

CHAPTER 1

On Cognitive Capacity

These reflections on the study of language will be non-technical for the most part, and will have a somewhat speculative and personal character. I am not going to try to summarize the current state of knowledge in the areas of language study that I know something about, or to discuss ongoing research in any depth. I want to consider, rather, the point and purpose of the enterprise, to ask—and I hope explain—why results obtained in technical linguistics might interest someone who is not initially enchanted by the relation between question formation and anaphora, the principles of rule ordering in phonology, the relation of intonation to the scope of negation, and the like. I will sketch what seems to me an appropriate framework within which the study of language may prove to have more general intellectual interest, and will consider the possibilities for constructing a kind of theory of human nature on a model of this sort.

Why study language? There are many possible answers, and by focusing on some I do not, of course, mean to disparage others or question their legitimacy. One may, for

example, simply be fascinated by the elements of language in themselves and want to discover their order and arrangement, their origin in history or in the individual, or the ways in which they are used in thought, in science or in art, or in normal social interchange. One reason for studying language—and for me personally the most compelling reason—is that it is tempting to regard language, in the traditional phrase, as “a mirror of mind.” I do not mean by this simply that the concepts expressed and distinctions developed in normal language use give us insight into the patterns of thought and the world of “common sense” constructed by the human mind. More intriguing, to me at least, is the possibility that by studying language we may discover abstract principles that govern its structure and use, principles that are universal by biological necessity and not mere historical accident, that derive from mental characteristics of the species. A human language is a system of remarkable complexity. To come to know a human language would be an extraordinary intellectual achievement for a creature not specifically designed to accomplish this task. A normal child acquires this knowledge on relatively slight exposure and without specific training. He can then quite effortlessly make use of an intricate structure of specific rules and guiding principles to convey his thoughts and feelings to others, arousing in them novel ideas and subtle perceptions and judgments. For the conscious mind, not specially designed for the purpose, it remains a distant goal to reconstruct and comprehend what the child has done intuitively and with minimal effort. Thus language is a mirror of mind in a deep and significant sense. It is a product of human intelligence, created anew in each individual by operations that lie far beyond the reach of will or consciousness.

By studying the properties of natural languages, their structure, organization, and use, we may hope to gain some understanding of the specific characteristics of human in-

telligence. We may hope to learn something about human nature; something significant, if it is true that human cognitive capacity is the truly distinctive and most remarkable characteristic of the species. Furthermore, it is not unreasonable to suppose that the study of this particular human achievement, the ability to speak and understand a human language, may serve as a suggestive model for inquiry into other domains of human competence and action that are not quite so amenable to direct investigation.

The questions that I want to consider are classical ones. In major respects we have not progressed beyond classical antiquity in formulating clear problems in this domain, or in answering questions that immediately arise. From Plato to the present time, serious philosophers have been baffled and intrigued by the question that Bertrand Russell, in one of his later works, formulated in this way: “How comes it that human beings, whose contacts with the world are brief and personal and limited, are nevertheless able to know as much as they do know?” (Russell, 1948, p. 5). How can we gain such rich systems of knowledge, given our fragmentary and impoverished experience? A dogmatic skeptic might respond that we do not have such knowledge. His qualms are irrelevant to the present point. The same question arises, as a question of science, if we ask how comes it that human beings with such limited and personal experience achieve such convergence in rich and highly structured systems of belief, systems which then guide their actions and interchange and their interpretation of experience.

In the classical tradition, several answers were suggested. One might argue, along Aristotelian lines, that the world is structured in a certain way and that the human mind is able to perceive this structure, ascending from particulars to species to genus to further generalization and thus attaining knowledge of universals from perception of particulars. A “basis of pre-existent knowledge” is a prerequisite

to learning. We must possess an innate capacity to attain developed states of knowledge, but these are "neither innate in a determinate form, nor developed from other higher states of knowledge, but from sense-perception." Given rich metaphysical assumptions, it is possible to imagine that a mind "so constituted as to be capable of this process" of "induction" might attain a rich system of knowledge.¹

A more fruitful approach shifts the main burden of explanation from the structure of the world to the structure of the mind. What we can know is determined by "the modes of conception in the understanding";² what we do know, then, or what we come to believe, depends on the specific experiences that evoke in us some part of the cognitive system that is latent in the mind. In the modern period, primarily under the influence of Cartesian thought, the question of what we can know became again a central topic of inquiry. To Leibniz and Cudworth, Plato's doctrine that we do not attain new knowledge but recover what was already known seemed plausible, when this doctrine was "purged of the error of preexistence."³ Cudworth argued at length that the mind has an "innate cognoscitive power" that provides the principles and conceptions that constitute our knowledge, when provoked by sense to do so. "*But sensible things themselves* (as, for example, light and colors) *are not known and understood either by the passion or the fancy of sense, nor by anything merely foreign and adventitious, but by intelligible ideas exerted from the mind itself, that is, by something native and domestic to it...*"⁴ Thus knowledge "consisteth in the awakening and exciting of the inward active powers of the mind," which "exercise[s] its own inward activity upon" the objects presented by sense, thus coming "to know or understand, ... actively to comprehend a thing by some abstract, free and universal ratio's, reasonings. . . ." The eye perceives, but the mind can compare, analyze, see cause-

and-effect relations, symmetries, and so on, giving a comprehensive idea of the whole, with its parts, relations, and proportions. The "book of nature," then, is "legible only to an intellectual eye," he suggests, just as a man who reads a book in a language that he knows can learn something from the "inky scrawls." "The primary objects of science and intellection," namely, "the intelligible essences of things," "exist no where but in the mind itself, being its own ideas. . . . And by and through these inward ideas of the mind itself, which are its primary objects, does it know and understand all external individual things, which are the secondary objects of knowledge only."

Among the "innate ideas" or "common notions" discussed in the rich and varied work of seventeenth-century rationalists are, for example, geometrical concepts and the like, but also "*relational* ideas or categories which enter into every presentation of objects and make possible the unity and interconnectedness of rational experience,"⁵ including such "relative notions" as "Cause, Effect, Whole and Part, Like and Unlike, Proportion and Analogy, Equality and Inequality, Symmetry and Asymmetry," all "*relative* ideas . . . [that are] . . . no material impresses from without upon the soul, but *her own active conception proceeding from herself whilst she takes notice of external objects.*"⁶ Tracing the development of such ideas, we arrive at Kant's rather similar concept of the "conformity of objects to our mode of cognition." The mind provides the means for an analysis of data as experience, and provides as well a general schematism that delimits the cognitive structures developed on the basis of experience.

Returning to Russell's query, we can know so much because in a sense we already knew it, though the data of sense were necessary to evoke and elicit this knowledge. Or to put it less paradoxically, our systems of belief are those that the mind, as a biological structure, is designed to construct. We interpret experience as we do because of our

special mental design. We attain knowledge when the "inward ideas of the mind itself" and the structures it creates conform to the nature of things.

Certain elements of the rationalist theories must be discarded, but the general outlines seem plausible enough. Work of the past years has shown that much of the detailed structure of the visual system is "wired in," though triggering experience is required to set the system in operation. There is evidence that the same may be true of the auditory structures that analyze at least some phonetic distinctive features. (Cf. Eimas et al., 1971.) As techniques of investigation have improved, Bower argues, "so has the apparent sophistication of the infant perceptual system." He reviews evidence suggesting that "the infant perceptual system seems capable of handling all of the traditional problems of the perception of three-dimensional space"—perception of solidity, distance, size-distance invariants, and size constancy. Thus "contrary to the Berkeleian tradition the world of the infant would seem to be inherently tridimensional" (Bower, 1972). There is evidence that before infants are capable of grasping, they can distinguish graspable from ungraspable objects, using purely visual information (Bruner and Koslowski, 1972).

Gregory observes that "the speed with which babies come to associate the properties of objects and go on to learn how to predict hidden properties and future events would be impossible unless some of the structure of the world were inherited—somehow innately built into the nervous system."⁷ He suggests further that there may be a "grammar of vision," rather like the grammar of human language, and possibly related to the latter in the evolution of the species. Employing this "grammar of vision"—largely innate—higher animals are able to "read from retinal images even hidden features of objects, and predict their immediate future states," thus "to classify objects according to an internal grammar, to read reality from their eyes." The neural basis

for this system is gradually coming to be understood since the pioneering work of Hubel and Wiesel (1962). More generally, there is every reason to suppose that "learning behavior occurs via modification of an already functional structural organization"; "survival would be improbable if learning in nature required the lengthy repetition characteristic of most conditioning procedures," and it is well known that animals acquire complex systems of behavior in other ways (John, 1972).

Despite the plausibility of many of the leading ideas of the rationalist tradition, and its affinity in crucial respects with the point of view of the natural sciences, it has often been dismissed or disregarded in the study of behavior and cognition. It is a curious fact about the intellectual history of the past few centuries that physical and mental development have been approached in quite different ways. No one would take seriously a proposal that the human organism learns through experience to have arms rather than wings, or that the basic structure of particular organs results from accidental experience. Rather, it is taken for granted that the physical structure of the organism is genetically determined, though of course variation along such dimensions as size, rate of development, and so forth will depend in part on external factors. From embryo to mature organism, a certain pattern of development is pre-determined, with certain stages, such as the onset of puberty or the termination of growth, delayed by many years. Variety within these fixed patterns may be of great importance for human life, but the basic questions of scientific interest have to do with the fundamental, genetically determined scheme of growth and development that is a characteristic of the species and that gives rise to structures of marvelous intricacy.

The species characteristics themselves have evolved over long stretches of time, and evidently the environment provides conditions for differential reproduction, hence evo-

lution of the species. But this is an entirely different question, and here too, questions can be raised about the physical laws that govern this evolution. Surely too little is known to justify any far-reaching claims.

The development of personality, behavior patterns, and cognitive structures in higher organisms has often been approached in a very different way. It is generally assumed that in these domains, social environment is the dominant factor. The structures of mind that develop over time are taken to be arbitrary and accidental; there is no "human nature" apart from what develops as a specific historical product. According to this view, typical of empiricist speculation, certain general principles of learning that are common in their essentials to all (or some large class of) organisms suffice to account for the cognitive structures attained by humans, structures which incorporate the principles by which human behavior is planned, organized, and controlled. I dismiss without further comment the exotic though influential view that "internal states" should not be considered in the study of behavior.⁸

But human cognitive systems, when seriously investigated, prove to be no less marvelous and intricate than the physical structures that develop in the life of the organism. Why, then, should we not study the acquisition of a cognitive structure such as language more or less as we study some complex bodily organ?

At first glance, the proposal may seem absurd, if only because of the great variety of human languages. But a closer consideration dispels these doubts. Even knowing very little of substance about linguistic universals, we can be quite sure that the possible variety of languages is sharply limited. Gross observations suffice to establish some qualitative conclusions. Thus, it is clear that the language each person acquires is a rich and complex construction hopelessly underdetermined by the fragmentary evidence available. This is why scientific inquiry into the nature of language is so

difficult and so limited in its results. The conscious mind is endowed with no advance knowledge (or, recalling Aristotle, with only insufficiently developed advance knowledge). Thus, it is frustrated by the limitations of available evidence and faced by far too many possible explanatory theories, mutually inconsistent but adequate to the data. Or—as unhappy a state—it can devise no reasonable theory. Nevertheless, individuals in a speech community have developed essentially the same language. This fact can be explained only on the assumption that these individuals employ highly restrictive principles that guide the construction of grammar. Furthermore, humans are, obviously, not designed to learn one human language rather than another; the system of principles must be a species property. Powerful constraints must be operative restricting the variety of languages. It is natural that in our daily life we should concern ourselves only with differences among people, ignoring uniformities of structure. But different intellectual demands arise when we seek to understand what kind of organism a human really is.

The idea of regarding the growth of language as analogous to the development of a bodily organ is thus quite natural and plausible. It is fair to ask why the empiricist belief to the contrary has had such appeal to the modern temper. Why has it been so casually assumed that there exists a "learning theory" that can account for the acquisition of cognitive structures through experience? Is there some body of evidence, established through scientific inquiry, or observation, or introspection, that leads us to regard mental and physical development in such different ways? Surely the answer is that there is not. Science offers no reason to "accept the common maxim that there is nothing in the intellect which was not first in the senses," or to question the denial of this maxim in rationalist philosophy.⁹ Investigation of human intellectual achievements, even of the most commonplace sort, gives no support for this thesis.

Empiricist speculation and the "science of behavior" that has developed within its terms have proved rather barren, perhaps because of the peculiar assumptions that have guided and limited such inquiry. The grip of empiricist doctrine in the modern period, outside of the natural sciences, is to be explained on sociological or historical grounds.¹⁰ The position itself has little to recommend it on grounds of empirical evidence or inherent plausibility or explanatory power. I do not think that this doctrine would attract a scientist who is able to discard traditional myth and to approach the problems afresh. Rather, it serves as an impediment, an insurmountable barrier to fruitful inquiry, much as the religious dogmas of an earlier period stood in the way of the natural sciences.

It is sometimes argued that modern empiricism overcomes the limitations of the earlier tradition, but I think that this belief is seriously in error. Hume, for example, presented a substantive theory of "the secret springs and principles, by which the human mind is actuated in its operations." In his investigation of the foundations of knowledge, he suggested specific principles that constitute "a species of natural instincts." Modern empiricists who disparage Hume have simply replaced his theory by vacuous systems that preserve empiricist (or more narrowly, behaviorist) terminology while depriving traditional ideas of their substance. I have discussed this matter elsewhere (cf. chapter 4), and will not pursue it here.

In recent years, many of these issues, long dormant, have been revived, in part in connection with the study of language. There has been much discussion of the so-called "innateness hypothesis," which holds that one of the faculties of the mind, common to the species, is a faculty of language that serves the two basic functions of rationalist theory: it provides a sensory system for the preliminary analysis of linguistic data, and a schematism that determines, quite narrowly, a certain class of grammars. Each

grammar is a theory of a particular language, specifying formal and semantic properties of an infinite array of sentences. These sentences, each with its particular structure, constitute the language generated by the grammar. The languages so generated are those that can be "learned" in the normal way. The language faculty, given appropriate stimulation, will construct a grammar; the person knows the language generated by the constructed grammar. This knowledge can then be used to understand what is heard and to produce discourse as an expression of thought within the constraints of the internalized principles, in a manner appropriate to situations as these are conceived by other mental faculties, free of stimulus control.¹¹ Questions related to the language faculty and its exercise are the ones that, for me at least, give a more general intellectual interest to the technical study of language.

I would now like to consider the so-called "innateness hypothesis," to identify some elements in it that are or should be controversial, and to sketch some of the problems that arise as we try to resolve the controversy. Then, we may try to see what can be said about the nature and exercise of the linguistic competence that has been acquired, along with some related matters.

A preliminary observation is that the term "innateness hypothesis" is generally used by critics rather than advocates of the position to which it refers. I have never used the term, because it can only mislead. Every "theory of learning" that is even worth considering incorporates an innateness hypothesis. Thus, Hume's theory proposes specific innate structures of mind and seeks to account for all of human knowledge on the basis of these structures, even postulating unconscious and innate knowledge. (Cf. chapter 4.) The question is not whether learning presupposes innate structure—of course it does; that has never been in doubt—but rather what these innate structures are in particular domains.

What is a theory of learning? Is there such a theory as *the* theory of learning, waiting to be discovered? Let us try to sharpen and perhaps take some steps towards answering these questions.

Consider first how a neutral scientist—that imaginary ideal—might proceed to investigate the question. The natural first step would be to select an organism, *O*, and a reasonably well delimited cognitive domain, *D*, and to attempt to construct a theory that we might call “the learning theory for the organism *O* in the domain *D*.” This theory—call it $LT(O,D)$ —can be regarded as a system of principles, a mechanism, a function, which has a certain “input” and a certain “output” (its domain and range, respectively). The “input” to the system $LT(O,D)$ will be an analysis of data in *D* by *O*; the output” (which is, of course, internally represented, not overt and exhibited) will be a cognitive structure of some sort. This cognitive structure is one element of the cognitive state attained by *O*.

For example, take *O* to be humans and *D* language. Then $LT(H,L)$ —the learning theory for humans in the domain language—will be the system of principles by which humans arrive at knowledge of language, given linguistic experience, that is, given a preliminary analysis that they develop for the data of language. Or, take *O* to be rats and *D* to be maze running. Then $LT(R,M)$ is the system of principles used by rats in learning how to run mazes. The input to $LT(R,M)$ is whatever preliminary analysis of data is used by rats to accomplish this feat, and the output is the relevant cognitive structure, however it should properly be characterized as a component of the state achieved by the rat who knows how to run a maze. There is no reason to doubt that the cognitive structure attained and the cognitive state of which it is a constituent will be rather complex.

To facilitate the discussion, let us make two simplifying assumptions. Assume first that individuals of the species *O* under investigation are essentially identical with respect to

their ability to learn over the domain *D*—for example, that humans do not differ in language-learning capacity. Second, assume that learning can be conceptualized as an instantaneous process in the following sense: assume that $LT(O,D)$ is presented with a cumulative record of all the data available to *O* up to a particular moment, and that $LT(O,D)$, operating on that data, produces the cognitive structure attained at that moment. Neither of these assumptions is true: there are individual differences, and learning takes place over time, sometimes extended time. I will return later to the question of just “how false” these assumptions are. I think that they give a useful first approximation, helpful for the formulation of certain issues and possibly much more.

To pursue the study of a given $LT(O,D)$ in a rational way, we will proceed through the following stages of inquiry:

1. Set the cognitive domain *D*.
2. Determine how *O* characterizes data in *D* “pretheoretically,” thus constructing what we may call “the experience of *O* in *D*” (recall the idealization to “instantaneous learning”).
3. Determine the nature of the cognitive structure attained; that is, determine, as well as possible, what is learned by *O* in the domain *D*.
4. Determine $LT(O,D)$, the system that relates experience to what is learned.

Step 4 relies on the results attained in steps 2 and 3.

To avoid misunderstanding, perhaps I should stress that the ordering of steps is a kind of rational reconstruction of rational inquiry. In practice, there is no strict sequence. Work at level 4, for example, may convince us that our original delimitation of *D* was faulty, that we have failed to abstract a coherent cognitive domain. Or, it may lead us

to conclude that we have misconstrued the character of what is learned, at step 3. It remains true, nevertheless, that we can hope to gain some insight at the level of step 4 only to the extent that we have achieved some understanding at levels 2 and 3 and have selected, wisely or luckily, at level 1. It is senseless to try to relate two systems—in this case, experience and what is learned—without some fairly good idea of what they are.

Parenthetically, we might observe that step 3 is missing in many formulations of psychological theory, much to their detriment. In fact, even the concept “what is learned” is missing in familiar “learning theories.” Where it is missing, the basic questions of “learning theory” cannot even be formulated.

How does the study of behavior fit into this framework? Surely a prerequisite to the study of behavior is a grasp of the nature of the organism that is behaving—in the sense of “prerequisite” just explained. An organism has attained a certain state through maturation and experience. It is faced with certain objective conditions. It then does something. In principle, we might want to inquire into the mechanism *M* that determines what the organism does (perhaps probabilistically) given its past experience and its present stimulus conditions. I say “in principle,” because I doubt that there is very much that we will be able to say about this question.

No doubt what the organism does depends in part on its experience, but it seems to me entirely hopeless to investigate directly the relation between experience and action. Rather, if we are interested in the problem of “causation of behavior” as a problem of science, we should at least analyze the relation of experience to behavior into two parts: first, *LT*, which relates experience to cognitive state,¹² and second, a mechanism, *M_{CS}*, which relates stimulus conditions to behavior, given the cognitive state *CS*.

To put it schematically, in place of the hopeless task of

investigating *M* as in (I), we may more reasonably undertake research into the nature of *LT* as in (II) and *M_{CS}* as in (III).

- (I) *M*: (experience, stimulus conditions) → behavior
- (II) *LT*: experience → cognitive state *CS*
- (III) *M_{CS}*: stimulus conditions → behavior (given *CS*)

I think that we can make considerable progress towards understanding *LT* as in (II); that is, towards understanding particular *LT(O,D)*'s, for various choices of *D* given *O*, and the interaction among them. It is this problem that I want to consider here. I doubt that we can learn very much, as scientists at least, about the second of these two parts, *M_{CS}*.¹³ But it seems to me most unlikely that there will be any scientific progress at all if we do not at least analyze the problem of “causation of behavior” into the two components *LT* and *M_{CS}* and their elements. An attempt along the lines of (I) to study directly the relation of behavior to past and current experience is doomed to triviality and scientific insignificance.

Returning to the problem of learning, suppose that we have determined a number of *LT(O,D)*'s, for various choices of organism *O* and cognitive domain *D*. We can now turn to the question: What is “learning theory”? Or better: Is there such a theory as learning theory? The question might be put in various ways, for example, the following two:

- (1) Is it the case that however we select *O* and *D*, we find the same *LT(O,D)*?
- (2) Are there significant features common to all *LT(O,D)*'s?

Before considering these questions, let us return to the first of our simplifying assumptions, namely, with regard to variability within the species *O*. I would like to suggest

that the interesting questions of "learning theory," those that might lead to a theory that is illuminating and that will ultimately relate to the body of natural science more generally, will be those for which our first assumption is essentially correct. That is, the interesting questions, those that offer some hope of leading to insight into the nature of organisms, will be those that arise in the investigation of learning in domains where there is a nontrivial structure uniform for members of *O* (with certain parameters relating to rapidity of learning, scope of learning, rate of forgetting, and other such marginal phenomena for which variability is to be expected). These are the questions that deal with significant characteristics of the species, or perhaps, of organisms generally. Again, I see no reason why cognitive structures should not be investigated rather in the way that physical organs are studied. The natural scientist will be primarily concerned with the basic, genetically determined structure of these organs and their interaction, a structure common to the species in the most interesting case, abstracting away from size, variation in rate of development, and so on.

If we can accept this judgment, then $LT(O,D)$ can be characterized for *O* taken not as an individual but as a species—hence for individuals apart from gross abnormalities. And we may proceed to qualify question (1), asking whether $LT(O,D)$ is identical with $LT(O',D')$ apart from such matters as rapidity, facility, scope, and retention, which may vary across species and, to a lesser extent, among individuals of a given species.

Consider now question (1), so qualified. Surely the answer must still be a firm No. Even the crudest considerations suffice to show that there is no hope of reaching a positive answer to this question. Take *O* to be humans (*H*) and *O'* rats (*R*); *D* to be language (*L*) and *D'* maze running (*M*). If even some vague approximation to question (1) had a positive answer, we would expect humans to be as much

superior to rats in maze-learning ability as they are in language-learning ability. But this is so grossly false that the question cannot be seriously entertained. Humans are roughly comparable to rats in the domain *M* but incomparable in the domain *L*. In fact, it seems that "white rats can even best college students in this sort of learning"—namely, maze-learning (Munn, 1971, p. 118). The distinction between the pair $(LT(H,L), LT(R,L))$ on the one hand and the pair $(LT(H,M), LT(R,M))$ on the other cannot be attributed to sensory processing systems and the like, as we can see by "transposing" language into some modality accessible to rats. (Cf. chapter 4, note 14.) As far as is now known—and I say this despite suggestions to the contrary—the same is true if we consider other organisms (say, chimpanzees) in place of rats. Putting this interesting but peripheral question to the side, it is surely obvious at once that no version of question (1) is worth pursuing.

Let us turn to the more plausible speculation formulated in question (2). No answer is possible, for the present. The question is hopelessly premature. We lack an interesting conception of $LT(O,D)$ for various choices of *O* and *D*. There are, I believe, some substantive steps possible towards $LT(H,L)$, but nothing comparable in other domains of human learning. What is known about other animals, to my knowledge, suggests no interesting answer to (2). Animals learn to care for their young, build nests, orient themselves in space, find their place in a dominance structure, identify the species, and so on, but we should not expect to find significant properties which are common to the various $LT(O,D)$'s that enter into these achievements. Skepticism about question (2) is very much in order, on the basis of the very little that is known. I should think that for the biologist, the comparative physiologist, or the physiological psychologist, such skepticism would appear quite unremarkable.

Thus, for the present, there seems to be no reason to

suppose that learning theory exists. At least, I see no interesting formulation of the thesis that there is such a theory that has initial plausibility or significant empirical support.

Within the odd variant of empiricism known as "behaviorism," the term "learning theory" has commonly been used, not as the designation of a theory (if it exists) that accounts for the attainment of cognitive structures on the basis of experience (namely, (II) above), but rather as a theory that deals with the relation of experience to behavior (namely, (I) above). Since there is no reason to suppose that learning theory exists, there is certainly no reason to expect that such a "theory of behavior" exists.

We might consider contentions more plausible than those implicit in questions (1) and (2). Suppose that we fix the organism O , and let D range over various cognitive domains. Then we might ask whether there is some interesting set of domains D_1, \dots, D_n such that:

- (3) $LT(O, D_i) = LT(O, D_j)$; or $LT(O, D_i)$ is similar in interesting ways to $LT(O, D_j)$.

There might be some way of delimiting domains that would yield a positive answer to (3). If so, we could say that within this delimitation, the organism learns in similar or identical ways across cognitive domains. It would be interesting, for example, to discover whether there is some cognitive domain D other than language for which $LT(H, L)$ is identical to or similar to $LT(H, D)$. To date, no persuasive suggestion has been made, but conceivably there is such a domain. There is no particular reason to expect that there is such a domain, and one can only be surprised at the dogmatic view, commonly expressed, that language learning proceeds by application of general learning capacities. The most that we can say is that the possibility is not excluded, though there is no evidence for it and little plausibility to the contention. Even at the level of sensory processing there appear to be adaptations directly related to

language, as already noted.¹⁴ The proposal that language learning is simply an instance of "generalized learning capacities" makes about as much sense, in the present state of our knowledge, as a claim that the specific neural structures that provide our organization of visual space must be a special case of the class of systems involved also in language use. This is true, so far as we know, only at a level so general as to give no insight into the character or functioning of the various systems.

For any organism O , we can try to discover those cognitive domains D for which the organism O has an interesting $LT(O, D)$ —that is, an $LT(O, D)$ that does not merely have the structure of trial-and-error learning, generalization along physically given dimensions, induction (in any well-defined sense of this notion), and so on. We might define the "cognitive capacity" of O as the system of domains D for which there is an interesting learning theory $LT(O, D)$ in this sense.¹⁵ For D within the cognitive capacity of O , it is reasonable to suppose that a schematism exists delimiting the class of cognitive structures that can be attained. Hence it will be possible, for such D , for a rich, complex, highly articulated cognitive structure to be attained with considerable uniformity among individuals (apart from matters of rate, scope, persistence, etc.) on the basis of scattered and restricted evidence.

Investigating the cognitive capacity of humans, we might consider, say, the ability to recognize and identify faces on exposure to a few presentations, to determine the personality structure of another person on brief contact (thus, to be able to guess, pretty well, how that person will react under a variety of conditions), to recognize a melody under transposition and other modifications, to handle those branches of mathematics that build on numerical or spatial intuition, to create art forms resting on certain principles of structure and organization, and so on. Humans appear to have characteristic and remarkable abilities in these do-

mains, in that they construct a complex and intricate intellectual system, rapidly and uniformly, on the basis of degenerate evidence. And structures created by particularly talented individuals within these constraints are intelligible and appealing, exciting and thought-provoking even to those not endowed with unusual creative abilities. Inquiry, then, might lead to nontrivial $LT(H,D)$'s, for D so chosen. Such inquiry might involve experimentation or even historical investigation—for example, investigation of developments in forms of artistic composition or in mathematics that seemed “natural” and proved fruitful at particular historical moments, contributing to a “mainstream” of intellectual evolution rather than diverting energy to an unproductive side channel.¹⁶

Suppose that for a particular organism O , we manage to learn something about its cognitive capacity, developing a system of $LT(O,D)$'s for various choices of D with the rough properties sketched above. We would then have arrived at a theory of the mind of O , in one sense of this term. We may think of “the mind of O ,” to adapt a formulation of Anthony Kenny's,¹⁷ as the innate capacity of O to construct cognitive structures, that is, to learn.

I depart here from Kenny's formulation in two respects, which perhaps deserve mention. He defines “mind” as a second-order capacity to acquire “intellectual abilities,” such as knowledge of English—the latter “itself a capacity or ability: an ability whose exercise is the speaking, understanding, reading of English.” Moreover, “to have a mind is to have the capacity to acquire the ability to operate with symbols in such a way that it is one's own activity that makes them symbols and confers meaning on them,” so that automata operating with formal elements that are symbols for us but not for them do not have minds. For the sake of this discussion, I have generalized here beyond first-order capacities involving operations with symbols, and am thus considering second-order capacities broader than

“mind” in Kenny's quite natural sense. So far there is no issue beyond terminology. Secondly, I want to consider mind (in the narrower or broader sense) as an innate capacity to form cognitive structures, not first-order capacities to act. The cognitive structures attained enter into our first-order capacities to act, but should not be identified with them. Thus it does not seem to me quite accurate to take “knowledge of English” to be a capacity or ability, though it enters into the capacity or ability exercised in language use. In principle, one might have the cognitive structure that we call “knowledge of English,” fully developed, with no capacity to use this structure;¹⁸ and certain capacities to carry out “intellectual activities” may involve no cognitive structures but merely a network of dispositions and habits, something quite different.¹⁹ Knowledge, understanding, or belief is at a level more abstract than capacity.

There has been a tendency in modern analytic philosophy to employ the notion “disposition” or “capacity” where the more abstract concept of “cognitive structure” is, I believe, more appropriate. (Cf. chapter 4; also Chomsky, 1975a.) I think we see here an unfortunate residue of empiricism. The notions “capacity” and “family of dispositions” are more closely related to behavior and “language use”; they do not lead us to inquire into the nature of the “ghost in the machine” through the study of cognitive structures and their organization, as normal scientific practice and intellectual curiosity would demand. The proper way to exorcise the ghost in the machine is to determine the structure of the mind and its products.²⁰ There is nothing essentially mysterious about the concept of an abstract cognitive structure, created by an innate faculty of mind, represented in some still-unknown way in the brain, and entering into the system of capacities and dispositions to act and interpret. On the contrary, a formulation along these lines, embodying the conceptual

competence-performance distinction (cf. Chomsky, 1965, chap. 1) seems a prerequisite for a serious investigation of behavior. Human action can be understood only on the assumption that first-order capacities and families of dispositions to behave involve the use of cognitive structures that express systems of (unconscious) knowledge, belief, expectation, evaluation, judgment, and the like. At least, so it seems to me.

Returning to the main theme, suppose that we now select a problem in a domain D that falls outside of O 's cognitive capacity. O will then be at a loss as to how to proceed. O will have no cognitive structure available for dealing with this problem and no $LT(O,D)$ available to enable it to develop such a structure. O will therefore have to proceed by trial and error, association, simple induction, and generalization along certain available dimensions (some questions arise here, which I put aside). Taking O to be humans, we will not expect the person to be able to find or construct a rich and insightful way to deal with the problem, to develop a relevant cognitive structure in the intuitive, unconscious manner characteristic of language learning and other domains in which humans excel.

Humans might be able to construct a conscious scientific theory dealing with problems in the domain in question, but that is a different matter—or better, a partially different matter, since even here there are crucial constraints. An intellectually significant science, an intelligible explanatory theory, can be developed by humans in case something close to the true theory in a certain domain happens to fall within human “science-forming” capacities. The $LT(H,D)$'s involved in scientific inquiry, whatever they may be, must be special and restrictive, or it would be impossible for scientists to converge in their judgment on particular explanatory theories that go far beyond the evidence at hand, as they customarily do in

those few fields where there really is significant progress, while at the same time rejecting much evidence as irrelevant or beside the point, for the moment at least. The same $LT(H,D)$'s that provide for the vast and impressive scope of scientific understanding must also sharply constrain the class of humanly accessible sciences. There is, surely, no evolutionary pressure that leads humans to have minds capable of discovering significant explanatory theories in specific fields of inquiry. Thinking of humans as biological organisms in the natural world, it is only a lucky accident if their cognitive capacity happens to be well matched to scientific truth in some area. It should come as no surprise, then, that there are so few sciences, and that so much of human inquiry fails to attain any intellectual depth. Investigation of human cognitive capacity might give us some insight into the class of humanly accessible sciences, possibly a small subset of those potential sciences that deal with matters concerning which we hope (vainly) to attain some insight and understanding.

As a case in point, consider our near-total failure to discover a scientific theory that provides an analysis of Mcs of (III) on page 17—that is, our very limited progress in developing a scientific theory of any depth to account for the normal use of language (or other aspects of behavior). Even the relevant concepts seem lacking; certainly, no intellectually satisfying principles have been proposed that have explanatory force, though the questions are very old. It is not excluded that human science-forming capacities simply do not extend to this domain, or any domain involving the exercise of will, so that for humans, these questions will always be shrouded in mystery.

Note, incidentally, how misleading it would be to speak simply of “limitations” in human science-forming capacity. Limits no doubt exist, but they derive from the same source as our ability to construct rich cognitive systems on

the basis of limited evidence in the first place. Were it not for the factors that limit scientific knowledge, we could have no such knowledge in any domain.²¹

Suppose that in investigating organisms, we decide, perversely, to restrict ourselves to tasks and problems that lie outside their cognitive capacity. We might then expect to discover simple "laws of learning" of some generality. Suppose further that we define a "good experiment" as one that provides smooth learning curves, regular increments and extinction, and so on. Then there will be "good experiments" only in domains that lie outside of O's cognitive capacity. For example, there will be no "good experiments" in the study of human language learning, though there may be if we concentrate attention on memorization of nonsense syllables, verbal association, and other tasks for which humans have no special abilities.

Suppose now that some branch of inquiry develops, limited in principle to "good experiments" in something like this sense. This discipline may, indeed, develop laws of learning that do not vary too greatly across cognitive domains for a particular organism and that have some cross-species validity. It will, of necessity, avoid those domains in which an organism is specially designed to acquire rich cognitive structures that enter into its life in an intimate fashion. The discipline will be of virtually no intellectual interest, it seems to me, since it is restricting itself in principle to those questions that are guaranteed to tell us little about the nature of organisms. For we can learn something significant about this nature only by inquiry into the organism's cognitive capacity, inquiry that will permit no "good experiments" in the strange sense just specified, though it may lead to the discovery (through experiment and observation) of intricate and no doubt highly specific LT(O,D)'s. The results and achievements of this perversely limited, rather suicidal discipline are largely an artifact. It will be condemned in principle to

investigation of peripheral matters such as rate and scope of acquisition of information, the relation between arrangement of reinforcers and response strength, control of behavior, and the like. The discipline in question may continue indefinitely to amass information about these matters, but one may question the point or purpose of these efforts.

A more elaborate study of cognitive capacity raises still further questions. Thus, some intellectual achievements, such as language learning, fall strictly within biologically determined cognitive capacity. For these tasks, we have "special design," so that cognitive structures of great complexity and interest develop fairly rapidly and with little if any conscious effort. There are other tasks, no more "complex" along any absolute scale (assuming that it is possible even to make sense of this notion), which will be utterly baffling because they fall beyond cognitive capacity. Consider problems that lie at the borderline of cognitive capacity. These will provide opportunity for intriguing intellectual play. Chess, for example, is not so remote from cognitive capacity as to be merely a source of insoluble puzzles, but is at the same time sufficiently beyond our natural abilities so that it is challenging and intriguing. Here, we would expect to find that the slight differences between individuals are magnified to striking divergence of aptitude.

The study of challenging intellectual tasks might give some insight into human intelligence, at the borders of cognitive capacity, just as the study of the ability to run a four-minute mile may give useful information about human physiology. But it would be pointless to study the latter feat at a very early stage of our understanding of human locomotion—say, if we knew only that humans walk rather than fly. Correspondingly, in the present state of our understanding of mental abilities, it seems to me that, for example, the study of chess-playing programs

may teach something about the theory of chess, but is unlikely to contribute much to the study of human intelligence. It is good procedure to study major factors before turning to tenth-order effects, to study the basic character of an intricate system before exploring its borders, though of course one can never know in advance just what line of inquiry will provide sudden illumination.²²

In the case of human cognition, it is the study of the basic cognitive structures within cognitive capacity, their development and use, that should receive priority, I believe, if we are to attain a real understanding of the mind and its workings.

The preceding discussion is not very precise. I hope that it is at least suggestive as to how a rational study of learning might proceed. Let me now turn to the particular questions in the "theory of learning" that concern language.

Let us take O to be humans (H) and D to be language (L). What is LT(H,L)? Of the two simplifying assumptions mentioned earlier, the first—invariability across the species—is, so far as we know, fair enough. It seems to provide a close approximation to the facts. Let us therefore accept it with no further discussion, while keeping a cautious and skeptical eye on the second assumption, that learning is "instantaneous." I will return to the latter in chapter 3.

LT(H,L) is the system of mechanisms and principles put to work in acquisition of knowledge of language—acquisition of the specific cognitive structure that we are calling "grammar"—given data which are a fair and adequate sample of this language.²³ The grammar is a system of rules and principles that determine the formal and semantic properties of sentences. The grammar is put to use, interacting with other mechanisms of mind, in speaking and understanding language. There are empirical assump-

tions and conceptual distinctions embedded in this account, and they might be wrong or misguided, but I think it is not unreasonable, given present understanding, to proceed with them.

To relate these remarks to earlier discussion, note that I am insisting that the relation of experience to action be subdivided into two systems: LT(H,L), which relates experience to cognitive state attained, and M_{CS} , which relates current conditions to action, given cognitive state attained (cf. (II)–(III), p. 17). One of the cognitive structures entering into the cognitive state CS attained and put to use by M_{CS} is grammar. Again, I see few present prospects for the scientific study of M_{CS} , though the study of LT(H,L), it seems to me, can be profitably pursued.

Let us define "universal grammar" (UG) as the system of principles, conditions, and rules that are elements or properties of all human languages not merely by accident but by necessity—of course, I mean biological, not logical, necessity. Thus UG can be taken as expressing "the essence of human language." UG will be invariant among humans. UG will specify what language learning must achieve, if it takes place successfully. Thus UG will be a significant component of LT(H,L). What is learned, the cognitive structure attained, must have the properties of UG, though it will have other properties as well, accidental properties. Each human language will conform to UG; languages will differ in other, accidental properties. If we were to construct a language violating UG, we would find that it could not be learned by LT(H,L). That is, it would not be learnable under normal conditions of access and exposure to data. Possibly it could be learned by application of other faculties of mind; LT(H,L) does not exhaust the capacities of the human mind. This invented language might be learned as a puzzle, or its grammar

might be discovered by scientific inquiry over the course of generations, with the intervention of individual genius, with explicit articulation of principles and careful experimentation. This would be possible if the language happened to fall within the bounds of the "science-forming" component of human cognitive capacity. But discovery of the grammar of this language would not be comparable to language learning, just as inquiry in physics is qualitatively different from language learning.

UG will specify properties of sound, meaning, and structural organization. We may expect that in all of these domains, UG will impose conditions that narrowly restrict the variety of languages. For familiar reasons, we cannot conclude from the highly restrictive character of UG that there is a translation procedure of any generality or significance, even in principle (cf. Chomsky, 1965). And quite obviously, nothing is implied about the possibility of translating actual texts, since a speaker or writer naturally presupposes a vast background of unspecified assumptions, beliefs, attitudes, and conventions. The point is perhaps worth noting, since there has been much confusion about the matter. For some discussion, see Keyser (1975).

We can gain some insight into UG, hence LT(H,L), whenever we find properties of language that can reasonably be supposed not to have been learned. To make the discussion more concrete, consider a familiar example, perhaps the simplest one that is not entirely trivial. Think of the process of forming questions in English. Imagine again our neutral scientist, observing a child learning English. Suppose that he discovers that the child has learned to form such questions as those of (A), corresponding to the associated declaratives:

- (A) the man is tall—is the man tall?
the book is on the table—is the book on the table?
etc.

Observing these facts, the scientist might arrive at the following tentative hypothesis as to what the child is doing, assuming now that sentences are analyzed into words:

Hypothesis 1: The child processes the declarative sentence from its first word (i.e., from "left to right"), continuing until he reaches the first occurrence of the word "is" (or others like it: "may," "will," etc.); he then preposes this occurrence of "is," producing the corresponding question (with some concomitant modifications of form that need not concern us).

This hypothesis works quite well. It is also extremely simple. The scientist has every right to be satisfied, and will be able to find a great deal of evidence to support his tentative hypothesis. Of course, the hypothesis is false, as we learn from such examples as (B) and (C):

- (B) the man who is tall is in the room—is the man who is tall in the room?
(C) the man who is tall is in the room—is the man who tall is in the room?

Our scientist would discover, surely, that on first presentation with an example such as "the man who is tall is in the room," the child unerringly forms the question (B), not (C) (if he can handle the example at all). Children make many mistakes in language learning, but never mistakes such as exemplified in (C). If the scientist is reasonable, this discovery will surprise him greatly, for it shows that his simple hypothesis 1 is false, and that he must construct a far more complex hypothesis to deal with the facts. The correct hypothesis is the following, ignoring complications that are irrelevant here:

Hypothesis 2: The child analyzes the declarative sentence into abstract phrases; he then locates the first occurrence of "is" (etc.) that follows the first noun

phrase; he then preposes this occurrence of "is," forming the corresponding question.

Hypothesis 1 holds that the child is employing a "structure-independent rule"—that is, a rule that involves only analysis into words and the property "earliest" ("left-most") defined on word sequences. Hypothesis 2 holds that the child is employing a "structure-dependent rule," a rule that involves analysis into words and phrases, and the property "earliest" defined on sequences of words analyzed into abstract phrases. The phrases are "abstract" in the sense that neither their boundaries nor their categories (noun phrase, verb phrase, etc.) need be physically marked. Sentences do not appear with brackets, intonation boundaries regularly marking phrases, subscripts identifying the type of phrase, or anything of the sort.

By any reasonable standards, hypothesis 2 is far more complex and "unlikely" than hypothesis 1. The scientist would have to be driven by evidence, such as (B), (C), to postulate hypothesis 2 in place of the simpler and more elementary hypothesis 1. Correspondingly, the scientist must ask why it is that the child unerringly makes use of the structure-dependent rule postulated in hypothesis 2, rather than the simpler structure-independent rule of hypothesis 1. There seems to be no explanation in terms of "communicative efficiency" or similar considerations. It is certainly absurd to argue that children are trained to use the structure-dependent rule, in this case. In fact, the problem never arises in language learning. A person may go through a considerable part of his life without ever facing relevant evidence, but he will have no hesitation in using the structure-dependent rule, even if all of his experience is consistent with hypothesis 1. The only reasonable conclusion is that UG contains the principle that all such rules must be structure-dependent. That is, the child's mind (specifically, its component $LT(H,L)$) con-

tains the instruction: Construct a structure-dependent rule, ignoring all structure-independent rules. The principle of structure-dependence is not learned, but forms part of the conditions for language learning.

To corroborate this conclusion about UG (hence $LT(H,L)$), the scientist will ask whether other rules of English are invariably structure-dependent. So far as we know, the answer is positive. If a rule is found that is not structure-dependent, the scientist will be faced with a problem. He will have to inquire further into UG, to discover what additional principles differentiate the two categories of rules, so that the child can know without instruction that one is structure-dependent and the other not. Having gotten this far, the scientist will conclude that other languages must have the same property, on the assumption that humans are not specifically designed to learn one rather than another language, say English rather than Japanese. On this reasonable assumption, the principle of structure-dependence (perhaps, if necessary, qualified as indicated above) must hold universally, if it holds for English. Investigating the consequences of his reasoning, the scientist would discover (so far as we know) that the conclusion is correct.

More complex examples can be produced, but this simple one illustrates the general point. Proceeding in this way, the scientist can develop some rich and interesting hypotheses about UG, hence $LT(H,L)$. Thus, learning theory for humans in the domain of language incorporates the principle of structure-dependence along with other more intricate (and, I should add, more controversial) principles like it. I will return to some of these in the third chapter.

Keeping this single example of a principle of UG in mind, let us return now to the "innateness hypothesis." Recall that there is no issue as to the necessity for such a hypothesis, only as to its character.

Assuming still the legitimacy of the simplifying assumption about instantaneous learning, the "innateness hypothesis" will consist of several elements: principles for the preliminary, pretheoretic analysis of data as experience, which serves as input to $LT(H,L)$; properties of UG , which determine the character of what is learned; other principles of a sort not discussed in the foregoing sketch.

We might, quite reasonably, formulate the *theory of language* so as to reflect this way of looking at $LT(H,L)$. A theory is a system of principles expressed in terms of certain concepts. The principles are alleged to be true of the subject matter of the theory. A particular presentation of a theory takes some of the concepts as primitive and some of the principles as axioms. The choice of primitives and axioms must meet the condition that all concepts are defined in terms of the primitives and that all principles derive from the axioms. We might choose to formulate linguistic theory by taking its primitive concepts to be those that enter into the preliminary analysis of data as experience, with the axioms including those principles expressing relations between the primitive concepts that enter into this preliminary analysis (thus, the primitive notions are "epistemologically primitive"; they meet an external empirical condition apart from sufficiency for definition). The defined terms belong to UG , and the principles of UG will be theorems of this theory. Linguistic theory, so construed, is a theory of UG incorporated into $LT(H,L)$ in the manner described.

The "innateness hypothesis," then, can be formulated as follows: Linguistic theory, the theory of UG , construed in the manner just outlined, is an innate property of the human mind. In principle, we should be able to account for it in terms of human biology.

To the extent that our simplifying assumption about instantaneous learning must be revised, along lines to which

I will return, we must accordingly complicate the "innateness hypothesis."

A fuller version of the "innateness hypothesis" for humans will specify the various domains belonging to cognitive capacity, the faculty of mind $LT(H,D)$ for each such domain D , the relations between these faculties, their modes of maturation, and the interactions among them through time. Alongside of the language faculty and interacting with it in the most intimate way is the faculty of mind that constructs what we might call "common-sense understanding," a system of beliefs, expectations, and knowledge concerning the nature and behavior of objects, their place in a system of "natural kinds," the organization of these categories, and the properties that determine the categorization of objects and the analysis of events. A general "innateness hypothesis" will also include principles that bear on the place and role of people in a social world, the nature and conditions of work, the structure of human action, will and choice, and so on. These systems may be unconscious for the most part and even beyond the reach of conscious introspection. One might also want to isolate for special study the faculties involved in problem solving, construction of scientific knowledge, artistic creation and expression, play, or whatever prove to be the appropriate categories for the study of cognitive capacity, and derivatively, human action.

In the next two chapters I want to say something more about a few of these mental faculties and their interaction.

Notes

Part I THE WHIDDEN LECTURES

Chapter 1. ON COGNITIVE CAPACITY

1. Aristotle, *Posterior Analytics* 2. 19 (ed. McKeon, 1941), pp. 184–6.
2. Cudworth (1838), p. 75. Except for those otherwise identified, quotations that follow in this paragraph are from the same source: respectively, pp. 65, 51, 49, 87, 122–3.
3. Leibniz, *Discourse on Metaphysics* (trans. Montgomery, 1902), p. 45. For a similar view, see Cudworth (1838), p. 64. For quotations and further discussion, see Chomsky (1966), § 4.
4. Cudworth, *True Intellectual System of the Universe*, cited by Lovejoy (1908).
5. Lovejoy (1908).
6. Henry More, “Antidote Against Atheism,” cited by Lovejoy (1908).
7. Gregory (1970). Gregory suggests further that the grammar of language “has its roots in the brain’s rules for ordering retinal patterns in terms of objects,” that is, “in a take-over operation, in which man cashed in on” the development of the visual system in higher animals. This seems questionable. The structure, use, and acquisition of language seem to involve special properties that are, so far as is known, not found elsewhere. Language is based on properties of the dominant hemisphere that may also be quite specialized. There seems to be no obvious relationship to the structure of the visual cortex in relevant respects, though so little is known that one can only speculate. It is not clear why one should expect to find an evolutionary explanation of the sort that Gregory suggests. For more on these matters, see the chapters by R. W. Sperry, A. M. Liberman, H.-L. Teuber, and B. Milner in Schmitt, and Worden (1974).
8. This view, popularized in recent years by B. F. Skinner, is foreign to science or any rational inquiry. The reasons for its popularity must be explained on extrascientific grounds. For

- further discussion, see my "Psychology and Ideology," reprinted in Chomsky (1973b); also Chomsky (1973c); and the discussion of liberalism and empiricism in Bracken (1972, 1973a).
9. Antoine Arnauld (1964), p. 36. On the importance of considering "language as a biological phenomenon [comparable] to other biological phenomena" and some implications for epistemology and the philosophy of language and mind, see Moravcsik (1975b).
 10. See, for example, the references of note 8. I return to the question in chapter 3.
 11. Appropriateness is not to be confused with control, nor can the properties of language use noted here (what I have elsewhere called "the creative aspect of language use") be identified with the recursive property of grammars. Failure to keep these very different concepts separate has led to much confusion. For discussion of the creative aspect of language use in rationalist theory, see Chomsky (1966) and (1972a).
 12. By LT I mean here the system of $LT(O,D)$'s, O fixed, D an arbitrary domain. In the terminology suggested, each $LT(O,D)$ constructs a cognitive structure. Operating in concert and interaction, the $LT(O,D)$'s for given O form a cognitive state.
 13. Hence I will not even raise the further question whether there is anything to say about M_2 (CS, stimulus conditions), namely, a possible general mechanism ranging over cognitive states that might be called a "general theory of behavior."
 14. See Eimas et al. (1971) and the references of note 7 above.
 15. Thus we take cognitive capacity to be the set of such domains with whatever further structure this complex may have.
 16. Consider the argument of Bourbaki that "as a matter of empirical fact, the bulk of mathematically significant notions can be analyzed profitably in terms of a *few* basic structures such as groups or topological spaces. They regard this fact as a discovery about our thinking . . ." (Kreisel, 1974).
 17. Anthony Kenny, "The Origin of the Soul," in Kenny et al. (1973).
 18. Imagine some hypothetical form of aphasia in which knowledge is unimpaired but all systems involving performance, i.e., putting knowledge to use, are destroyed. For discussion of this matter, see Stich (1972) and Chomsky and Katz (1974).
 19. Thus my use of the term "cognitive capacity" (p. 21) might be misleading, though I have not found a term less likely to mislead.
 20. I have discussed elsewhere why I think that modern criticisms of "Descartes's myth" by Ryle and others simply miss the point. Cf. Chomsky (1966), p. 12; (1972a), p. 13; (1975a).
 21. On this matter, see chapter 4. Also, Chomsky (1972a), pp. 90ff.; (1971), pp. 20ff.

22. To avoid misunderstanding, I am not making the absurd suggestion that science should study what is familiar and commonplace rather than search for perhaps exotic data that shed light on deeper principles. In the study of language no less than physics, this would be a self-defeating program. Gross coverage of familiar phenomena can be achieved by very different theories; it is generally necessary to seek unusual data to distinguish them. To cite an example, idioms in natural language are (by definition) abnormal, but the capacity of various linguistic theories to deal with their character and peculiarities has often proved quite relevant for distinguishing empirically among these theories.
23. Note that the notions "fair" and "adequate" are yet to be made precise, as biological properties of humans, though it is no great problem to place some reasonable bounds.

Chapter 2. THE OBJECT OF INQUIRY

1. Interesting questions can be raised about just what kinds of knowledge are involved in knowledge of language. For some discussion, see chapter 4, pp. 162–6, and references cited there.
2. There is a valuable critical review of available evidence in Fodor, Bever, and Garrett (1974). See also Fodor, Fodor, and Garrett (forthcoming) for a discussion of the possible bearing of such evidence on lexical decomposition.

This is not the place to pursue the matter in detail, but some of the argument in the former book relating to issues discussed here is less than compelling. Thus, the authors consider an approach to $LT(H,L)$ stipulating that the grammar acquired must meet (at least) two conditions: conformity to UG and conformity to a data base consisting of phonetically represented sentences. They argue (i) that the condition is not sufficient and (ii) that a "paradox" prevents enrichment of the data base to sufficiency. But (i), while perhaps true, follows only from unexpressed assumptions about the range of permitted grammars; there is no difficulty in formulating constraints on UG sufficient to rule out the alleged counterexamples cited, and they give no general argument from which (i) follows. As for (ii), there is a problem, but no paradox. Furthermore, the problem arises in exactly the same form for any other theory of language learning. Hence I see no force to their strictures, apart from the observation, which is surely correct, that the theory they discuss is far too weak as it stands.

3. Cf., e.g., Braine (1974). Braine asserts that theories of abstract