How Scientific is Biolinguistic Science?

Christina Behme

Christina.Behme@dal.ca

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This paper is an extended version of my review of Massimo Piattelli-Palmarini, Juan Uriagereka, & Pello Salaburu (Eds) *Of minds and language: a dialogue with Noam Chomsky in the Basque Country*, which will appear in the next issue of *Journal of Linguistics*. It contains information that had to be eliminated from the review to meet the length limit of the journal but is very relevant to evaluating the quality of the book.

The Minimalist Program (Chomsky, 1995, henceforth MP), centerpiece of the biolinguistic enterprise, has created controversy virtually from the moment it was published. Hailed as the crowning achievement of decades of research on generative grammar (e.g., McGilvray, 2006; Smith, 1999; Uriagereka, 1998) it also has been severely criticized. Some of the criticism went beyond challenging details of MP and questioned its very foundations and scientific justification (e.g., Johnson & Lappin, 1997; Lappin, Levine, & Johnson, 2000; Postal, 2004, 2009; Pullum, 1996; Seuren, 2004). "The Minimalist Program … fails to satisfy basic scientific criteria such as respect for data, unambiguous formulation, falsifiability" (Seuren, 2004, p. 4). Postal (2009) argues that the ontological foundation of MP is incoherent and Culicover claims that the perspective taken in MP "explicitly rules out precisely the major theoretical achievements of the past. All of them" (Culicover, 1999, p.138). Chomskyans have issued spirited rejections of any

criticisms (e.g. Smith, 1999; Chomsky, 2002, 2005; Reuland, 2001; Roberts, 2001; Uriagereka, 2001), but convincing refutations are still elusive. One reply to the challenge that with MP Chomsky has abandoned scientific practice seems to confirm such abandonment: "Can [Chomsky's] goal be pursued by 'normal scientific procedure'? …Why should we expect Chomsky to follow normal scientific practice?" (Fiengo, 2006, p. 471). For readers who want to answer Fiengo's question: "We should expect it because the Chomskyan biolinguistic enterprise claims to be a scientific enterprise", the current volume seems like an ideal place to find support. Advertised as "state-of-the-art" account of the "great interdisciplinary enterprise" of biolinguistics, with contributions by leading researchers in linguistics, psycholinguistics, language acquisition, cognitive neuroscience, comparative cognitive psychology, and evolutionary biology, it could be expected to provide a coherent defense of MP.

The table of contents gives hope that this volume would live up to the dust-jacket praise, cover a wide variety of topics, and refute criticism of the biolinguistic enterprise. The 24 chapters are thematically grouped into four sections. Part 1, *Overtures* promises an exploration of biological perspectives on language and other cognitive functions, Part II, *On language* situates linguistics within the natural sciences, Part III, *On Acquisition*, focuses on how innate resources accommodate the seemingly effortless language learning by all normal children, and, Part IV, *Open Talks on Open Inquiries* expands into areas loosely connected to linguistic theory (e.g., ethics, aesthetics, neural correlates of emotions). Each chapter is accompanied by a discussion section that provides "spirited exchanges" (p. 1) between the author and an audience. Of course, one should not judge a book by its table of contents. The stark contrast between Penthouse advertisement and Hinterhof reality becomes quickly obvious in Chomsky's pedestrian *Opening*

Remarks and none of the other collaborators provided a chapter of much higher quality. As a result, any reader hoping for serious engagement with long standing criticism or novel insights will likely be disappointed. I will defend my unfavourable opinion with some general remarks followed by a more detailed discussion of three chapters.

First, the quality of writing is surprisingly poor. This concerns both style and content. Presumably editing was kept to a minimum to maintain the flavor of an oral exchange. Yet, this is hardly an excuse for exposing the reader to repetitive passages of irrelevant musings. For example, Laka (p. 338) wants to formulate one wish about what she would like to know about language. Yet, 255 words later no identifiable wish can be detected and the exhausted reader wonders why this pointless passage was not edited. In several contributions the exposition is so obscure that the point of the discussion is lost. Higginbotham obfuscates a simple contrast between direct and indirect quotation (p. 143), and provides an explanation of compositionality that is nearly incomprehensible (pp. 142-3). The point of his chapter is never clearly explained. Seemingly trying to provide a summary of the nature and prospects of combinatorial semantics Higginbotham says: "Goodness knows how it will turn out" (p. 150). Finally, it should not be left to the reader to discover Chomsky's obvious misinterpretation of another participant's contribution: "Randy Gallistel was telling us about jays that can count to many thousands" (p. 391). Gallistel told no such thing but said "In times of plenty….jays, gather food and *store it* in more than ten thousand different caches" (p. 61, my emphasis). It seems that many contributors expected extensive editing but the editors expected perfect manuscripts. The reader is left with the unfortunate consequences of these mismatched expectations.

A more serious flaw of the volume is the almost complete absence of any engagement with current opposition to the work presented. The reference section is almost completely devoid of entries for authors who work outside the Chomskyan framework, containing no publications of Everett, Lakoff, MacWhinney, Partee, Pollard, Pullum, Sag, Sampson, Smolensky, Tomasello, and no recent publications of Chater, Christiansen, Elman, and Postal. There are, however, frequent jabs at unnamed Quinians and behaviourists and, sadly, several instances of severe distortion of work in competing frameworks (to be discussed below). This biased treatment creates the highly misleading impression that, by now, the biolinguistic enterprise is universally accepted. The 'spirited exchanges' between the participants were not conducted in a spirit of critical engagement. Instead, questions often remained unanswered. For example, Piattelli-Palmarini (p. 32) asks whether the parallel between the numbering system and language holds given that the latter lacks an equivalent of the unique empty set. Chomsky's 230-word-reply never answers this simple question, and an alleged 'clarification' by Higginbotham leads the discussion further away from a potential problem for MP. Frequently participants engage in musings of no relevance to the chapter topic; Laka's chapter on UG is followed by a 'very brief' remark about history that derails the debate completely to events in the 17th and 18th and eventually the 14th centuries. Others ask questions that cannot possibly be answered by the presenter; Friederici repeatedly has to remind questioners that she has not conducted the research they ask about. When a questioner is uncharacteristically persistent (as Higginbotham is when insisting that Hauser confuses the categorical imperative and utilitarian principles), he does not receive an answer but gets briskly brushed off: "Right, More questions?" (p. 321). And rare opposing views are flatly rejected.

In addition, the reader is overexposed to uninformative platitudes and analogies ("language would be something like a snowflake", "syntax acquisition becomes a matter of tripping switches", "language and basic mathematics are virtually isomorphic", "we are not angels", "Father, Mother and Child Case", "core and 'hell on wheels' domains", etc.), to inaccurate depictions of historic events, to models of generative grammar that have features allegedly eliminated by MP, and to a plethora of technical terms that are neither defined nor used consistently. The last problem is especially troublesome in a work that aims at a multidisciplinary audience. Conventions need to be explained and used consistently. Yet, in the linguistic contributions we find a bewildering variety of types of syntactic tree diagrams. This seems entirely unmotivated given that all participants subscribe to essentially the same theoretical framework. Rizzi acknowledges that it is important to offer illustrative examples "for our non-linguistic friends" (p. 213). Yet, he sees no need to provide any explanation for the labels on his tree diagrams (p. 214). Non-linguists will not understand these diagrams, and readers run the risk of being confused by the different labeling conventions used by various authors. Readers who seek clarification in important MP resources (e.g., Chomsky, 1995; Uriagereka, 1998) are sure to encounter additional label-name varieties. Similarly, Boeckx acknowledges, "that the [MP] jargon is so developed that it is hard to … address questions of an interdisciplinary nature, much less design adequate experiments" (p. 46). Nevertheless, within a single paragraph he adds two new terms for the same operation: "Concatenate" and "Basic Grouping", and later he introduces needlessly an uncommon application of the German terms *Neubildung* and *Umbildung* for processes that can be better described by using English vocabulary.

Further, some contributors provide disclaimers that make it dubious whether they have any confirmed results to report: "All of these studies are very recent and more data is needed, but they nevertheless point in a direction that is very suggestive, albeit premature" (Galles, 2009, p. 345). It is similarly dubious whether problems have been solved: "…we solved Plato's Problem, at least conceptually (though not in detail)" (Boeckx, p. 49). Other work is sketched so broadly that it becomes impossible to evaluate it. Where details are given, inaccuracies and inconsistencies emerge. Friederici's reports very time-sensitive experiments. Yet, in the double violation condition (p. 363) the first violation of the test sentence occurs at the beginning not at the end[1]. Any German speaker will notice the very salient mismatch in gender between 'der' and 'Buch' long before she encounters the syntactic and semantic violation introduced by the last word of "Der Buch wurde trotz verpflanzt…" (Ibid). It is dubious that the recorded event related brain potential wave could be explained based on the final violations alone. Gallistel reports experiments in which jays hid food and retrieved different caches, depending on how much time had passed. From these experiments he concludes "that the remembered past of the birds is temporally organized *just as is our own*" (p. 63, my emphasis). Here Gallistel ignores important differences between the species and jumps to an entirely unwarranted conclusion. Unfortunately, the tendency to draw far-reaching conclusions from meager empirical evidence is found in several of Gallistel's examples and shared by other contributors. Cherniak discusses research showing that connections in the nervous system of the nematode *Caenorhabditis elegans*, as well as in certain parts of the brains of rats, cats, and macaques, are placed so as to minimize wirelength, and takes this to indicate neural optimization in human brains, concluding that "neuroanatomy is intimately meshed with the computational order of the universe" (p. 116).

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[1] It is possible that "der Buch" was a typographical error. But this would provide more evidence for astoundingly sloppy editing. The mismatch between 'der' and 'Buch' is virtually impossible to miss for native German speakers and would have been detected by even very superfiscial editing.

Hauser uses his finding that in experimental settings cotton-top tamarins and common marmosets prefer lullabies to German techno tunes and slow to fast tempos to support his hypothesis that we share innate musical preferences with these species (pp. 317-8). However, the monkeys also prefer silence over any kind of music, and during the discussion Hauser reveals "we are at such an early stage of this work that it is hard to make much sense of it" (p. 326) It would not be surprising if, after reading example after example of poorly supported theorizing, some readers would agree with Hauser's reasoning here: "…even though [the monkeys] have a preference for certain kinds of music, there seems to be a strong preference for silence over noise. Perhaps they are just ahead of their time, prescient animals…" (p. 318).

Furthermore, several of the reported studies have been designed not as open inquiries in a clearly defined problem but rather as support for the resaercher's theoretical commitments. The examples above show this tendency. But there are stronger cases. Uriagereka claims to provide an explanation for uniterpretable syntactic features. The information provided by these "cannot be explicated in terms of interface conditions based on interpretability" (p. 170). He claims that Case is an example of this uninterpretability, but it turns out that he makes this claim because he knows "of no sucessful account of Case as interpretable that is compatible with the minimalist perspective" (p. 171, fn.2). An objective researcher might be motivated to look for such an account *outside* of MP. But Urigereka procedes instead to providing a nearly incomprehensible minimalist account. He introduces the family mother, father, and child Case because linguistic derivations "are really like a family affair, relations being reset at each new generation (p. 172). Tragically at one point all family members turn into virues and get killed, leaving "the phrase marker warped in a characteristic shape" (p. 178). In the process of this "entire catastrophe" (p.

178) new phrasal dependecies emerge. Allegedly, the eliminated Case features are "very stupid features with absolutely no interpretation" (p. 178) that we share with non human primates and the reason they do not have a human-like language is because they "either lack the resources to come up with the elimination of this Case virus, or perhaps the very virus" (p. 179). Readers not familiar with linguistics might believe that this bizarre account needed to be invented because nothing else could explain the phenomenon. This is not quite so. According to Postal (presonal communication) linguists outside MP have provided an account for the feature of Ergative languages Uriagereka discusses. Briefly, an object is being deobjectivized in a way usually called antipassive. Then the subject takes on a status of a special kind of subject usually called unaccusative. The most obvious visible effect of antipassives is that transitive subjects, usually having a case called Ergative, and taking objects with accusative sometimes called absolutive, end up taking absolutive case while their objects are some other one, dative, genitive, instrumental (for details see Dixon, 1994; Manning, 1996).

The following three chapters, one might think, might be where the most severe criticisms of MP would be addressed. First, in *Hierarchy, Merge, and Truth* Wolfram Hinzen seems to take up Postal's ontological challenge. In a nutshell Postal (2009) argues that a finite physical organ (like the human brain) cannot possibly generate an infinite output. Hence, any biolinguistic theory requiring human brains to generate infinitely many linguistic objects is internally inconsistent. Postal's own ontology (rational realism) is not widely accepted because it requires the existence of abstract objects that cannot be confirmed by the natural sciences. Nevertheless, the basis for the criticism remains. Hinzen does not address this criticism directly but he claims to derive an ontology of language within the biolinguistic framework. If successful, obviously, this project

would undermine Postal's criticism. However, hope fades quickly. Hinzen claims "If you know just one natural number – you really know what that particular number means – then you know infinitely many: you master a generative principle. The same is true for your understanding of a single sentence: if you know one you know infinitely many' (p. 123). Depending on how we interpret 'to know' these claims are either vacuous or false. Seemingly Hinzen derives the foundation for linguistic knowledge from a fanciful reincarnation of set theory. Many of the details remain obscure and proposals are not defended. For example, semantics seems to be reduced entirely to syntax "Different kinds of syntactic objects thus intrinsically correlate with different kinds of semantic objects, such that in the absence of the syntactic construction of the latter at the semantic interface, they would not exist. Their reality is at the interface and nowhere else" (p. 130). Anyone who rejects Postal's ontology based on the empirical unconfirmability of abstract objects should be worried about the confirmability of objects that have an ephemeral existence at currently unconfirmed interfaces. Further, Hinzen's account seems to cover only a subset of linguistic objects: "Beyond the possible forms that the computational system of language provides, there are *no* thoughts that you can think. You can *of course* think associative, poetic, non-propositional kinds of thoughts, too. But these are *not* the ones we are (here) trying to naturalize" (Ibid., my emphasis). Presumably we couch many of our associative, poetic, and non-propositional thoughts in language. Hence, even if Hinzen's ontology were defensible (I doubt it is), it would leave much of language unaccounted for.

The second severe criticism was that MP has abandoned scientific practice and rests on mainly unconfirmable claims. One would expect that any detailed explanation of UG would respond to this charge. But in Itziar Laka's chapter *What is there in Universal grammar?* (as elsewhere) this

hope is vain. After plowing though a mixture of nostalgic reminiscing about Chomsky's famous

Syntactic Structures (1957) and *Review of Verbal Behavior* (1959), and incomplete accounts of

rather dated research[2], one discovers that none of Laka's candidates passes the test for UG. That

is none of them is innate, species and domain specific. The low point of the chapter is reached

when an accomplished linguist claims to provide an explanation for center-embedded recursion:

> …the syntactic structures of human languages can resemble matryoshkas, those Russian wooden dolls you open to find smaller but identical dolls nested inside. Consider for instance the English sentence:
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> (1) The girl the boy saw thinks the parrot likes cherries
>
> Here, we find sentences nested inside sentences, and there is no grammatical limit to the number of times I can make a bigger doll, a longer sentence (p. 337).

On non-MP interpretations sentence (1) is not an example of center-embedded recursion. It has a

restrictive relative clause inside the subject "the girl (who) the boy saw" and a complement

clause object. Each manifests one level of embedding. This means that (1) does not have a

matryoshka-like structure. Under current minimalist analysis branching can only be binary. This

creates massively many extra "embeddings" (of which many branches are empty). Here it may

be correct to speak of matryoshka-like structures. But calling these structures "sentences" is

misleading even in minimalist terms. Finally, well-known challenges to MP are simply ignored.

Laka claims: "Of course, [recursion] is not only a property of English, but a property of

language, and the fact that all human grammars can build these matryoshka-structures tells us

that this is a very essential aspect of human language" (Ibid.). Any reader familiar with the issue

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[2] I do not mean to suggest that's own research is dated. Regrettably, she tells the reader very little about her work. Most of the chapter focuses on facts that have been known for a long time, research by non-Chomskyan researchers is left unmentioned, and the discussion of *Syntactic Structures* and the *Review of Verbal Behavior* occurs in several contributions. So the criticism here was directed more at the editors of the volume than at Laka.

knows that findings on Pirahã challenge this universality claim (Everett, 2005). Laka sees no need to even mention a challenge she could not have been ignorant of[3]. None of these shortcomings are corrected in the discussion. Instead, there the reader is subjected to a history lecture of questionable value. It is not clear how these remarks contribute to giving a state of the art account of *what* UG is. It is even less clear how Chomsky's groundless, mean-spirited allegation that Leonard Bloomfield was "completely schizophrenic" (p. 342), voiced twice, could possibly contribute to our understanding of UG or why none of the contributors objected to this slander. Having a genuine interest in UG I felt cheated by this chapter.

A review of this work would not be complete without some attention to Noam Chomsky's contributions. My focus will be on the final chapter in which "virtually all of the different threads spun during the conference finally come together" (p. 9). The reader is told "with his vast knowledge and perspective [Chomsky]… insists on the strangeness of the amnesia that has struck the cognitive sciences in the last couple of decades" (Ibid.). Indeed, the *Concluding Remarks* provide many examples of strangeness. If Chomsky has vast knowledge he hides it well. Instead of salvaging the sinking intellectual ship and correcting the countless factual errors contained in the contributions, he muses about dinner-conversations about Greek philosophy and Hauser's father's moral intuitions. As a state of the art account of the ontology of personhood he provides two fairytale examples (a handsome prince who turned into a frog and baby-donkey Sylvester who turned into a rock, p. 382). From this data set he deduces that already infants have a deep understanding of psychic continuity over time. Chomsky calls this a typical case of a

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[3] Laka may not have considered Everett's work worth mentioning. But this work had received massive attention and was considered as serious threat to the UG hypothesis (e.g., Jackendoff & Pinker, 2005). Challenges of this work (e.g., Nevins et al., 2009) had not been published when the conference in the Basque Country took place (summer 2006). The complete absence of any critial discussion of this work in the entire volume is remarkable.

"semantic or conceptual property that is impossible to identify in material terms… [showing] that there is simply *no* notion of reference in natural language" (p. 383, my emphasis). Drawing such an astounding conclusion from a superficial discussion of fairytales seems justifiable only if we subscribe to Chomsky's understanding of scientific work: "Whatever this crazy thing is that scientists do, it seems… very much disconnected from sort of finding your way in the world" (p. 37). The chapter would not be complete without Chomsky's obfuscation of historic events and misinterpretation of work completed in the cognitive sciences. One example, representative of many others, is this account of Jeff Elman's early work on connectionist networks:

> One of the most quoted connectionist papers is Jeffrey Elman's work on how you can get two nested dependencies. This is true, you can write a program that will do that. But Elman's program totally breaks down when you get to three, and you have to rewrite the whole program. In Turing machine terms, the control unit has to be totally changed, which means you're not capturing the rules. And to make things worse, his approach also works for crossing dependencies, so in the case of the example earlier:
> (4) *The men who John see is tall.
> It works just as well for those. It's not capturing the dependencies, it's just using brute force to go up to two things that you remembered. And that kind of work is never going to get anywhere. There's no point modeling performance that is bounded by time and space, just as you can't study arithmetic knowledge that way (p. 392).

This passage reveals fundamental misunderstandings of Elman's work. Chomsky attributes an absurd view to Elman and rejects, based on this view, the work of all connectionists. However, Elman has never claimed that his program[4] works just as well on nesting and crossing

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[4] Chomsky's use of 'program' indicates that may not be familiar with what Elman actually did. An SRN is not a program – though it is implemented by way of a program. Elman didn't code the SRNs or reprogram them, instead they "learn" from the input they have been exposed to during "training". The use of intentional terminology in reference to SRNs can be criticized. But talking in this context of programs makes no sense.

dependencies. He has not reported *any* work on crossing dependencies and, hence, could not have claimed that his method works on them.

Connectionist work on crossing dependencies has been reported by Morten Christiansen and Nick Chater. Contrary to what Chomsky implies, these researchers are well aware of the difference between nested and crossing dependencies: "The fact that cross-dependencies cannot be handled using a context-free phrase structure grammar … appears to demonstrate that natural language is not context-free" (Christiansen & Chater, 1999, 162). They reported that: "simulation results indicate that SRNs can embody constraints which limit their abilities to process center-embeddings and cross-dependencies to levels similar to human abilities" (Christiansen & Chater, 1999, 165). This means that crossing dependencies are not processed 'just as well' as nested dependencies. They are processed worse than right branching dependencies but better than center embeddings. The authors further comment explicitly on the surprising *difference* in performance between center embeddings and crossing dependencies:

> …the nets appeared to find the cross-dependency language easier to learn than the center-embedding language (at least in terms of their ability to reduce MSE). This is an important result because people appear to be better at dealing with cross-dependency constructions than equivalent center-embedding constructions. This is surprising from the perspective of linguistic theory because, as we noted above, cross-dependency constructions are typically viewed as more complex than center-embedding constructions because they cannot be captured by phrase-structure rules (Christiansen & Chater, 1999, 177)

Given that the performance of humans and SRNs is similar one might suspect that "the language processor cannot be primarily based on a stack-like memory store. This is because cross-dependencies, which require a queue, are easier to process than center embeddings, which

require a stack" (Christiansen & Chater, 1999, 186). Of course, Chomsky could have challenged the findings reported by Christiansen and Chater (1999). But he does not even mention this important paper that has been cited by hundreds of researchers[5]. This indicates a surprising lack of familiarity with research in a field Chomsky so confidently criticizes.

It might be possible that Chomsky was unaware of the work on crossing dependencies that contradicts his claims. However, his remarks about Elman's work are also incorrect. Elman and his coworkers have repeatedly reported differences in performance for *different types of nested dependencies*: "… given the prediction task, the network is more successful at right-branching structures than center-embedded ones" (Weckerly & Elman, 1992, 414, see also below). One important fining of this work was that it showed that "the network's performance parallels that of human listeners" (Ibid., 418). In his criticism, Chomsky never mentions that there are different types of nested dependencies but lumps them all into one category. This is problematic for Chomsky's claim that "[Elman's] program works up to depth two but fails totally on depth three" (226). On right branching dependencies, Elman has not reported any failure on depth three. Regarding the performance for center embedded dependencies Elman reported:

> In the current simulation, the representation degraded after about three levels of embedding. The consequences of this degradation on performance (in the prediction task) are different for different types of sentences. Sentences involving center embedding (e.g., 9c and 9d), in which the level of embedding is crucial for maintaining correct agreement, are more adversely affected than sentences involving so called tail-recursion (e.g., l0d). (Elman, 1991, 215)

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[5] For a current number of citations see Google Scholar:
http://scholar.google.com/citations?view_op=view_citation&hl=en&user=_0jbd88AAAAJ&citation_for_view=_0jbd88AAAAJ:M3NEmzRMIkIC Presumably this number was slightly lower at the time *Of Minds and Language* had been published but it is still surprising that, seemingly, Chomsky was unaware of this work.

Elman does not say that his program fails totally at level three but that representations degrade. Further, he specifically states that there exist differences between sentences involving different types of recursive structures at level three. Yet, Chomsky continues to misrepresent it as if he were struck with amnesia (for discussion of a similar distortion of the same work see Behme, 2009). Chomsky even admitted that he did not understand all the work presented at the conference and expressed "an apology in advance to everyone whose work I misrepresent" (p. 379). This apology should be extended to Elman.

One final example may give the reader a sense of the truly mediocre level of what is offered in this volume. To provide some background information: one proposal in support of MP, the strong minimalist thesis, suggests that language is "an optimal solution to conditions imposed by the interface conditions" (p. 126; see also Chomsky, 1995; Uriagereka, 1998). And, there "are more radical proposals under which optimal satisfaction of semantic conditions becomes close to tautologous" (p. 28). Critics have pointed out that Darwinian evolution does not provide optimal solutions, and that MP is so vague that it would be impossible to decide what an optimal solution to a hopelessly underspecified problem would be (e.g., Johnson & Lappin, 1997; Lappin, Levine & Johnson, 2000; Seuren, 2004). Chomsky offers this suggestion, that can be taken as attempt to refute any criticism because it commits him to nothing: "I think there's every option open from a perfect solution to a minimax problem to a worst possible solution, which is one damn thing after another. Anywhere in there could be some kind of answer to this question" (p. 386). As Fiengo said: Why should anyone still expect Chomsky to follow accepted scientific practice?

The evidence that he does not is overwhelming. Instead, he follows an "abductive instinct" he

attributes to Pierce. This instinct allows him to find theories that are just right and hold on to them no matter how much counter evidence turns up. Chomsky seems to think this is the Einsteinian method: "Einstein didn't have much empirical evidence [for relativity theory]. In fact, there was a great deal of experimentation done …that refuted it, and nobody paid any attention. They didn't pay any attention to the refutations, because it was obviously right" (p. 35). The use of "refutation" here is misleading; none of the experiments he refers to actually refuted the theory of general relativity. Chomsky's own "science" works like this "You just see that some ideas simply look right, and then you sort of put aside the data that refute them and think, *somebody else* will take care of it" (p. 36, my emphasis). This method allows him to be vastly more efficient than traditional scientists. He is not troubled by counter evidence because sooner or later someone else will take care of it. And there is, of course, also no need for conducting experiments or gathering data. After discussing very briefly some differences between human and bee communication Chomsky demonstrates his infallible abductive instinct. Claiming that both systems share some "minimal search principle" he proposes: "And maybe that's the answer. A shot in the dark, but I think it might be a direction to look" (p. 35). The following exchange between Chomsky, who has no training in genetics and never completed any work in that field, and distinguished geneticist Gabriel Dover, is a textbook example of Chomskyan "science".

> Dover: …the whole thrust of modern-day genetics is going against [your] ideas of laws of form and principles of natural law…nothing seems to be obeying laws of form, out of the reach of genes.
> Chomsky: That can't be. I mean, take, say, the division of cells into spheres, not cubes. Is there a gene for that?
> Dover: Yes, of course there is … there are tens upon tens, if not hundreds, of genes directly responsible for very wide-ranging differences in the shapes, sizes, numbers, divisions, life spans, senescence, functions, and behavior of the several hundred types of cells in our species. Cells are not soap bubbles. There are constraints of course but these are a

matter largely of history not of physics, over and above the obvious physics/chemistry of molecular contacts.

Chomsky: No there isn't such a gene. Cells form spheres because that is the least-energy solution … If particular combinations of proteins and molecules and so on do particular things, that is because of physics and chemistry. … all of that is presupposing massive amounts of maybe unknown physical and chemical principles, which are leading things in a certain direction, kind of like cell division into spheres. I mean, there may be a couple of genes involved, but fundamentally it is physical principles. (pp. 38-39).

Here the Chomskyan scientist "refutes" an expert not based on work he has completed himself. Instead, he relies on his abductive instinct, telling him that "conceptually it has to be like this" (p. 40), leaving it up to others to figure out the details.

Should anyone consult *Of Minds & Language*? Presumably, for readers who doubt the legitimacy of recent criticism of the biolinguistic enterprise (e.g., Johnson & Lappin, 1997; Seuren, 2004; Levine & Postal, 2004; Postal, 2004), this volume could be an eye-opener. But readers interested in an accurate account of recent findings in linguistics, psycholinguistics, language acquisition, cognitive neuroscience, comparative cognitive psychology, and evolutionary biology are urged to direct their attention elsewhere.

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