brain needs a higher energetic supply for its proper functioning, which would explain why, among the genes that have experimented a positive selection during human evolution, we find in particular those encoding different components of the mitochondrial electron transport chain (Grossman *et al.* 2004); significantly, there exist a number of diseases caused by the mutation of several of these genes, which include, among their distinctive symptoms, certain language impairments and deficits. Also in connection to energy supply, the APOE gene should be mentioned. This gene encodes a cholesterol transporter which plays a key role in axon elongation and myelinization (Mahley 1988). Its $\epsilon 3$ allele, in particular, appears to have played a crucial role in the full adaptation to a meat-rich diet by providing some protection against specific pathological consequences of meat eating (e. g. hypercholesterolemia) and seems to have spread in the same time range as the FOXP2 gene within the genus *Homo* (Fullerton *et al.* 2000; Finch and Stanford 2004). Interestingly, another allele of this gene ($\epsilon 4$) has been associated with a decline in different cognitive functions (Caselli *et al.* 2004), and particularly with sporadic (i.e. non-familiar) forms of Alzheimer's disease (Raber *et al.* 2004).

On the other hand, it seems to be the case that a more complex brain also needs an optimization of the intra- and interregional communication devices, which would explain why some genes involved in the regulation of the homeostasis of certain neurotransmitters have also been subject to positive selection during our evolution. A particularly interesting case in this connection is that of GLUD2, which encodes an alternative glutamate dehydrogenase to the one present in the other mammals (encoded in our species by GLUD1), which is presumably adapted to the biochemical environment found in a metabolically more active brain, particularly in astrocytes under high neuron firing (Plaitakis *et al.* 2003). The gene would have appeared through duplication (in fact, by retroposition of the spliced mRNA of GLUD1) just before the appearance of primates as a group, and it would have been subject to a subsequent positive selection in the ancestors of higher primates (Burki and Kaessmann 2004). Moreover, the functional optimization of this more complex brain could also have been the result of some modification in the glycosylation patterns of (certain) cerebral proteins: it has been suggested that one way to attain this result could have been the inactivation of CMAH, a gene which encodes one of the

enzymes involved in the transfer of sugar residues to glycoproteins; such inactivation would have taken place just a little before the encephalization process occurred in the genus *Homo* between 2.1 and 2.2 my ago (Chou *et al.* 2002).

As for lateralization, its main interest resides in the fact that linguistic processing is substantially lateralized in functional and anatomic terms (Martin 2003). It is nevertheless important not to forget that functional lateralization is an archaic feature in vertebrates (Bradshaw and Nettleton 1981), whereas anatomical lateralization of the most relevant regions involved in language processing (in particular, Broca's area) is already present in other primates (Cantalupo and Hopkins 2001; also Section 25.3.1); moreover, linguistic activity regularly implies the recruitment of different areas in the right hemisphere (Martin 2003). In any case, and given the existing positive (although not particularly significant in statistical terms) correlation between language and the lateralization of certain activities that might share with linguistic processing (also in phylogenetic terms) a (homologous) subjacent neuronal mechanism (as would be the case for motor tasks responsible for object manipulation), Leask and Crow (2001) have proposed that one of the relevant genes for language evolution could be one of the genes involved in the regulation of lateralization, i.e. the protocadherine 11 gene, a type of protein involved in cell adhesion. There are two variants of this gene, one linked to X (*PCDHX* [*PCDH11X*]), and the other linked to Y (*PCDHX* [*PCDH11Y*]), with the particularity that the latter is the result of a transposition of a copy of *PCDHX* (*PCDH11X*) to chromosome Y, which occurred after the branching of the evolutionary line leading to the human species (Sargent *et al.* 1996), whence its absence in other primate species (Wilson *et al.* 2006) and these do not show the sexual disparity in the degree of lateralization nor, consequently, in the degree of manual dexterity, observed in our species.

25.4.2 The evolution of genes related to language: The form–function problem re-revisited

In the light of our discussion in Section 25.4.1, we are now in a position to offer a positive answer to the question posed in Section 25.3.3 as to whether the mutation of certain genes associated with neuronal proliferation, migration, and interconnection could have played a relevant role in the evolution of the brain and, specifically, in the emergence of language.

A plausible scenario could be the following. In the primate clade, a number of mutations would have accumulated in genes involved in the regulation of basic brain processes (such as neuronal proliferation and migration, the establishment of basic brain interconnection patterns, brain metabolism, the homeostasis of certain neurotransmitters, or the posttranslational modification of certain cerebral proteins), which would

have given rise in turn to fundamental changes in brain anatomy and physiology. Such changes would be the essential prerequisite for the emergence of language, and, plausibly, also account for the homologous capacities to some of those changes associated with language that we can find in related species (the increase in brain size, with all its implications, as discussed in [Section 25.3](#), would be the paradigmatic case). However, it is evident that language must also have been the result of some modifications that occurred specifically after the branching of the human clade, probably in genes involved in the configuration of specific neuronal circuits (the processing device in whose configuration and functioning *FOXP2* participates being a plausible example). But also, at the same time, however paradoxical this may sound, the neural substrate of (the nuclear components of) language appears to be quite old evolutionarily, as witnessed for example by the fact that the homology of the cortico-striatal-cortical circuit to whose arrangement *FOXP2* contributes, and which seems to play a crucial role in linguistic processing, may be traced, at least, as far back as the avian group.

But the case of *FOXP2* may also hint at a solution of this apparent evolutionary enigma, by pointing at what seems to have been a fairly general tendency, or at least a quite significant one, with respect to the modifications in the brain that would have occurred in our evolutionary line and that would have originated the emergence of language. If such modifications would have affected regulatory elements of gene expression, (i) the transformations to which they would have given rise would be much more significant than those resulting from the modification of structural genes, genes which in addition are quite costly in evolutionary terms, and (ii) they would have allowed the modification and selection of complete functional modules, made up of a variable number of genes expressing in a coordinated way, in the sense suggested by Oldham *et al.* ([2006](#)), and as is also adequately illustrated by *FOXP2* (Konopka *et al.* [2009](#) and [Section 25.3.2](#)). It is particularly remarkable in this respect that a high percentage of genes related to language (that is, those showing mutations associated or linked to certain cognitive disorders affecting linguistic competence) encode regulatory factors of gene expression or components of signal transduction pathways.

This possibility would moreover confirm the idea that brain evolution (and also the evolution of language) would have fundamentally been the result of changes in the brain transcriptome (not only due to the modification of regulatory factors in *trans*, as would be the case for *FOXP2*, but also in *cis*), which would have implied in turn both a modification of the expression levels of certain genes and also (and this is particularly significative) the creation of new expression patterns (in spatial and/or temporal terms) of preexisting genes, something that is frequently observed at the ontogenetic level, where prominent differences in phenotypic terms can be explained on the basis of changes in the spatial-temporal

regulation of the expression of a few transcription factors (Hofmann 2003). On the other hand, the cerebral proteome would have been substantially preserved during our evolution, perhaps because the majority of the proteins integrating it are employed in other histological environments, which would have prevented or hindered the appearance of too radical changes in their design (Mattick and Gagen 2001). As a consequence, we again encounter the form–function problem, which we already addressed in different contexts in Sections 25.2.2.3 and 25.3.4: also at the genetic (and molecular) level, what would be relevant for the evolution of language would not be the modification of the sequence of the genes involved in the development of the neuronal substrate of language – which in fact would not be so different from their homologous and orthologous precursor manifestations in other species – but rather the place and time in which these genes are expressed and, in short, the developmental context in which they act. Thus, the emergence of language in evolutionary terms would not be, as one would expect given its discontinuity at the phenotypic level, a consequence of an increase in structural complexity, but fundamentally an increase of the modification and co-option of preexisting developmental programs of the brain, through recycling of components (molecular, ontogenetic, cytological, histological) already present in our organism as a result of our evolutionary history.

The case of brain size as discussed above is particularly illustrative in this respect, as the modification of the functional properties of a few regulatory elements essentially involved in the modulation of basic cellular processes intervening in the division of neuronal precursors – in essence, in the organization of the DNA and in the assembly and correct disposition of the microtubules of the cytoskeleton – would have made possible such a conspicuous change, because it is the noteworthy increase of relative brain size observed in our evolutionary lineage, and this would in turn have elicited (or permitted) an eventual series of modifications in the design of certain neuronal circuits, fostering or facilitating the appearance of some of the necessary neuronal components for the emergence of language. On the other hand, the case of FOXP2 clarifies the issue of how certain basic circuits in the language processing devices (in particular, the cortico-thalamic-striatal circuits which make up the core of the *grammar of the basal ganglia* referred to in Section 25.3.4) could have appeared as a consequence of the recycling (and partial modification) of the regulatory cascades involved in the development and the functioning of certain brain structures, mainly of circuits involved in social cognition, in spatial-temporal representation, and especially in (the learning of) motor control (Marcus and Fisher 2003; Fisher and Scharff 2009; White 2010), ultimately giving rise to the emergence of an unlimited and recursive combinatory capacity of conceptual elements.

25.5 Concluding remarks: brain evolution, genes, and language

The emergence of the neural structures involved in language processing could fundamentally have been the result in evolutionary terms of the remodeling of preexisting programs implicated in the regulation of the development (and functioning) of other processing devices associated with other cognitive capacities. Language would have emerged as a consequence of the integration, in a single functional system, of cognitive systems responsible for thought and for perception and motor functions (Hauser *et al.* 2002). Thus, language is not necessarily the product of prolonged and gradual evolutionary development of a significantly high number of discrete components optimized by the effect of natural selection, as was apparently required, given the idiosyncratic nature and the discontinuity that characterizes it at the phenotypic level (Pinker and Bloom 1990). However this is a possibility that is substantially incompatible with the brief interval of time in the phylogenetic terms within which its evolution appears to have occurred ([Section 25.2](#)).

It is in this context that the importance of the possibility pointed out in [Section 25.4.2](#) is manifested: that the modification of a few regulatory genes (and/or their transcriptional profile) could have given rise to a remarkable remodeling of certain neuronal circuits, now dedicated (plausibly not exclusively) to the processing, and the operating, of computational tasks with symbolic units. The result would be transformed in externalizable and internalizable structures by preexisting sensorimotor systems. Given the very nature of these genes (to a large extent they encode transcriptional factors and components of signal transduction pathways), and also, in particular, given the evolutionary continuity observed in the organization of language at all levels of biological, and especially neurobiological, complexity (as pointed out in [Sections 25.3](#) and [25.4](#)), this process could have occurred in a relatively brief lapse of time in evolutionary terms, and could perhaps have coincided with the appearance of *Homo sapiens*.

Notes

Chapter 3

1. Much belatedly, in the spring of 2005, I had the fortune of benefiting myself from an extant residue of that grant, when I was invited by Alec Marantz, then Head of the Department of Linguistics and Philosophy of MIT, to be a visiting professor and to give (guess what) a graduate course on biolinguistics.

Chapter 4

1. This is not the only work of Descartes on method: prominently, there is also *Rules for the Direction of the Mind* and several other works, in some of which he actually “does science.” Less prominently, although probably more importantly, there are his many letters, the majority of which have not been translated. Since I am not proposing to present Descartes’s work as would a historiographer or historian of ideas, and because the *Discourse* is a work in which Descartes quite clearly makes what I take to be a crucial distinction (that between science and common sense), and also because the work is fairly well known, I focus on it.
2. While Descartes did not make clear distinctions between “atomic” concepts, sentential expressions without forces, and judgments, the Port-Royal Grammarians who followed did. So far as I can tell, though, it does not distort what Descartes was up to if one imports these distinctions from later work that in many respects honor Descartes’s views of the mind, of “innate ideas,” and of the use of language.
3. Although he also agreed with a great deal of what he wrote, Descartes disagreed with Galileo on some crucial features of scientific theory construction. For useful discussion of some of the differences, see Ariew (1986).

4. Descartes on Galileo: “It seems to me that he is greatly deficient in that he digresses continually and that he does not stop to explain fully any subject; this shows that he has not examined any in an orderly fashion, that he has sought for the reasons of some particular effects without having considered the first causes of nature, and thus, he has built without foundation” (Adam and Tannery 1964–74 Vol II: 380; translation Roger Ariew). It is clear from this that Descartes thought that the matter of theory unification (which comes up at the end of the quote) is connected with “order,” although likely it is best divided from that.
5. Alan Turing in 1950 rashly predicted that by the year 2000 one could program a computer to yield “speech” that is appropriate to the circumstances of ordinary language use (encapsulated in producing answers to random questions in ways that would appear to an audience of judges to be no less appropriate than those produced by a human being). This condition for a computer “test for mind” has not been met; at best, some programs manage in some cases to yield responses to questions that are very limited in their scope. While he was rash in his prediction, it should be mentioned that Turing was much more circumspect in his judgment concerning how to interpret the result, should this “test for mind” be met. In his view, all that meeting it would justify is a decision to change *usage* from “machines do not think” to “machines think.” Ironically, this change in “ordinary use of language” has taken place without the test being met. But this just indicates how little of interest there is in change of use. Change in usage is a result of human choice and beyond the reach of naturalistic science; it is in the domain of free action. No scientific issues turn on usage of the concepts we employ in our natural languages.
6. “Empiricist” and “rationalist” as used here represent views concerning how to study the mind. They are idealizations. Empiricists are generally speaking anti-nativists, for example, but the supposedly paradigmatic empiricist Hume acknowledged that the mind operated according to “secret springs and principles” that nature built into it, and in both his treatment of the missing shade of blue and his remarks on the fact that humans seem to be able to come up with moral judgments for randomly chosen actions and states – not to mention his account of causation – he seems to assume that there are native operations at work, even if he was skeptical about coming to know what they are. Perhaps one of the few clear examples of empiricist commitments is found in B.F. Skinner’s work on language. His views are still to a degree honored by some connectionists such as Elman, although Elman does acknowledge at least some native constraints on how (say) language learning takes place (Elman 1999, 2005). Nevertheless, he and others working within the connectionist learning paradigm can be said to try to minimize nativist commitments. For example, Elman appeals to biologically

based (and presumably domain-general) developmental timing constraints, suggesting however only that these make sense of his (1991, 1993, 1996) “start small” learning strategy. As for the label “rationalist”: again, there are few “pure” cases, Chomsky and perhaps Descartes and Cudworth aside. Fodor is a self-labeled rationalist, but is an externalist (though not, I grant, of language, but rather of concepts and thoughts, which he divorces from language – incorrectly, I believe). Note that philosophers sometimes called “rationalists,” such as Hegel, are by the standards of the distinction between empiricist and rationalist in question here, empiricists.

7. There are other forms of externalism, such as the view that language is an abstract object. But the social institution view – whether adopted explicitly or implicitly – dominates.
8. Newton’s *Principia* was published in 1687, but it was presented to the Royal Society in 1686. Arguably, moreover, in 1679 Hooke anticipated “Newton’s law” and wrote Newton about it. He did not get credit for the law, although Newton acknowledged his role.
9. I discuss Descartes’s account of depth computation and some possible implications of it near the end of my introduction to the 3rd edition of Chomsky’s *Cartesian Linguistics* (1966/2002/2009).
10. Cudworth dealing with the matter of innate ideas used the term “prolepsis,” which he borrowed from the Stoics, to indicate his view that our “innate cognoscitive power” appears to be able to anticipate what is needed to classify, understand, think ... on an occasion. An alternative terminology is found in speaking of an idea being “latent in the mind.” Placing the issue in question in the context of the rationalist/empiricist distinction, Chomsky remarks in his *Cartesian Linguistics* that “The strong assumption about innate mental structure made by rationalistic psychology and philosophy of mind eliminated the necessity for any sharp distinction between a theory of perception and a theory of learning. In both cases, essentially the same processes are at work; a store of latent principles is brought to the interpretation of the data of sense” (102).
11. See in this regard Stuart Kauffman (1993) on the origins of biological structure and order: selection plays a rather minor role.

Chapter 8

1. I intend “genome” to be interpreted broadly, that is to designate whatever the mode of heritable transmission and control of neural activity is. Recent investigations suggest that much of DNA beyond “genes” is responsible for biological control, e.g. exons, so called “junk” DNA, and probably much beyond that.
2. There is now a stream of literature that argues that in fact if the question is tested in certain different ways, even young infants show

knowledge of TOM. The issue is far from resolved, and threatens to disappear in a morass of psychological ways of describing the effect. Even fascinating results might have this fate, an object lesson of the need for Type 1 theories (Marr 1977, which we'll later discuss) if at all possible.

3. David Marr was a major, original thinker in the computational and biological basis of visual perception. His work in general offers an elegant understanding of the nature of problems in cognitive science, even beyond the study of perception.
4. Type 2 theories are theories of those domains that depend on a wide variety of interacting, complex phenomena, where there may not be a clean, elegant theory. In particular (p. 3), a type 2 theory is what we can wind up with “when a problem is solved by the simultaneous action of a considerable number of processes, whose *interaction is its own simplest description.*” Marr gave as an example the problem of protein folding.
5. The first data column is labeled V1/V2 because sometimes the child drops the first constituent, thereby resulting in a verb-first production. Topic-Drop of this sort is grammatical in Dutch, so we expect such sentences. That the verb-first sentences do in fact represent topic-drop finds good support in the data. See de Lint and Wexler (2011)
6. Age x; y means x years and y months.
7. See also Sano and Hyams (1994). Wexler (1992, 1994) proposed that it was rich agreement or null-subject possibilities that might be characteristic of the non-OI languages, but Sigurjónsdóttir (1999) showed that Icelandic, a rich agreement, non-null-subject language displayed a rich array of OI's in development. Since then NS/OI has been the accepted generalization.
8. We don't have room to explain why this should occur despite the lack of main verb infinitivals in young Italian children. See Wexler (1998a) for the explanation and Schütze (2004) for a subtle analysis of *be*-omission in the OI age-range, with evidence from much data.
9. See Wexler (2011) for a detailed discussion of why the ideas of Legate and Yang (2007) that attempt to predict the OI stage from the difficulty of learning a “tense parameter” predict the wrong results. They can't even predict the finiteness/word order correlation in e.g. the French OI stage.
10. The other two factors are:
 - “(2) Experience, which leads to variation, within a fairly narrow range, as in the case of other subsystems of the human capacity and the organism generally” and
 - “(3) Principles not specific to the faculty of language.”
11. It would be worth researching (it's from a published standardized test) exactly what is in that test in detail, as a hint at what might be heritable in grammar. Are particular sub-parts of that test more heritable than others?

12. For the LI Risk group, the correlation, while still significant, was only .209.
13. The estimates of bivariate heritability for Sentence Structure and finiteness were just short of significance, so there may in fact be some common genetic basis for the development of finiteness and whatever is in the Sentence Structure test. In my opinion, it is unlikely that this is just one “syntax” development; there are too many phenomena like timing rates that are different, as we have discussed. And it may be that only a particular sub-part of Sentence Structure is related to finiteness in this genetic way. Much remains to be done.
14. p indicates the smaller arm of a chromosome and q indicates the larger arm.

Chapter 9

1. Note that this is ungrammatical in English, which does show a *that*-trace effect.
2. Metrical feet are indicated by parentheses (...), while stress is indicated by CAPITAL LETTERS henceforth.
3. This is true even if it takes some time for children to choose the correct parameter value (perhaps because they will not change their current hypothesis unless they are sure that it is really unable to analyze the input). No matter when they decide to choose the correct parameter value, once they choose it, they should continue to use it exclusively.
4. Note, however, that the “light switch” account may be able to produce more gradual-seeming behavior if it used for learning entire grammars rather than individual parameter values. See [Section 9.3.3](#) for more discussion of learning grammars vs. learning individual parameters.
5. Fodor (1998) and Lightfoot (1999) suggest that children might be able to discover the relevant unambiguous cues by a process they already use for comprehension: parsing the incoming data. To “parse” a data point is to know the underlying structure for a data point, i.e., to know what parameter values generated it. If a particular parameter value is required for a successful parse of a given data point, that data point is unambiguous for that parameter value.
6. This is true provided that the value for the extrametricality parameter is learned after the value of the quantity sensitivity parameter. See discussion below regarding the learning path.
7. The underlined portion of the example unambiguous data represents the most relevant portion of the data point.
8. We note that the equation for p_{vnew} includes numbers in the numerator (1.5) and denominator (3) that represent the learner bringing minimal prior knowledge about which parameter value is correct, believing in both equally at the start. This learner is strongly driven by the data

points encountered. See Pearl (2011) for a more detailed discussion of the intuitions behind this equation.

Chapter 10

1. The current literature offers no clear account of how the need to engage conflict resolution is detected and triggered. In other words, up to now, conflict monitoring remains a sort of homunculus in charge of extracting an abstract index of conflict (cf. Botvinik *et al.* 2001).
2. The fact that the reduced conflict effect in bilinguals was restricted to the first part of the 75% congruent task-version of Costa *et al.*'s (2009) study replicates Costa *et al.*'s (2008) results. This observation has been interpreted as evidence that monolinguals can equal bilingual's performance after having acquired some practice.
3. Some authors have raised concerns about the reliability of such huge differences between bilinguals and monolinguals (e.g., Colzato *et al.*, 2008). Morton and Harper (2007) also raised concerns about the reliability of the bilingual advantage on executive control based on potential socio-economical status effects.

Chapter 11

1. Dabrowska (2010) interpreted this to be the result of different frequencies of exposure to specific kinds of unacceptable sentences. Since the training of generative linguists includes reading scores of textbooks' and articles' examples of specific ungrammatical sentences, this could lead to a higher familiarity with them, which might lower generative linguists' sensitivity to the unacceptability of these sentences. It is important to note, however, that this kind of explanation systematically predicts the *opposite* of the cognitive bias hypothesis for the phenomena studied by syntacticians: judgments of generative linguists are going to be systematically *less* sensitive to the predicted contrasts than the judgments of naïve participants.

Chapter 12

1. This equation need not make a direct theoretical claim – it simply fits the data well; see Ratcliff (2006) for an alternative parameterized equation driven by a particular theory of processing speed and accuracy.
2. This observation has interesting consequences for subjects modified with an object-relative clause (“The editor that the book amused laughed.”). In those sentences, the gap site in the relative clause was adjacent to the sentence-final verb. Therefore it would have been possible to complete this task based on string-local information

(perhaps reasoning: typically, individuals that can be amused can also laugh). It gives support to the idea that restoring the scope of focal attention to the higher subject phrase is obligatory. Notice also that even though the same head might participate in the subject-verb dependency and the relative clause verb-object dependency, different token encodings of that head are relevant for completing the task.

3. There were some other important differences between the two studies. Wagers and McElree (2009) used a variant of SAT design called multiple-response SAT (MR-SAT), in which participants are trained to give many judgments per stimulus to a series of tones following a probe (Wickelgren, Corbett, and Dosher 1980). Despite this task difference, the absolute rate differences between focal and non-focal conditions, about 74 ms, was similar to McElree, Foraker, and Dyer (2003).

This study also included a direct comparison of subject vs. object-extracted relative clause, as in “The editor who enjoyed the book laughed,” vs. “The editor who the book amused laughed.” Though the subject-attached relative clause condition led to slightly higher accuracy, there were no rate differences between the two.

4. This simplifies the actual feature system used in distinguishing singular and plural. See Harley and Ritter (2002), Farkas and de Swart (2010), among others, but it suffices for the discussion of English.
5. It is possible to extend the “command domain ID” proposal to allow distance to play a role in retrieval. If the command ID were conceived of as a high-dimensional vector that is slowly, randomly changing, then more recently encoded c-commanders will have an advantage over less recently encoded ones. Proposals similar in spirit have been made for encoding linear order (Page and Norris 2000). It is only a heuristic system, however, since it relies on an alignment between encoding order and hierarchical order. This alignment could be violated in many instances, particularly in cases where a more deeply embedded head is predicted but not yet attested in the input. See Wagers (2008). Thanks to Brian Dillon for discussion on these issues.
6. There is a question about whether the unergative clause structure that “sail” licenses (but “fix” does not) affects the retrieval structure. Independently of its occurrence in a filler-gap dependency, the string “the guy who lived by the sea fixed” has outstanding thematic requirements: the verb “fix” must discharge its THEME role; on the other hand, in the string “the guy who lived by the sea sailed” is well formed without further constituents. Inclusion of PPs like “in two days” forces the transitive interpretation of “sail” ultimately, though this information is not available in the critical region. The thematic structure of the verb is important because there is evidence for argument anticipation (Altmann and Kamide 1999) which occurs independently of filler-gap dependency completion. See Wagers (2008).

7. One important difference between reflexive anaphora resolution and agreement or *wh*-dependency formation is the fact that reflexive anaphora may not be a direct relationship between reflexive and subject; for example, it may be mediated via the verb (see Reuland 2011, for the panoply of options).

Chapter 14

1. The “grain size” problem was aptly described by Embick and Poeppel (2006) with regard to one of the most discussed potential correlations between brain and language, namely the issue of whether Broca’s region (i.e. the pars opercularis and triangularis of the left inferior frontal gyrus, see Figure 14.1) mediates syntactic computation. According to Embick and Poeppel, this question is too coarse at both a cognitive and a neuroanatomical level, since “syntax” is not a single, monolithic task, nor is ‘Broca’s area’ a single, monolithic area of the brain.”
2. Indeed, research in the domain of speech perception has revealed a general continuum of “intelligibility” in temporal cortex, with increasingly intelligible speech leading to activation in increasingly anterior temporal regions (Scott *et al.* 2000).
3. Fitch and Hauser (2004) examined how these two types of grammars are learned by humans and non-human primates (cotton-top tamarins). They reported that, while humans were able to learn both types of grammars, tamarins could only distinguish grammatical from ungrammatical strings in the non-embedding case. This finding appears to corroborate Hauser *et al.*’s (2002) assumption about recursion as a uniquely human aspect of language (but see below for some concerns regarding the experimental design employed in this study).
4. Whenever possible, we refer to macroscopic brain regions (e.g. POp, PTr) when describing functional neuroanatomy in this chapter. In some cases, however, we make reference to Brodmann areas (BAs) additionally or instead (e.g. BA 44, BA 45). BAs were defined on the basis of cytoarchitectural differences (i.e. differences in the cell structure within the layers of the cortex) by German neurologist Korbinian Brodmann (Brodmann 1909). Though BAs are often used interchangeably with brain region labels (e.g. BA 44 with POp and BA 45 with PTr), they are not identical. In particular, cytoarchitecture differs considerably across individuals, but, since it can only be examined in post mortem analyses, it cannot be determined for the participants in neuro-imaging studies on language. In order to address this problem, Amunts and colleagues have developed probability maps for a number of BAs in language-related regions (e.g. Amunts *et al.* 1999, for the lIFG) such that the likelihood of a particular activation falling into a specific BA can be

determined. When the results of a particular study are described primarily with reference to BAs rather than to anatomically defined regions, we also refer to BAs in our discussion of those results here.

5. The original design used by Friederici *et al.* (2006a), which was adopted from Fitch and Hauser (2004), was subject to the criticism that participants may have been able to process the grammar containing embeddings simply by counting the number of occurrences of each type of symbol (Perruchet and Rey 2005). This problem was overcome in a follow-up fMRI study (Bahlmann *et al.* 2008) which ensured that the corresponding A and B categories (i.e. those at the same structural level) matched according to a particular feature. This experiment also showed increased activation of BA 44 for artificial grammars containing centre-embeddings vs. adjacent dependencies.
6. Notably, this subdivision also contradicts Chomsky's notion of an "evolutionary leap" based on the operation Merge. While Hauser *et al.* (2002) focused on the recursive aspect of syntactic structure building when outlining their evolutionary perspective, subsequent publications by Chomsky suggest that they were, in fact, alluding to the fact that Merge serves to create hierarchical and potentially unbounded structures:

The simplest account of the "Great Leap Forward" in the evolution of humans would be that the brain was rewired, perhaps by some slight mutation, to provide the operation Merge, at once laying a core part of the basis for what is found at that dramatic "moment" of human evolution, at least in principle [...]. (Chomsky 2005a: 11–12)

7. Of course, current syntactic analyses would posit that movement (IM) occurs in both cases, i.e. both in the +movement and in the –movement conditions in (2). However, the crucial comparison is relative and based on the assumption that there is an additional movement operation in the case of the wh-question.
8. Note that the issue of movement is not necessarily central here, since the dependency must be reconstructed in some way – be it via a gap, trace, or copy in the phrase structure or by some other means in theories that do not assume movement operations (for discussion, see Van Valin and LaPolla 1997).
9. Note that, here and in the following, we abstract away from the details of how relative clauses are to be analyzed syntactically. Whether we view the dependency as holding between the relative pronoun and the argument's base position within the relative clause, between a relative clause operator and the base position, or even between the head noun and the base position is of no consequence for the present discussion.
10. In an even more recent paper, Santi and Grodzinsky (2010) adopt yet another distinction pertaining to movement, namely "canonical"

(subject) movement vs. “non-canonical” (object) movement. By means of a design crossing “movement type” (subject vs. object in subject vs. object relative clauses) and type of embedding (centre embedded vs. right-branching), they argue for a movement-specialization of anterior IIFG. In our view, this illustrates the need to resort to ad hoc theoretical assumptions in order to attempt to maintain a movement-related interpretation of the role of (parts of) the IIFG in language processing. As noted by Gordzinsky and colleagues in previous publications (Ben-Shachar *et al.* 2004), both subject and object relative clauses involve (the same type of) movement. In fact, Ben-Shachar *et al.* (2004: 1334) used this line of argumentation to derive the fact that they did not find differential IIFG activation for object vs. subject wh-questions in Hebrew.

11. In some languages, for example, only direct objects that are high in prominence (e.g. animate: Spanish; specific: Turkish; animate and specific: Hindi) are marked via case or a preposition (“differential object marking,” DOM, Bossong 1985; Aissen 2003). This phenomenon is often explained via the prototypical association between subjects (which often express the actor of an event) and high prominence (e.g. animacy) and objects (which often express the undergoer of an event) and low prominence (e.g. inanimacy) (e.g. Silverstein 1976; Comrie 1989; Aissen 2003; de Swart 2007). (For an in-depth discussion of prominence information in the context of language processing, see Bornkessel-Schlesewsky and Schlesewsky 2009b.)
12. It remains to be seen whether this assumption is, in fact, cross-linguistically valid. While languages such as German or English may be assumed to lexically encode word category and this lexical information thus directly and unambiguously determines whether an element is potentially a projecting head or not, other languages display a considerable degree of fluidity in this regard (“transcategoriality”). A case in point is Mandarin Chinese, in which a large number of lexemes can potentially function as different word categories (e.g. noun or verb) depending on the sentence context in which they occur (Bisang 2008). Possibly, the use of sequence information for the establishment of a hierarchical syntactic structure therefore involves processes of contextual control rather than stimulus-driven control in languages of this type and should thereby be expected to correlate with activation in the POp rather than in the DFO/PMC. An initial indication that this might indeed be the case stems from an early imaging study on Chinese (Luke *et al.* 2002), which reported activation in BA 44 (amongst other inferior frontal regions) for a category sequencing violation (verb-adverb order). However, since this activation was revealed by a contrast between the sequencing violation and a font change detection condition (i.e. a relatively low

- level control), this result cannot be considered conclusive with regard to the question under consideration.
13. For more detailed arguments in favor of this position based on the direction of information flow within the lIFG and the timing of subprocesses within sentence comprehension, see Bornkessel-Schlesewsky *et al.* (2012).
 14. Note, however, that a recent comparison between sentences and word lists using MEG did, in fact, reveal increased lIFG (specifically POp) activation for sentences – in addition to increased activation in further regions such as the ATL and frontomedian cortex (Brennan and Pylkkänen 2012). On the basis of these findings, the authors suggest that the relatively restricted set of activations (typically in the ATL) observed in previous neuroimaging studies comparing sentences and word lists (see Section 14.2.1) may reflect the fact that fMRI may not be optimally suited to detecting syntactic structuring operations due to the timescale at which they occur (within the millisecond range). Interestingly, the fMRI study by Snijders *et al.* (2009) that was discussed in Section 14.2.3 revealed higher lIFG activation for sentences as opposed to word lists, which, at a first glance, appears at odds with this explanation and with the results on the ATL that were described in Section 14.2.1. However, as already noted in Section 14.2.3, the lIFG activation observed by Snijders and colleagues in fact resulted from an interaction between stimulus type (sentences vs. word lists) and ambiguity (presence or absence of a category ambiguity), with sentences involving an ambiguity leading to higher lIFG activation (POp bordering on premotor cortex). However, since the resolution of the category ambiguities at a later point in the sentence will very likely have required reanalysis processes at least in a subset of trials, this activation could again be due to the increased need for cognitive control rather than, as assumed by Snijders *et al.*, to increased unification demands and thereby sentence processing proper.

Chapter 15

1. There are a number of different variants of this paradigm. In masked priming, primes are presented only for a very brief time period and followed by a visual mask. They are thus assumed to not be consciously perceived by participants and, hence, to provide a window on automatic aspects of morphological analysis. In delayed priming, by contrast, participants are presented with a word list within which the prime is embedded.
2. They argue that, since all words in Hebrew are morphologically complex and require morphological decomposition, the lIFG will always be involved in the processing of any real word. Thus, no differential

activation was observed in this region for a morphological relatedness task vs. other types of tasks (e.g. semantic relatedness, rhyme judgments) in Bick *et al.* (2008).

3. At this point, we would ask the reader to imagine how many real-world situations in fact require a categorizing decision (such as a lexical decision) with regard to a single-word expression. While there are examples (“Stop!,” “Help!”), more complex morphological forms (either inflected or derived) such as “swum” or the German equivalent “geschwommen” never occur in isolation, thereby raising the question of what these studies actually tell us about language. They are indeed highly informative with regard to the ability for fulfilling a cognitive task, but the explanatory capacity for language and language architecture is still open to debate
4. Note that there is some evidence to suggest that the convergence of codes may be more structured than has been assumed in the discussion here, i.e. that it requires a certain type of sublexical structure (see Taft and Kougiou 2004).
5. Berko devised what was to become known as the “wug test,” i.e. she asked children to produce inflected forms of nonce words (“This is a wug.” [referring to a picture of an unknown object or animal] “Now there is another one. There are two of them.” [referring to a picture of several of the above-mentioned creatures or objects] “There are two ____.” [child is expected to complete the sentence]).
6. This assumption was essentially motivated by classic findings on ERPs and language processing, which suggested that N400 effects correlate with semantic processing, while LAN and P600 effects correlate with syntactic processing (e.g. Kutas and Hillyard 1980; Osterhout and Holcomb 1992; Hagoort, Brown, and Groothusen 1993). However, it has become clear over approximately the last decade that this straightforward dissociation is too narrow, since N400 effects have also been observed for manipulations that would typically be considered (morpho-)syntactic such as word order variations (Bornkessel *et al.* 2004; Haupt *et al.* 2008) and case-marking violations (Frisch and Schlesewsky 2001; Frisch and Schlesewsky 2005; Mueller *et al.* 2005; Mueller, Hirotani, and Friederici 2007; Choudhary *et al.* 2009).
7. One potential problem with the study by Joanisse and Seidenberg (2005) is that the pseudoregular forms include a potential suffix (-d/-t) while the true irregulars do not. Thus, one might argue that automatic processes of morphological decomposition can explain the activation pattern in this region.
8. Note that both of these studies in fact observed increased IIFG activation for irregular vs. regular forms, while the reverse contrast yielded increased activation in temporal regions. These results directly contradict the assumptions of the DP model.

9. It also appears important to note that the sentences used in this study were presented in a word-by-word manner with a stimulus onset asynchrony (SOA) of 1600 ms. Given this highly atypical presentation rate, it appears questionable whether participants indeed drew upon normal reading strategies.
10. Somewhat disconcertingly, however, Inui *et al.* (1997) reported that this activation difference was no longer observable when a different type of judgment task was used. This once again speaks against an interpretation of the effect in terms of automatic morphological processing.
11. Note that the N400 was followed by a late positivity in one case, but this is not central to the present discussion. See Choudhary *et al.* (2009) for details.
12. Both types of violations also typically engender late positive ERP effects (P600s). (In fact, some studies on agreement violations have only observed P600 effects without an additional LAN, e.g. Hagoort *et al.* 1993; Osterhout and Nicol 1999; Nevins *et al.* 2007.) We will not discuss these positivities here, however, since they appear to be a general index of the violation rather than being sensitive to more fine-grained linguistic properties (for functional interpretations of these effects in terms of general mechanisms such as conflict monitoring or stimulus categorization, see, for example, van de Meerendonk *et al.* 2009; Bornkessel-Schlesewsky *et al.* 2011).

Chapter 16

1. Or in the scope of some other operator that supports decreasing inferences (Ladusaw 1979, 1980, 1983).
2. Although such a correlation can and must obviously be built in, given the descriptive generalization that syntactic and semantic operations do, by and large, stand in a one-to-one correspondence.
3. For an extensive discussion on the possible empirical arguments for this, see Pylkkänen (2008).

Chapter 17

1. In what follows, we will assume that neural and cognitive levels are closely correlated, although specifying the exact nature of that relation is outside the scope of this chapter.
2. Although the notion of modularity in cognitive science is most often couched in terms of “classical” symbol-manipulation accounts of cognitive science, it is also compatible with connectionist accounts that seek to explain cognition in terms of the interactions between large numbers of simple processing units (even in approaches to

computation that eschew explicit programming, modular structure still often plays an important role; Jacobs, Jordan, and Barto 1991; Miikkulainen 1993).

3. Modules could also be taken to be devices that are innate, mandatory, fast, etc., and indeed there has been extensive discussion in the literature about what might count as a module (Carruthers and Chamberlain 2000; Coltheart 1999; Fodor 1983). Because we take the current arguments here to be independent of the modularity's precise formulation, we won't rehearse those discussions here.
4. A similar view is apparently expressed by Thomas and Karmiloff-Smith (2002), in their suggestion that "genes do not code directly for high-level cognitive modules ... processing structure is emergent and experience-dependent, the *outcome* of developmental processes" (p. 731).
5. All of these expectations should be tempered by time. Other things being equal, we should expect that systems that have diverged more recently should share more in common than those that have descended less recently. We should thus, for example, expect greater overlap between the substrates of language (which evolved relatively recently, presumably since the last common ancestor with chimpanzees some six or so million years ago, and perhaps, as some have argued, only within the last 1,000 years) and general cognition than between, say the heart and the lung, which are separated by at least a few hundred million years (Maina 2002).
6. The Hoen *et al.* study should be seen as preliminary both because it lacks a placebo control and because the results seem to indicate facilitation only for some syntactic structures but not others in ways that would not be immediately obvious from the nature of the non-linguistic training. Furthermore, additional work needs to be done to show that subjects are truly treating the linguistic materials as linguistic.
7. Ramus (2004) makes a related point, arguing that two clusters of symptoms could arise from a common source. Such complexities could arise in many ways, for example a single gene could take on multiple function (pleiotropy), through duplication and divergence a copy of a gene might take on new functions, or a single gene might serve a single function (say transporting a signal from a cell surface to a nucleus) that is recruited in multiple domains. See Gerhart and Kirschner (1997) for a number of excellent examples.
8. Or there could be shared subcomponents used in different ways in different systems.
9. Overlap in, say, brain activity at a coarse grain doesn't guarantee overlap at a fine grain; A single voxel measured in 3 tesla magnet may capture blood flow metabolism associated with hundreds of thousands neurons, and it entirely possible that different subpopulations within are devoted to different tasks or computations.

10. For an argument that recursion has evolutionary precedent in non-human primates, see Fragaaszy *et al.* (2002).

Chapter 23

1. The simplification here is rather drastic. It appears, for example, that primate alarm call systems may well involve strong innate coding in the producer of the signal, but rather more learning and inference on the part of the receiver, as evidenced by species' ability to respond to other species' alarm calls (e.g. Zuberbühler 2000).
2. See also Di Paolo (1999) for an alternative way in which spatial structure can influence the evolution of communication.
3. In fact some authors have raised questions about the generality of these results (e.g. Mayley 1996) even in the non-linguistic case.

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