THE BEHAVIORAL AND BRAIN SCIENCES (1980), 3, 1-61
Printed in the United States of America

Rules and representations

Noam Chomsky
Department of Linguistics and Philosophy, Massachusetts Institute of Technology, Cambridge, Mass. 02139

Abstract: The book from which these sections are excerpted (N. Chomsky, *Rules and Representations*, Columbia University Press, 1980) is concerned with the prospects for assimilating the study of human intelligence and its products to the natural sciences through the investigation of cognitive structures, understood as systems of rules and representations that can be regarded as "mental organs." These mental structures serve as the vehicles for the exercise of various capacities. They develop in the mind on the basis of an innate endowment that permits the growth of rich and highly articulated structures along an intrinsically determined course under the triggering and partially shaping effect of experience, which fixes parameters in an intricate system of predetermined form. It is argued that the mind is modular in character, with a diversity of cognitive structures, each with its specific properties and principles. Knowledge of language, of the behavior of objects, and much else crucially involves these mental structures, and is thus not characterizable in terms of capacities, dispositions, or practical abilities, nor is it necessarily grounded in experience in the standard sense of this term.

Various types of knowledge and modes of knowledge acquisition are discussed in these terms. Some of the properties of the language faculty are investigated. The basic cognitive relation is "knowing a grammar"; knowledge of language is derivative and, correspondingly, raises further problems. Language as commonly understood is not a unitary phenomenon but involves a number of interacting systems: the "computational" system of grammar, which provides the representations of sound and meaning that permit a rich range of expressive potential, is distinct from a conceptual system with its own properties; knowledge of language must be distinguished from knowledge of how to use a language; and the various systems that enter into the knowledge and use of language must be further analyzed into their specific subcomponents.

Keywords: evolution; grammar; knowledge; language; mental structures

1. Mind and body

In this paper I would like to explore a number of issues relating to human cognitive capacities and the mental structures that serve as the vehicles for the exercise of these capacities. Plainly, this formulation of a problem embodies assumptions that are far from clear and are highly controversial insofar as they are clear. I will try to make them clearer and, I hope, more plausible as I proceed. In the end, the best way to clarify and evaluate these assumptions is to construct specific models guided by them in particular domains, then to ask how these models fare when interpreted as explanatory theories. If the leading ideas are appropriate, they will be sharpened and justified by the success of explanatory theories that develop them in a specific way. I will not attempt a systematic presentation of such a model here, but I will discuss properties of some that are being investigated – though in technical studies they are not presented in these terms, which, I want to suggest, are the appropriate terms. The cognitive domain that will primarily concern me is human language. The reason for the choice is in part personal, relating to limits of my own understanding. I think it is fair to say, however, that the issues are more easily formulated and better understood in connection with human language than other domains of human cognition – which is not to say that they are clearly formulated or well understood. There are some who would virtually identify the study of language and the study of mind (such as Quine, 1975, in terms of dispositions). This is not my own view.

I would like to think of linguistics as that part of psychology that focuses its attention on one specific cognitive domain and one faculty of mind, the language faculty. Psychology, in the sense of this discussion, is concerned, at the very least, with human capacities to act and to interpret experience, and with the mental structures that underlie these capacities and their exercise; and more deeply, with the second-order capacity to construct these mental structures (see Chomsky 1975a for more detail).

The term "capacity" is used with varying degrees of strictness. When I say that a person has the capacity to do so-and-so at a particular time, I mean that, as physically and mentally constituted at that time, he needs no further instruction, learning, training, or physical development, to do so-and-so; if placed under appropriate external conditions, he is able to do it. Thus, a person who does not know how to swim lacks the capacity to swim, in this sense. Similarly, the Olympic swimming champion lacks the capacity to swim if his arms and legs are amputated or broken, but not if he is tied to a chair or asleep or absorbed in a book. Thus, having the capacity to do so-and-so is not the same as knowing how to do so-and-so; in particular, there is a crucial intellectual component in "knowing how" (Chomsky 1975b). We might distinguish further between what one is able to do at will and what falls within one's capacity, though one cannot do it at will. Thus Babe Ruth had the capacity to hit a home run, but not at will, whereas he had the capacity to lift a bat at will (cf. Danto and Morgenbesser 1957).

There is also a second-order sense of "capacity," as when we say that any normal child has the capacity to swim, or to run a mile, or to speak Italian, if only given the appropriate training or opportunities for development. In this sense the child does not have the capacity to fly, and other (terrestrial) organisms do not have the capacity to speak Italian. Sometimes the term is used more loosely, as when we speak of "capacities" in the sense of "mental faculties." The distinctions can be sharpened, but this should be enough for my purposes here.

To begin with, let us assume that it makes sense to say, as we normally do, that each person knows his or her language – that you and I know English, for example – that this knowledge is in part shared among us and represented somehow in our minds, ultimately in our brains, in structures that we can

 0140-525X/80/010001-62$4.00/0

hope to characterize abstractly, and in principle quite concretely, in terms of physical mechanisms. When I use terms such as "mind," "mental representation," "mental computation," and the like, I am keeping to the level of abstract characterization of the properties of certain physical mechanisms, as yet almost entirely unknown. There is no further ontological import to such references to mind or mental representations and acts. In the same way, a theory of human vision might be formulated in concrete terms, referring, say, to specific cells in the visual cortex and their properties; or it might be formulated abstractly in terms of certain modes of representation (say, images or stick-figure sketches), computations on such representations, organizing principles that determine the nature of such representations and rules, and so on. In the latter case the inquiry belongs to the study of mind in the terminology that I will adopt, though it need in no sense imply the existence of entities removed from the physical world.

I am interested, then, in pursuing some aspects of the study of mind – in particular, such aspects as lend themselves to inquiry through the construction of abstract explanatory theories that may involve substantial idealization and will be justified, if at all, by success in providing insight and explanations. From this point of view, substantial coverage of data is not a particularly significant result; it can be attained in many ways, and the result is not very informative as to the correctness of the principles employed. It will be more significant if we show that certain fairly far-reaching principles interact to provide an explanation for crucial facts – the crucial nature of these facts deriving from their relation to proposed explanatory theories. It is a mistake to argue, as many do, that by adopting this point of view one is disregarding data. Data that remain unexplained by some coherent theory will continue to be described in whatever descriptive scheme one chooses, but they will simply not be considered very important for the moment.

From this point of view we can proceed to approach the study of the human mind much in the way that we study the physical structure of the body. In fact, we may think of the study of mental faculties as actually being a study of the body – specifically the brain – conducted at a certain level of abstraction. It may be useful, as a point of departure, to consider for a moment how we do proceed to study the human body.

We assume, no doubt correctly, that the human species is characterized by a certain biological endowment. The embryo grows ultimately to the adult as its genetic program unfolds under the triggering and shaping effect of the environment. These effects are worth distinguishing. Take the standard conditioning paradigm, in which a course of behavior is constructed in a step-by-step process by manipulation of reinforcement contingencies – that is, contingencies that for some reason change the probability of behavior. This is an example of a shaping effect of the environment. Or suppose that there is some domain, however narrow, in which traditional empiricist psychology is valid; say that a child receives simultaneously a visual and an auditory impression and associates them, the residue of the auditory impression serving as the name of the object taken to have caused the visual impression. There are notorious problems in working any of this out – crucially, the problem of how we can have sensory experience uninformed by conceptual judgement (see, for example, Williams 1977; Schopenhauer 1974). But suppose that we put these problems aside. Then the empiricist paradigm can serve as an example of the shaping effect of the environment on knowledge, furthermore a case in which there is some sort of "resemblance" between what is in the mind and what it perceives.

Suppose, in contrast, that certain environmental conditions are required to set in operation an intrinsically determined process, as nutrition is required for cellular growth to take place in predetermined ways. (It has been reported, for example, that handling of rats induces lateralization for spatial and affective processes; Denenberg et al. 1978.) In such cases the processes that take place are not shaped by the environment; they do not reflect the course of interchange with it or somehow "resemble" the stimulus, any more than a child is a reflection of the food he eats. When external conditions are necessary for or facilitate the unfolding of an internally controlled process, we can speak of their "triggering" effect. If institutionalized children do not learn a language, the reason may be that a crucial triggering factor (appropriate social interchange) is lacking, as in the case of Harlow's deprived monkeys [see Rajecki et al.: "Toward a General Theory of Infantile Attachment" *BBS* 1(3) 1978]; but we would not therefore say that attention, care, and love shape the growth of language in the sense that a schedule of reinforcement shapes the behavior of a pigeon. The distinction between the two kinds of effects of the environment is not sharp, but it is conceptually useful. My own suspicion is that a central part of what we call "learning" is actually better understood as the growth of cognitive structures along an internally directed course under the triggering and partially shaping effect of the environment. In the case of human language, there evidently is a shaping effect; people speak different languages, which reflect differences in their verbal environment. But it remains to be seen in what respects the system that develops is actually shaped by experience, rather than reflecting intrinsic processes and structures triggered by experience.

Returning to the analogy to the physical body, we take for granted that the organism does not learn to grow arms or to reach puberty – to mention an example of genetically-determined maturation that takes place long after birth. Rather, these developments are determined by the genetic endowment, though the precise manner in which the genetic plan is realized depends in part on external factors, both triggering and shaping. For example, nutritional level can apparently affect the time of onset of puberty over a considerable range. As the biological plan unfolds, a system of interacting organs and structures matures – the heart, the visual system, and so on, each with its specific structures and functions, interacting in largely predetermined ways.

Our biological endowment determines both the scope and limits of physical growth. On the one hand, it permits the growth of a complex system of highly articulated physical organs, intrinsically determined in their essential properties. Were it not for this highly specific innate endowment, each individual would grow into some kind of an amoeboid creature, merely reflecting external contingencies, one individual quite unlike another, each utterly impoverished and lacking the intricate special structures that make possible a human existence and that differentiate one species from another. Our biological endowment permits a vast potential for development, roughly uniform for the species. At the same time, it in fact narrowly limits what each individual can become; the human embryo presumably cannot become a bird by modification of the external environment. Scope and limits of development are intimately related. Innate factors permit the organism to transcend experience, reaching a high level of complexity that does not reflect the limited and degenerate environment. These very same factors rule out many possible courses of development and limit drastically the final states that can be attained in physical growth.

Now all of this should be transparent and hardly controversial. Apparently very little is known about how any of it happens, but no one really doubts that something of this sort is roughly true. If it were proposed that we "taught" to pass through puberty, or "learn" to have arms rather than wings, no one would take the suggestion very seriously, even in the present state of ignorance concerning the mechanisms involved. Why is this so? Presumably the reason derives from the vast qualitative difference between the impoverished and unstructured environment, on the one hand, and the highly specific and intricate structures that uniformly develop, on the other.

When we turn to the mind and its products, the situation is not qualitatively different from what we find in the case of the body. Here, too, we find structures of considerable intricacy, developing quite uniformly, far transcending the limited environmental factors that trigger and partially shape their growth. Language is a case in point, though not the only one. Think, for example, of the capacity to deal with the number system, common to humans (aside from those with gross pathology) and, as far as we know, unique to humans, surely a major factor in the remarkable success of the "Galilean style" in physics. Russell once wrote that we would not have developed the concept of number had we lived on the sun. Perhaps the opportunity to employ those faculties of mind that present us with a world of individual objects provides a triggering effect for the growth of the "number faculty," but beyond that, it seems reasonable to suppose that this faculty is an intrinsic component of the human mind. One should not be misled by the fact that some birds, for example, can be taught to pick n elements from an array for small n – about up to seven (Koehler 1956). The very essence of the number system is the concept of adding one, indefinitely. The concept of infinity is not just "more" than seven, just as human language, with its discrete infinity of meaningful expressions, is not just "more" than some finite system of symbols that can laboriously be imposed on other organisms (nor, by the same token, just "less" than an essentially continuous system of communication, like the dance of bees – (see *Cognition and consciousness in nonhuman species, BBS* 1(4) 1978). The capacity to deal with the number system or with abstract properties of space is surely unlearned in its essentials. Furthermore, it is not specifically "selected" through evolution, one must assume – even the existence of the number faculty could not have been known, or the capacity exercised, until human evolution had essentially reached its current stage.

We may usefully think of the language faculty, the number faculty, and others as "mental organs," analogous to the heart or the visual system or the system of motor coordination and planning. There appears to be no clear demarcation line between physical organs, perceptual and motor systems, and cognitive faculties in the respects in question. In short, there seems little reason to insist that the brain is unique in the biological world, in that it is unstructured and undifferentiated, developing on the basis of uniform principles of growth or learning – say those of some learning theory, or of some yet-to-be conceived general-purpose learning strategy – that are common to all domains.

David Hubel, who has pioneered some of the most exciting work of recent years on the physical basis for mammalian vision, concludes that

"... we are led to expect that each region of the central nervous system has its own special problems that require different solutions. In vision we are concerned with contours and directions and depth. With the auditory system, on the other hand, we can anticipate a galaxy of problems relating to temporal interactions of sounds of different frequencies, and it is difficult to imagine that the same neural apparatus deals with all of these phenomena ... for the major aspects of the brain's operation no master solution is likely (Hubel 1978)."

There may well be properties common to diverse systems. For example, experience is necessary for "fine tuning" of the visual and auditory systems, as it is for other systems that develop in accordance with fixed genetic instructions. Recent work on motor coordination in monkeys seems to show "that many motor programs are part of a primate's genetic endowment. No sensory feedback or spinal reflex loops are necessary for learning the repertoire of movements ... though ... sensory feedback is necessary for 'fine tuning' ...." (Taub 1976). [See also Roland: "Sensory Feedback to the Cerebral Cortex During Voluntary Movement in Man" *BBS* 1(1) 1978.] Perceptual and motor systems are doubtless partly "set" by the shaping effect of other aspects of the environment (Blakemore 1973), but the systems that emerge seem to be highly specialized and intrinsically programmed in quite different ways. In short, what is taken for granted without direct evidence in the case of physical growth on the basis of an implicit argument from poverty of the stimulus is also being found in the study of the brain and nervous system – not surprisingly, one would think.

In the case of cognitive faculties, it is widely assumed that development is uniform across domains, and that the intrinsic properties of the initial state are homogeneous and undifferentiated – an assumption found across a spectrum of opinion reaching from Skinner to Piaget (who differ on much else) and common in contemporary philosophy as well. Notice that there are two issues here: the issue of innate structure, and that of modularity. One might hold that there is rich innate structure but little or no modularity. But there is a relation between the views, in part conceptual. Insofar as there is little in the way of innate structure, what develops in the mind of an individual will be a homogeneous system derived by the application to experience of common principles that constitute the innate endowment. Such differentiation as there may be will reflect differentiation in the environment. Correspondingly, the belief that various systems of mind are organized along quite different principles leads to the natural conclusion that these systems are intrinsically determined, not simply the result of common mechanisms of learning and growth. It is not surprising, then, to find that opinions "cluster." Those who tend toward the assumption of modularity tend also to assume rich innate structure, while those who assume general multipurpose learning mechanisms tend to deny modularity.

Once we begin to take seriously the actual states attained in particular cases, we are, I believe, led to the conclusion that intrinsic structure is rich (by the argument from poverty of the stimulus) and diverse (by virtue of the apparent diversity in fundamental principles of capacities and mental structures attained). These conclusions are, I think, to be expected in the case of systems that have any significant function in the life of an organism. As noted, they are taken for granted without much thought or evidence in the study of physical development; no one doubts that the instructions for a liver and visual system will be quite different. Insofar as anything is known about cognitive systems – which is not very far – the related assumptions of poverty of initial structure and homogeneity do not seem tenable, and the general line of argument that keeps analogous assumptions from being considered at all in the case of physical growth seems applicable. The more we learn about specific systems, that more applicable it becomes, and I would hazard a guess that this will continue to be so. In the case of human conceptual systems, for example, intrinsic even to such apparently elementary notions as "thing" or "object," there is a subtle interaction between conditions of spatiotemporal contiguity, the willed acts of an agent responsible for the object, and other factors (see Chomsky 1975a). It is difficult to interpret this except in terms of our intrinsic modes of cognition. When we turn to language, many examples have been studied of shared knowledge that appears to have no shaping stimulation – knowledge without grounds, from another point of view – and that seems to be based on principles with only the most superficial resemblances to those operative in other cognitive domains.

Let me give some simple examples, in part to illustrate the point, and in part for later reference. Consider, for example, the process of forming questions. We select some noun phrase in a sentence, replace it by an appropriate questionword, place the latter at the beginning of the sentence, and with other mechanical operations, form a question. Thus, on the model of the sentence, "John saw a man," we can form "Whom did John see?" Or, to take a more complex case, on the model of the sentence, "The teacher thought that his assistant had told the class to study the lesson," we can question "the class" and ask: "Which class did the teacher think that his assistant had told to study the lesson?" But consider the following example, of roughly comparable complexity: "The lesson was harder than the teacher had told the class that it would be." Here, if we question "the class," we derive: "Which class was the lesson harder than the teacher had told that it would be?" Evidently, this is not a well-formed question, though its intended sense is clear enough and perfectly intelligible, with a little thought. It is difficult to imagine that people capable of these judgments have all had relevant training or experience to block the obvious inductive generalization. Rather, it seems that some specific property of the human language faculty – hence a general property of language – leads to these consequences, a property that derives from our modes of cognition.

To take a second case, consider the rule forming reciprocal expressions such as, "The men saw each other." A child learning English, or someone learning English as a second language, must learn that "each other" is a reciprocal expression – that is, an idiosyncratic fact about English. Given that it is a reciprocal expression, it must have an antecedent: for example, "the men" in "The men saw each other," which has the meaning, roughly: "Each of the men saw the other." The antecedent can be in a different clause, as in: "The candidates wanted [each other to win]," where "each other" appears in a subordinate clause as the subject of "win," whereas its antecedent, "the candidates," appears in the main clause. Sometimes, however, the reciprocal cannot find its antecedent outside of its clause, as in: "The candidates wanted me to vote for each other," which is not well-formed with the perfectly sensible meaning: "Each of the candidates wanted me to vote for the other." One might assume that the antecedent must be the "nearest noun phrase," but this is false, as we can see from such sentences as, "The candidates hurled insults at each other." While this sentence could mean that the candidates hurled each insult at the other insults, plainly that is not the way we normally interpret it.

In this case, too, it can hardly be maintained that children learning English receive specific instruction about these matters, or even that they are provided with relevant experience that informs them that they should not make the obvious inductive generalization, say, that "each other" takes some plural antecedent that precedes it. Children make many errors in language learning, but they do not assume, until corrected, that "The candidates wanted me to vote for each other" is a well-formed sentence meaning that each candidate wanted me to vote for the other. Relevant experience is never presented for most speakers of English, just as no pedagogic or traditional grammar, however compendious, would point out these facts. Somehow, this is information that the children themselves bring to the process of language acquisition as part of their mode of cognition.

Some general principle of language applies to permit the proper choice of antecedent – not an entirely trivial matter, as these examples suggest. Similarly, some general principle of language determines which phrases can be questioned. These principles, which have many ramifications, are among those that provide a basic framework within which knowledge of language develops as the child progresses to the mature state of knowledge; they are on a par with the factors that determine that the child will have binocular vision. As we consider such principles and their interaction, we begin to approach the richness of the language faculty – one element of our biological endowment, and, it appears, a distinctive element.

It would be surprising indeed if we were to find that the principles governing these phenomena are operative in other cognitive systems, although there may be certain loose analogies, perhaps in terms of figure and ground, or properties of memory, as we see when the relevant principles are made explicit. Such examples illustrate two points, in the present connection: first, that there is good reason to suppose that the functioning of the language faculty is guided by special principles specific to this domain; second, that the argument from poverty of the stimulus provides a useful device for inquiry into these principles – indeed, at the moment, the most useful device, I think, for inquiring into universal grammar.

It seems reasonable to assume that the language faculty – and, I would guess, other mental organs – develops in the individual along an intrinsically determined course under the triggering effect of appropriate social interaction and partially shaped by the environment – English is not Japanese, just as the distribution of horizontal and vertical receptors in the visual cortex can be modified by early visual experience. The environment provides the information that questions are formed by movement of a question word and that "each other" is a reciprocal expression; in other languages this is not the case, so that these cannot be properties of biological endowment in specific detail. Beyond such information, much of our knowledge reflects our modes of cognition and is therefore not limited to inductive generalization from experience, let alone any training that we may have received. And just as the visual system of a cat, though modified by experience, will never be that of a bee or a frog, so the human language faculty will develop only one of the human languages, a narrowly constrained set.

A familiar argument against a modular approach to the study of mind is that it "reduces the possibility of viewing language as an aspect of the total corpus of behavior" and "obscures the connections between language and other aspects of cognition" (Hill and Most 1978, pp. 651–2). By parity of argument, we should conclude that the belief that the eye and the ear work on different principles reduces the possibility of viewing vision as an aspect of behavior and obscures the relations between vision and hearing. It is a sad commentary on the field that such arguments can even be advanced.

Consider again the question whether cognitive functions are both diverse and determined in considerable detail by a rich innate endowment. If the answer is positive, for some organism, that organism is fortunate indeed. It can then live in a rich and complex world of understanding shared with others similarly endowed, extending far beyond limited and varying experience. Were it not for this endowment, individuals would grow into mental amoeboids, unlike one another, each merely reflecting the limited and impoverished environment in which he or she develops, lacking entirely the finely articulated and refined cognitive organs that make possible the rich and creative mental life that is characteristic of all individuals not seriously impaired by individual or social pathology – though, once again, we must bear in mind that the very same intrinsic factors that permit these achievements also impose severe limits on the states that can be attained; to put it differently, that there is an inseparable connection between the scope and limits of human knowledge.

Finally, let me emphasize the limits on the enterprise I have been outlining. Two individuals with the same genetic endowment and common experience will attain the same state – specifically, the same state of knowledge of language (random elements aside). But this does not preclude the possibility of diversity in the exercise of this knowledge, in thought or action. The study of acquisition of knowledge or of interpretation of experience through the use of acquired knowledge still leaves open the question of the causation of behavior and, more broadly, our ability to choose and decide what we will do.

2. Structures, capacities, and conventions

I began the first section by speaking of human cognitive capacities and the mental structures that serve as their vehicles; I then went on to consider the legitimacy of studying such mental structures in the manner of the natural sciences and the prospects for this study; I ended by expressing some skepticism as to whether the mind is as uniform and undifferentiated as it is regarded by many modern psychologists and philosophers. Numerous questions arise at every point along the way. The questions of initial structure and modularity are relatively straightforward empirical ones, if we accept the presuppositions they embody: Is the mind organized into distinct cognitive faculties with their specific structures and principles, or are there uniform principles of learning, accommodation, assimilation, abstraction, induction, strategy, or whatever, that simply apply to different stimulus materials to provide our knowledge of the behavior of objects in physical space, our knowledge that certain strings of words do or do not have certain meanings, and so on. It is reasonably clear how to proceed to settle these issues. As I have already indicated, the available evidence seems to me to favor a modular approach. The other questions, while they have an empirical component, are nevertheless of a very different order.

To know a language, I am assuming, is to be in a certain mental state, which persists as a relatively steady component of transitory mental states. What kind of mental state? I assume further that to be in such a mental state is to have a certain mental structure consisting of a system of rules and principles that generate and relate mental representations of various types. Alternatively, one might attempt to characterize knowledge of language – perhaps knowledge more generally – as a capacity or ability to do something, as a system of dispositions of some kind, in which case it is perhaps not unreasonable to think of behavior as providing a criterion for the possession of knowledge. In contrast, if such knowledge is characterized in terms of mental state and structure, then behavior simply provides evidence for possession of knowledge, as might facts of an entirely different order – electrical activity of the brain, for example.

The choice between these alternatives cannot be settled by *a priori* argument, but only by trying to refine each of them to the point where we can ask how they fare as theories that explain some significant range of facts – for example, that certain sentences do or do not mean such-and-such and that ✓ we know this to be the case. For the moment, at least, there is no substantive issue. There has been a fair amount of work sketching theories of rules and representations that have at least a degree of descriptive and explanatory success. The proposal that particular items of our knowledge, such as those given for illustration in the preceding section, can be explained on the assumption that "the speaker has a number of abilities" or "has acquired a number of psychological dispositions" (Kitcher 1978) remains merely a promissory note. I won't attempt to explore the possibilities of fulfilling the promise. I suspect that the attraction will dim when it is recognized how little can be said in terms of "sets of dispositions." Generally the dispositional analysis is put forth on the grounds that the facts do not compel us to adopt the alternative, which is true but hardly relevant. Surely it suffices that the alternative stands as the "best explanation," if that much is correct. In the absence of a coherent alternative, and with at least partial successes to show from the study of theories of rules and representations, I will continue to assume that it is correct to analyze knowledge of language, and to offer explanations for particular instances, in terms of mental structures of rules and representations – to assume, in short, that our linguistic abilities are based on such mental structures.

The issue of structure is not a straightforward empirical one, but it does have an empirical component. In particular, success in developing a structural theory of mind, knowledge, and belief would count against the picture of cognition in terms of capacities without structured vehicles, and would indicate that the prevailing concern with organization of and potential for behavior misconceives a certain category of evidence as criterial.

If, as I am now assuming, to know a language is to be in a certain mental state comprised of a structure of rules and principles (comparably, for certain other aspects of cognition), then in theory one could know a language without having the capacity to use it. Let's begin by considering this issue.

Imagine a person who knows English and suffers cerebral damage that does not affect the language centers at all but prevents their use in speech, comprehension, or, let us suppose, even thought. Suppose that the effects of the injury recede, and, with no further experience or exposure, the person recovers the original capacity to use the language. In the intervening period, he had no capacity to speak or understand English, even in thought, though the mental (ultimately physical) structures that underlie that capacity were undamaged. Did the person know English during the intervening period?

This is reminiscent of the question whether a person who is in a dreamless sleep can properly be said to know English, or to know his name. The cognitive property that concerns me holds of the person who possesses the mental structure, thus of the aphasic throughout, as we learn from the fact of his recovery. In this case the fact of his recovery provides evidence that he had knowledge of English, though none of his behavior (even his thought) at the time provided any evidence for possession of this knowledge.

Suppose that there is a second aphasic like the first, but because of some other and irrelevant problem (say, a circulatory disorder) he never recovers speech. Should we say in this case that the knowledge of English was lost? That would seem perverse. The first aphasic recovered because he had retained a certain mental (ultimately physical) state, a certain state of knowledge – namely, knowledge of English. His recovery provides evidence for the fact. One can imagine all sorts of evidence that might indicate that the aphasic who did not recover was in exactly the same (relevant) state – say, electrical activity of the brain or evidence from autopsy. The conclusion that the second aphasic retained his knowledge of English would have to be based on evidence, of course, but not necessarily evidence from behavior. To deny that the aphasic who did not recover had knowledge of his language would seem as odd a move as to deny that the one who did recover knew his language when he was unable to use this knowledge.

Both of these aphasics remained in a certain cognitive state throughout. "Knowing English" seems to me the appropriate term for designating this state. Were we to identify capacity and knowledge, we would presumably be led to saying that the aphasic does not know English when the capacity is lacking, and we would hence be committed to the belief that full knowledge of English can arise in a mind totally lacking this knowledge without any relevant experience whatsoever, as the case of recovery shows. This is plainly not true of the child's mind and seems an exotic claim. I do not want to argue terminology, but I will use the term "knows English" with reference to the person with the appropriate mental structure, quite apart from his capacity to use the internally represented knowledge (even in thought) or even to gain access to it. In these terms, which do not seem to me to strain normal usage, two individuals might share exactly the same knowledge (e.g., of English, of music, of calculus, of geography, etc.) but differ greatly in their capacity to use it, which also seems a commonplace; and a person might increase his capacities while gaining no new knowledge. In the same usage, behavior is no criterion for knowledge, though it provides evidence for possession of knowledge, along with much else, in principle. Similar remarks apply in the case of "knowing how," which also involves a crucial intellectual component, often ignored.

Consider now a different case: a child learning English. Suppose that the child is at the stage in which he produces so-called "telegraphic speech" – that is, a series of content words without grammatical elements. Imagine the following (not entirely hypothetical) sequence of events. At one point the child produces only telegraphic speech. Shortly after, he makes correct use of grammatical particles such as "do" and, let us say, the whole auxiliary system of English, and does so across the board – that is, in questions, assertions, negations, and so on. At the earlier stage, the child did not have the capacity to use such items (so his behavior indicates). Did he have the knowledge of the appropriate rules and forms? In the framework that I am suggesting, the answer might be that he did. That is, it might be that he had fully internalized the requisite mental structure but for some reason lacked the capacity to use it; perhaps because he spoke through a filter that passed only content words, perhaps because of limits on memory. There is evidence in other domains that changes in memory or attention can lead to what appears to be change in the stage of cognitive development (Bryant 1974). I am assuming this to be a question of fact, for which we might find varied sorts of evidence. One kind of evidence that the child did have the knowledge, though without any capacity to use it, would be that the whole system appeared, full-blown, in all constructions. This could be explained on the assumption that the knowledge was already internalized, though not exhibited, because of a filtering effect: a conclusion that might be supported, for example, if we found evidence for the lifting of the memory restriction in other domains. Or, experiment might show that at the earlier stage, the child understood normal speech better than speech with noncontent words randomly interspersed – a finding that would again provide evidence that he possessed the knowledge. Or, in principle, more exotic means might be devised: say, study of electrical activity of the brain. We cannot enumerate the kinds of evidence that might bear on the truth of the hypothesis that the child had the knowledge, any more than we can in the case of investigation of some other complex system, the internal elements and working of which we are trying to determine. But I see no reason to deny that there is a fact of the matter, however difficult it may be to establish, or that behavior is only one kind of evidence – sometimes not the best, and surely no criterion for knowledge.

Suppose that, in contrast to the above sketch, our tests indicated that the child did not have knowledge of the full system of relevant rules when he was in the stage of telegraphic speech. We might then propose a very different account of his state of knowledge at the time. Here is one possibility, again not entirely fanciful. Suppose that what we call "knowing a language" is not a unitary phenomenon, but must be resolved into several interacting but distinct components. One involves the "computational" aspects of language – that is, the rules that form syntactic constructions or phonological or semantic patterns of varied sorts, and that provide the rich expressive power of human language. A second component involves the system of object-reference and also such relations as "agent," "goal," "instrument," and the like – what are sometimes called "thematic relations" or (misleadingly) "case relations." For want of a better term, let us call the latter a "conceptual system." We might discover that the computational aspect of language and the conceptual system are quite differently represented in the mind and brain, and perhaps that the latter should not, strictly speaking, be assigned to the language faculty at all, but rather should be considered as part of some other faculty that provides "common-sense understanding" of the world in which we live. Involved in this system might be what Moravcsik (1975, 1977), in a series of very interesting papers, has called the "aitiational" structure of our concepts – that is, their analysis, more or less along Aristotelian lines, in terms of such "generative factors" as origin, function, material constitution, and the like – notions that have reentered recent discussion in the misleading framework of "essences of things" and "identity across possible worlds" (See Chomsky 1975a, pp. 46f.) Supposing all of this, let us distinguish a system of "computational" rules and representations that constitute the language faculty, strictly speaking, and a system of conceptual structure organized along the lines just indicated. The two systems interact. Thus certain expressions of the linguistic system are linked to elements of the conceptual system, and perhaps rules of the linguistic system refer to thematic relations. But it nevertheless might be correct, in a fuller theory of the mind, to distinguish these systems much as we distinguish the visual and circulatory system, though of course they interact. The conceptual system, for example, might have a central role in all sorts of mental acts and processes in which language plays no significant part; it might have a different physical basis and different evolutionary history, and so on.

Tentatively assuming such a framework as this, let us return to the child in the telegraphic speech stage. Suppose that his conceptual system in the sense of the preceding remarks has partly matured, but his linguistic system has not, apart perhaps from peripheral components that provide sounds and words. The transition from the telegraphic stage to the later stage might be behaviorally identical to the first case we considered, but markedly different in actual character. In the first case a peripheral change, say in memory, led to the capacity to use an already represented system of knowledge; in the second case the system of knowledge changed from one state to a different state. Again, the two distinct processes could in principle be distinguished by evidence of various sorts, but the child's behavior occupies no privileged place and may in fact tell us little or nothing.

Pursuing the matter further, consider again the child in the second example, with a partially-developed conceptual system and a minimally-functioning language system – which we might think of on the analogy of incipient fluttering motions of a bird before the system of flight has matured. The child might be able to make sense of much of the adult speech around him, as we can often make out what is said in a foreign language when we can identify some of the words, impose a thematic and aitiational structure, and use contextual cues, even without much knowledge of the grammar. Actually, normal comprehension under noisy conditions in our own language is in some ways a similar task. But the child's success would not lie in his possession of knowledge of the language that he hears, apart from peripheral aspects.

To evaluate a picture such as this, we might, again, turn to evidence of various sorts. For example, there might be clinical evidence. It seems that patients whose left hemispheres have been surgically removed in infancy do not, as had been thought previously, develop fully normal language. They show surprising abnormalities in handling very simple structures that involve a degree of computational facility such as simple passive sentences; general understanding in normal life, however, is so good that these abnormalities may go unnoticed (Dennis and Whitaker 1976; Dennis 1978). Conceivably, further research may show that while the conceptual system is intact, certain elements of the language system are not, and that language use, while superficially normal, involves rather different mechanisms.

Or, to mention another possible example, consider a recent study of a child, deprived of language experience until age 13, who showed a degree of apparent language development in subsequent years under therapy and training, with a fair degree of comprehension attained (Curtiss 1977). There is some reason to believe that her knowledge of language does not involve the normal computational system of language [see Arbib and Caplan: "Neurolinguistics Must be Computational" *BBS* 2(3) 1979] but may rather involve the use of a conceptual system of the type just outlined – a system quite distinct from language (though it interacts with it) and perhaps in a sense more "primitive." One might speculate that higher apes, which apparently lack the capacity to develop even the rudiments of the computational structure of human language, may nevertheless command parts of the conceptual structure just discussed and thus be capable of elementary forms of symbolic function or symbolic communication (see Savage-Rumbaugh et al. 1978; and "Cognition and Consciousness in Nonhuman Species" *BBS* 1(4) 1978; Limber 1977; Terrace, forthcoming; Chomsky, forthcoming) while entirely lacking the human language faculty. Possible support for such a view derives from work indicating that humans with severe language deficit – perhaps literal destruction of the language faculty – can acquire systems similar to those that have been taught to apes (see Velletri-Glass et al. 1973; Gardner et al. 1976; Davis and Gardner 1976), as if, to put it very loosely, apes were in this regard like humans without the language faculty.

These speculations are directed to two points. First, the approach to the study of mind and language that I have been outlining leaves open a variety of avenues of inquiry into the nature and organization of mental structures and the ways in which they develop. While evidence is meager and research is barely beginning, there is enough promise, I think, to suggest that the approach is well worth pursuing, quite apart from what has been achieved within the narrower frame of linguistic inquiry. Second, we should not exclude the possibility that what we normally think of as knowledge of language might consist of quite disparate cognitive systems that interweave in normal cognitive development. The system of conceptual structures that involves object-reference with all of its subtleties and complexities, thematic structures, aitiational factors, and the like, might be distinct from the language faculty, though related to it. The latter possibility relates in obvious ways to recent debate in the philosophical literature about the theory of meaning and belief systems – in part an outgrowth of Quine's important and influential critique of empiricist semantics, and in part influenced by Wittgenstein's insights into use of language against the background of belief, intent, and so on, among other sources.

How these issues will be resolved, we can only guess. My own guess is that something along the lines I have just described may be correct. Let us tentatively assume so and continue to inquire into the unitary or modular character of the faculty of language itself. It makes sense, I think, to analyze the mental state of knowing a language into further components – in particular, to distinguish what is sometimes called "grammatical competence" from "pragmatic competence." The term "competence" entered the technical literature in an effort to avoid entanglement with the slew of problems relating to "knowledge," but it is misleading in that it suggests "ability" – an association that I would like to sever, for reasons just discussed. By "grammatical competence" I mean the cognitive state that encompasses all those aspects of form and meaning and their relation, including underlying structures that enter into that relation, which are properly assigned to the specific subsystem of the human mind that relates representations of form and meaning. A bit misleadingly, perhaps, I will continue to call this subsystem "the language faculty." Pragmatic competence underlies the ability to use such knowledge along with the conceptual system to achieve certain ends or purposes. It might be that pragmatic competence is characterized by a certain system of constitutive rules represented in the mind, as has been suggested in a number of studies.

Again, there are empirical assumptions embedded in the conceptual distinction. For example, I assume that it is possible in principle for a person to have full grammatical competence and no pragmatic competence, hence no ability to use a language appropriately, though its syntax and semantics are intact. To adopt an analogy of Kasher's (1977), this would be something like the case of a policeman who knows the syntax of traffic signals (red and green lights and their sequence, etc.) and their semantics (red means *stop*, etc.) but lacks the knowledge of how to use them to direct traffic. There have, in fact, been some clinical descriptions of language disability that might reflect such a situation in part at least (e.g. Blank et al. 1978). The assumptions involved are by no means innocent. They bear directly on questions about the "essence of language" that have figured significantly in contemporary philosophy (see Chomsky 1975a, Chapter 2).

Keeping to grammatical competence, what is to be included under aspects of form and meaning properly assigned to the language faculty? If one rejects the modular approach outlined earlier, the question is meaningless, or rather, the decision is arbitrary, since there is no language faculty and the mental state of knowing a language is some artibrarily-selected subpart of one's total mental state. By "arbitrary" I do not mean "selected on no basis at all," but rather selected on no basis having to do with the structure of the mind. But if one believes the modular approach to have merit, as I do, the question is a reasonable one. It is on a par with the question: What is the human visual system, or the heart, or the circulatory system? Such systems are not physically isolable. As in the case of a "mental organ," which I am taking to be an integrated system of rules and principles, generating representations of various sorts, the question is one of appropriate idealization at a certain level of theoretical discussion – a question with empirical content no doubt, but one that can be fully resolved only in the context of broader study of a system that incorporates the given idealized "organ" as a part. It seems to me that no problem of principle arises in the case of the language faculty that does not arise in the case of the visual system or some other system conventionally isolated for special study. We abstract away from connections that obviously exist, hoping to be able ultimately to reconstruct a full picture of the structure and functioning of the total system – recognizing, at the same time, that even the "total system," in this case the individual organism, is itself an idealization reflecting a particular way of looking at things and processes in the world, which does not come ontologically prepackaged as a set of individuals with properties (essential or other) apart from our mode of conception.

In the case of the language faculty, there is a fair consensus on some of the elements that should be incorporated within it, along with considerable dispute about others – in particular, those on what might be called "the periphery," to use a metaphor that I hope will not be too misleading. It is conventional, at least since Aristotle, to think of language as form with a meaning. If so, among the representations provided by the system of grammatical competence – henceforth, the grammar – will be representations of form and representations of meaning. These representations are "peripheral" in the sense that they may be regarded as the point of contact between the language faculty and other systems of the mind – again not a necessary assumption, but a reasonable one I think. We may think of these representations as the ones that enter directly into the use of language. What are the elements of these representations? What is their character? On the modular assumptions outlined, these are obscure empirical questions, to be clarified and answered by empirical inquiry.

To what extent, for example, does the organization of sound properly belong to the system of language rather than to other systems? Here there are real and significant empirical questions concerning perceptual categorization and its possible special relation to language (Mehler and Bertoncini 1979). Studying the interaction between the perceptual system and the system of language, with particular attention to possible specialization for language, we can hope to refine our understanding of the representation of form provided by grammar, and thus of the rules that enter into determining this representation.

Still more debated, perhaps more intractable problems arise in considering representations of meaning. Do the "semantic rules" of natural language that are alleged to give the meanings of words belong to the language faculty strictly speaking, or should they be regarded perhaps as centrally-embedded parts of a conceptual or belief system, or do they subdivide in some way? Much of the debate about these matters has been inconclusive. It turns on dubious intuitions as to whether we would still call our pets "cats" if we learned that they are robots controlled by Martians, or whether we would call the stuff in the Ganges "water" were we to discover that it differs in chemical composition from what flows in the Mississippi [see Fodor, this volume]. It is harder, I think, to make up a story compatible with the assumption that I persuaded you to leave but at no point did you intend to leave; quite generally, arguments for analytic connections of some sort involving properties of the verbal system, with its network of thematic relations, are more compelling than those devised for common nouns. Even assuming there to be certain analytic connections, as I believe to be the case, the question remains whether these are to be assigned to the language faculty, hence to appear in some form in its representations of meaning, or to a conceptual system of a sort perhaps organized along the lines already discussed. Similarly, suppose that our notion of a "thing" involves considerations of spatio-temporal contiguity as well as human action, as discussed in references noted earlier (Chomsky 1975a, pp. 44 f.; 203). If so, have we isolated a "semantic universal" or a fact about our conceptual systems? Similar questions arise in connection with more complex semantic properties – for example, certain possible universals relating to abstract and concrete interpretation of common nouns (Chomsky 1976; Jackendoff, forthcoming).

Or consider another question that has been much debated. Certain discourses are appropriate in a sense in which others are not. For example, if I say "It was *Dickens* who wrote "Moby Dick," – italics indicate heavy stress – you can appropriately respond: "No, it was *Melville*, but not: "No, it was *David Copperfield*." If you were to give the latter response, I would understand you to be asserting that David Copperfield wrote "Moby Dick." The point can be expressed in terms of focus and presupposition, in one of the senses of these terms, and relates to the position of main stress. Is this a matter of semantics or pragmatics? Is it to receive expression in terms of the representations provided by the grammar or not?

Or, to take a last case, suppose that we agree that some sort of representation of quantificational structure is to be given by the grammar at the level of representation of meaning. Does the notation matter? Should the representation involve quantifiers or variables, or should it be in a variable-free notation, or is the question without empirical import?

There are innumerable questions of this sort. How we proceed to deal with them depends on the point of view we adopt towards what we are doing. Suppose, for example, that someone rejects the approach outlined here or is just not interested in it and is pursuing some other aim – let us say, to codify logical inference using natural language sentences (e.g. Katz 1972). Then all properties entering into such inference, and only these, will appear in his "representations of meaning." For example, suppose it can be shown that pragmatic rather than logical presupposition is involved in the case of "It is Dickens who wrote Moby Dick." Then the relevant properties of this sentence will not be encoded in "representations of meaning" for the study of logical entailment, no matter what the character of the rules that govern the phenomenon may be. Suppose it turns out that presupposition, in the sense required to account for these cases, is governed by rules that fall within grammar in a narrow sense – perhaps rules that enter into determining representations involved in logical inference in other cases. Then the person who is concerned to map out the properties of the language faculty will include these properties in his "representations of meaning," whatever the status of the presupposition. There has been much empty discussion of these questions – though perhaps deeper issues are masked in it (for example, questions of psychological realism and modularity).

If one adopts the point of view that I have been discussing here, then it is clear how to proceed to answer these questions, at least in principle. We will try to construct coherent and integrated systems with explanatory power and see where the examples fall. If the idealization to the language faculty and its subcomponents is legitimate – that is, if it corresponds to some significant aspect of reality – we may be able to find an answer to these questions. My impression is that at the moment we often cannot; the available evidence is inadequate and the theories not sound enough to bear the weight. In some cases I think there are possibilities for an answer. For example, in the matter of stress and presupposition there is reason to believe that the rules fall within grammatical competence, so that the properties appear in the representations of meaning it provides. Were it to be shown that these matters do not bear on logical inference but only, say, on conversational implicature, we would then conclude that representations of meaning generated by the rules of grammar provide materials for conversational implicature, not that they must exclude these elements. And if some attribute of a sentence that enters into logical inference turns out not to be provided by the best theory of grammar that we can devise, we will conclude that this is not an element of the representations of meaning provided by grammatical competence. Proceeding in this way, we will try to identify just what it is that we have loosely been calling "representations of meaning," much in the way that we will try to determine the properties of linguistic representations of sound. The fact that the conclusions may not conform to some *a priori* scheme or satisfy some specific need such as codifying inference is, plainly, irrelevant to this empirical inquiry. I am assuming, in short, that we are trying to answer a difficult empirical question, only partially clear, which can become more precise only in the course of finding some answers to it: namely, what are the real components of mental states?

Taking a grammar to be a system of rules that provides representations of sound and meaning (among others), their specific character to be determined as research progresses, our task is to discover the representations that appear and the rules operating on them and relating them; and more important, to discover the system of universal grammar that provides the basis on which they develop. One may think of the genotype as a function that maps a course of experience into the phenotype. In these terms, universal grammar is an element of the genotype that maps a course of experience into a particular grammar that constitutes the system of mature knowledge of a language – a relatively steady state achieved at a certain point in normal life.

Thinking about the question we now face in rather general and qualitative terms, we can make a fair guess as to what we should discover, if this inquiry succeeds. An investigation of the final states attained – that is, the grammars – reveals that the knowledge acquired and to a large extent shared involves judgments of extraordinary delicacy and detail. The argument from poverty of the stimulus leaves us no reasonable alternative but to suppose that these properties are somehow determined in universal grammar, as part of the genotype. There is simply no evidence available to the language learner to fix them, in many crucial cases that have been studied. Nevertheless, people obviously speak different languages depending on their limited individual experience.

As translators are well aware, there need be no point-by-point comparison between languages, and linguistic work reveals considerable variety. Given these properties of language, what we should expect to discover is a system of universal grammar with highly restrictive principles that narrowly constrain the category of attainable grammars, but with parameters that remain open to be fixed by experience. If the system of universal grammar is sufficiently rich, then limited evidence will suffice for the development of rich and complex systems in the mind, and a small change in parameters may lead to what appears to be a radical change in the resulting system. What we should be seeking, then, is a system of unifying principles that is fairly rich in deductive structure, but with parameters to be fixed by experience. Endowed with this system and exposed to limited experience, the mind develops a grammar that consists of a rich and highly articulated system of rules, not grounded in experience in the sense of inductive justification, but only in that experience has fixed the parameters of a complex schematism with a number of options. The resulting systems, then, may vastly transcend experience in their specific properties but yet be radically different from one another, at least on superficial examination; and they may not be comparable point-by-point in general.

Keeping to the rather vague level of these qualitative remarks (which I try to make a bit more precise elsewhere; see Chomsky 1980, Chapter 4), the problem of accounting for the growth of different languages, each of which lacks an inductive grounding in experience, is not unlike the general problem of growth or, for that matter, speciation. It appears that the biochemistry of life is rather similar across all living organisms and that, as Francois Jacob (1978) puts it:

> "... it was not biochemical innovation that caused diversification of organisms.... What accounts for the difference between a butterfly and a lion, a chicken and a fly, or a worm and a whale is not their chemical components, but varying distributions of these components ... specialization and diversification called only for different utilization of the same structural information.... It is thanks to complex regulatory circuits, which either unleash or restrain the various biochemical activities of the organism, that the genetic program is implemented. [In related organisms, mammals for example], the diversification and specialization ... are the result of mutations which altered the organism's regulatory circuits more than its chemical structures. The minor modification of redistributing the structures in time and space is enough to profoundly change the shape, performance, and behavior of the final product." (see also Jacob 1977)

The logic is rather similar to what I have outlined in the case of acquisition of knowledge of language. In a system that is sufficiently intricate in structure, small changes in particular points can lead to substantial differences in outcome. In the case of growth of organs, mental organs in our case, small changes in parameters left open in the general schematism can lead to what appear to be very different systems.

I think we may now be at the stage where, for the first time really, we can propose systems of universal grammar that at least have the right properties. I have no doubt that they are incorrect, at least in detail, perhaps in general conception. But they do have the right properties, and that seems to me of some importance. That is, the systems that are now being investigated by a number of linguists do have a deductive structure that permits a range of empirical phenomena to be derived from some simple and, I think, rather natural principles, and they also have the property that small changes in the parameters in some of the general principles lead to quite different languages. These are the kinds of systems we hope to find, whether or not the systems of universal grammar currently being investigated will prove to be on the right track.

I have been speaking of "knowing English" as a mental state (or a stable component of mental states), or a property of a person in a certain mental state, but we may want to analyze this property in relational terms. What is it that is known? Ordinary usage would say: a language – and I have so far been keeping to this usage, speaking of knowing and learning a *language* – e.g. English. But it is implicit in what I have said that this way of talking can be misleading. I think it has been, and I would now like to explain why. To avoid terminological confusion, let me introduce a technical term devised for the purpose – namely "cognize" – with the following properties. The particular things we know we also cognize. In the case of English, we know that "The candidates wanted each other to win" means that each wanted the other to win, and that "The candidates wanted me to vote for each other" is not well-formed, with the meaning that each wanted me to vote for the other. We therefore cognize these facts. Furthermore, we cognize the system of mentally-represented rules from which the facts follow. That is, we cognize the grammar that constitutes the current state of our language faculty and the rules of this system as well as the principles that govern their operation. And finally, we cognize the innate schematism, along with its rules, principles, and conditions.

In fact, I don't think that "cognize" is very far from "know," where the latter term is moderately clear, but this seems to me a relatively minor issue, similar to the question whether the terms "force" and "mass" in physics depart from their conventional sense. If the person who cognized the grammar and its rules could miraculously become conscious of them, we would not hesitate to say that he knows the grammar and its rules, and that this conscious knowledge is what constitutes his knowledge of his language. Thus "cognizing" is tacit or implicit knowledge, a concept that seems to me unobjectionable. Putting aside the question of "innate knowledge" (see Chomsky 1980, Chapter 3), we note that cognizing has the structure and character of knowledge but may be, and in the interesting cases is, inaccessible to consciousness. Concluding this terminological digression, I will return to the terms "know" and "knowledge," but I will now use them in the sense of "cognize" – that is, admit both conscious and tacit knowledge – and hope that possible confusion will have been allayed. The fundamental cognitive relation is knowing a grammar: knowing the language determined by it is derivative. Correspondingly, it raises additional problems (see Chomsky 1980, Chapter 3).

I see no reasonable alternative to the position that grammars are internally represented in the mind, and that the basic reason why knowledge of language comes to be shared in a suitably idealized population (and partially shared in actual populations) is that its members share a rich initial state, hence develop similar steady states of knowledge. There are, obviously, many reasons for skepticism about specific proposals that are put forth in the effort to characterize these grammars and the universal grammar from which they allegedly develop, and about the various idealizations that enter into them. In the next section I want to consider some further questions about knowledge of grammar.

3. Knowledge of grammar

In the preceding sections I have been discussing elements of a theory of human knowledge that is concerned with such traditional questions as "What is human knowledge?", "How does it arise in the individual?", "In what respects is it shared?". Comparable questions arise concerning other cognitive systems – for example, systems of individual and shared belief. The discussion has been "mentalistic," but, I think, in an innocuous sense. It takes no stand on issues of materialism (in fact, I have expressed some skepticism about the content of these issues, given that "body" is an open and evolving concept), but rather it proceeds as an inquiry at a certain level of abstraction into the nature of certain mechanisms, presumably neural mechanisms, now largely unknown. The level of abstraction is appropriate insofar as it enables fundamental properties of these systems to be isolated, analyzed, and explained; and insofar as results obtained at this level provide a guide for the study of mechanisms, much as study of chemical properties provides a guide for inquiry into atomic theory or a theory of feature detectors in the auditory system might lay a basis for research into the neurophysiology of hearing. I am tentatively assuming the mind to be modular in structure – a system of interacting subsystems that have their own special properties. What little is known supports this view. The only reasonable research strategy, so far as I can see, is to study particular systems and their interaction. If the modular approach is incorrect, such study will reveal, contrary to what I expect, that these systems involve the same principles and develop in the same way from a common basis. I have been attempting to isolate for special study one of these systems: the faculty of human language. I have suggested that what we loosely call "knowledge of language" involves, in the first place, knowledge of grammar – indeed, that language is a derivative and perhaps not very interesting concept – and beyond that, other cognitive systems that interact with grammar: conceptual systems with their specific properties and organizing principles may be quite different in character from the "computational" language faculty; pragmatic competence might be a distinct cognitive system with a different structure from that of grammatical competence; these systems may be further composed of distinct though interacting components. If so, we should not expect a unitary answer to the question "What is knowledge of human language and how does it arise?" Rather, we will find that the question was wrongly put; it does not identify a natural kind. If this turns out to be correct, it should occasion no surprise. There is little reason to suppose that ordinary common-sense notions will prove any more appropriate for the theory of knowledge than they are for the study of the physical world (or I should say, other aspects of the physical world).

I am assuming grammatical competence to be a system of rules that generate and relate certain mental representations, including, in particular, representations of form and meaning, the exact character of which is to be discovered, though a fair amount is known about them. These rules, furthermore, operate in accordance with certain general principles. I have informally discussed rules of grammar that, for example, move a question-word to the front of a sentence or associate an antecedent with an anaphoric expression such as "each other." Such rules can be formulated quite precisely within an explicit theory of rules and representations (along lines discussed elsewhere). I offered a few linguistic examples to illustrate certain principles that govern the application of these rules. I will simply give names to these principles; further characterization appears elsewhere (Chomsky 1980, Chapter 4): the movement rule is governed by a principle of "locality" – elements of mental representation can't be moved "too far" – and the choice of antecedent is governed by a principle of "opacity" – variable-like elements can't be free in certain opaque domains, in a sense specific to the language faculty.

Certain factors that govern or enter into the adult system of rules, representations, and principles belong to universal grammar; that is, they are somehow represented in the genotype, along with the instructions that determine that we will grow arms instead of wings, or have a mammalian eye instead of an insect eye. Among the elements of the genotype, I am tentatively assuming, are the principles of locality and opacity, the option of moving question-words and relating a variable-like expression such as *each other* (an anaphor) to an antecedent and, in general, certain basic properties of the mental representations and the rule systems that generate and relate them. These will become empirical hypotheses, once they are stated in more explicit form.

One basic element in what is loosely called "knowledge of a language," then, is knowledge of grammar, now analyzed in terms of a certain structure of rules, principles, and representations in the mind. This grammar generates paired representations of sound and meaning, along with much else. It thus provides at least partial knowledge of sound-meaning relations. A fuller account of knowledge of language will consider the interactions of grammar and other systems – specifically, the system of conceptual structures and pragmatic competence – and perhaps others (for example, systems of knowledge and belief that enter into what we might call "common sense understanding" of the world). These systems and their interactions also arise from a primitive basis – part of an innate endowment that defines "the human essence."

Keeping to knowledge of grammar, I have also been speaking of knowledge of the rules and principles of grammar, and also knowledge of the innate elements that enter into this mature knowledge. It is, I feel, a relatively uninteresting question whether "knowledge" in the sense of this discussion is the same concept as that expressed by the English word "knowledge." If one disagrees, he can replace "know" wherever I am using it by the technical term "cognize," which has just the properties I am assigning to it. In fact, it is not at all clear that the ordinary concept of "knowledge" is even coherent, nor would it be particularly important if it were shown not to be.

Against an approach to the study of knowledge of the sort that I have been describing, it is commonly argued that it would not permit us to distinguish properly between, say, knowing a language and knowing how to ride a bicycle [see Premack: "Does the Chimpanzee Have a Theory of Mind" *BBS* 1(4) 1978]. Thus, in the latter case, too, one might say that the bicycle rider "cognizes" both "the rules of riding he can articulate – push with the feet on the pedals – and those that he cannot, even though his practice is in accord with them – e.g. lean into a curve. But nothing much of interest may turn out to have been attributed when we attribute to him the [cognizing] of a rule of riding." (Donnellan 1977, p. 720) I have discussed similar objections elsewhere, explaining why I think they are misguided (Chomsky 1975a, pp. 222–3; 1972, p. 87) Let's consider Donnellan's specific case. True, there would be little point to a concept of "cognizing" that did not distinguish "cognizing the rules of grammar" from the bicycle rider's knowing that he should push the pedals or lean into a curve, given what we assume to be the facts of the matter. But it seems easy enough to make the relevant distinction. In the case of riding a bicycle, there is no reason to suppose that the rules in question are represented in a cognitive structure of anything like the sort I have described. Rather, we take bicycle riding to be a skill, whereas knowledge of language and its concomitants – for example, knowledge that reciprocal expressions have the properties I mentioned – is not a skill at all. The skill in question might, perhaps, be based on certain reflex systems, in which case it would be incorrect to attribute a cognitive structure in the sense of this discussion to the person who exercises the skill. In contrast to the case of language, nothing seems to be explained by attributing to the bicycle rider a cognitive structure incorporating the rules with which his practice accords. But suppose we are wrong, and in fact the bicycle rider does have a representation of certain physical principles in his mind and uses them to plan or compute his next act. In this case we *should* attribute to him a cognitive structure, and in fact, it would be quite appropriate to say that he cognizes these principles as he does the rules of his language. The question, I take it, is basically one of fact.

To help clarify the issue, consider two missile systems, each of which is designed to send a rocket to the moon. One of them operates along lines once proposed by B. F. Skinner; it has several pigeons looking at a screen that depicts what lies directly ahead, trained to peck when the rocket veers off course, their pecking restoring the image of the moon to a focused position on the screen. Consider, in contrast, a system that incorporates an explicit theory of the motions of the heavenly bodies and information about its initial position and velocity and that carries out measurements and computations using its internalized theory to adjust its course as it proceeds. This rocket might hit the very same spot as the servomechanism with the pigeons, but it would do so in a very different way. Mere investigation of behavior might tell us little, perhaps nothing. A deeper look might be required to distinguish the two systems. In the second case, but not the first, inquiry might lead us to attribute to the missile something like a "mental state" [see Griffin: "Prospects for a Cognitive Ethology" *BBS* 1(4) 1978]. That is, it might lead us to formulate an abstract characterization of perhaps unknown mechanisms, postulating a system that involves the cognizing of certain principles and representations. In the first case, such an account would be factually wrong. I think that the two cases fall on opposite sides of an important divide, and that the second – the cognizing missile – shares properties with human knowledge. It also lacks crucial properties; for example, it is a task-oriented device, whereas knowledge of language, for example, is not.

No doubt one can construct other cases that are not so readily distinguished. This would be a valuable exercise, which might contribute to an understanding of just what distinguishes knowledge (cognizing), in the sense of this discussion, from skill and ability. But the many examples of the sort just mentioned that appear in the literature do not seem to me very helpful in this regard because they are too easily handled. Nor do they suggest any problem in the course I have been pursuing, so far as I can see.

A recurrent theme throughout this discussion has been the question whether it is legitimate to adopt the standard "realist" assumptions of the natural sciences in studying language – and cognition more generally. I have been assuming that the questions I have been raising are (rather obscure) questions of fact. Thus, it is a question of fact whether knowledge of grammar is represented in the mind along the lines I have been suggesting, or in other ways, or not at all; or whether such knowledge arises from some sort of learning or differential response to stimuli or in some other way, perhaps through the unfolding of a fairly detailed genetic program under the triggering and partially shaping effect of experience; or whether my knowledge that the typewriter before me will not suddenly fly away is grounded in experience with similar objects or derives in significant part from a conceptual system that has its roots in human biological endowment and is only modified and sharpened in certain ways by experience – clearly a very different picture, which is susceptible to considerable further refinement. I am taking these to be empirical questions, so that we cannot specify in advance what categories of evidence may be relevant to advancing our understanding of them or settling them. Is it correct to regard these questions as empirical?

In the natural sciences, when a theory is devised in some idealized domain, we ask whether the theory is true and try to answer the question in various ways. Of course, we expect that the theory is probably false, and even if on the road to truth, that it does no more than describe at some appropriate level of abstraction properties alleged to be true of whatever the real elements of the world may be when considered at this level of description; it has, in short, some of the properties of so-called "functionalist" theories in psychology. There is a familiar morass of problems about just what is meant when we take a theory to be true: What is the status of its theoretical entities, its principles, its idealizations? How have the facts been recast through the medium of the experimental situation or our perceptual and cognitive faculties? I am not now concerned with these questions but rather with some special and additional ones that are held to arise, beyond these, when we turn to psychology.

Presumably these problems arise somewhere on the boundary between physiology and psychology. Consider again the study of vision. Suppose that some series of experiments leads to the conclusion that particular cells are sensitive to lines with certain orientations. In this case no special problems are held to arise, though of course the conclusion is underdetermined by evidence, the cell is abstracted from its environment, nothing is said (at this level) about the mechanisms that might be responsible for what the cell is alleged to be doing, the results are obtained under highly idealized conditions built into the experimental situation and apparatus, and so on. Suppose next that it is proposed that identification of objects involves analysis into stick-figures or geometrical structures, though nothing is said or known about neural mechanisms that might carry out such analysis. Is the situation fundamentally different in some way, apart from the fact that the theory abstracts still further from the physical and chemical properties of the brain? It is not clear why one should assume so. That is, in this case there seems no reason to refrain from taking the theory to be true, then seeking evidence to verify, disconfirm, or sharpen it, proceeding to search for mechanisms with the properties postulated, and so on.

Let us turn now to the problem that seems to many people to be more disturbing. Consider the elements postulated to account for the facts used for illustration earlier: the rule of movement, the principles of opacity and locality, the representations postulated, and so on. Suppose that all of this can be made as precise as one pleases and that the system meets some very high level of success in explaining such facts as those mentioned over a substantial range. Suppose further that by assuming elements of this system to be innate we can go on to explain how it is that children presented with some information – say, that "each other" is a reciprocal expression rather than the name of a tree – reach the conclusions they do about such facts as those mentioned. Are we permitted to regard the theories of particular and universal grammar so constructed as true, respectively, of the steady state attained in language acquisition and of the initial state common to the species, so that we then proceed to test, refine, and extend them, search for mechanisms with the properties they codify, and so on? It is this step that gives rise to various qualms and objections. It is commonly felt to be unwarranted, requiring different sorts of evidence, or even to be without sense (as advocates of "indeterminacy" would maintain). The question is, what boundary have we crossed that requires us to abandon normal scientific practice, with all of its familiar pitfalls and obscurities?

What is commonly said is that theories of grammar or universal grammar, whatever their merits, have not been shown to have a mysterious property called "psychological reality," What is this property? Presumably, it is to be understood on the model of "physical reality." But in the natural sciences one is not accustomed to ask whether the best theory we can devise in some idealized domain has the property of "physical reality," apart from the context of metaphysics and epistemology, which I have here put aside, since I am interested in some new and special problem that is held to arise in the domain of psychology. The question is: what is "psychological reality," as distinct from "truth, in a certain domain"?

As has been evident throughout, I am not convinced that there is any such distinction. I see no reason not to take our theories tentatively to be true at the level of description at which we are working, then proceeding to refine and evaluate them and to relate them to other levels of description, hoping ultimately to find neural and biochemical systems with the properties expressed in these theories. Perhaps we can gain some insight into the question, if there is one, by asking how it arose.

The first discussion of "psychological reality" that I know of in this connection was in the classic paper of Edward Sapir's (1933) on "the psychological reality of the phoneme." We can reconstruct Sapir's argument – unfairly to him, though in accord with subsequent interpretation – as proceeding in essentials as follows. Considering first what is often called "linguistic evidence," Sapir arrived at a phonological analysis for a certain language: an abstract system of rules and underlying representations that offered a plausible account of the linguistic data. The phonological analysis was not empirically vacuous over the domain of "linguistic evidence." It had some predictive power in the language for which it was offered (e.g., with regard to previously unanalyzed forms) and also had indirect empirical content in that the principles on which it was based could be tested for validity in other languages, or in study of language change, and so on. In our terms, his principles of phonological analysis can be regarded as elements of universal grammar, and one should then ask whether they yield, in each language, the best account of phonetic organization for this language, with the proper predictive consequences, the most far-reaching generalizations, and so forth. So far, what Sapir was doing was standard linguistics, though unusually well-conceived.

But he then proceeded to raise a new question: do the phonemes he postulated have "psychological reality"? To answer this question he turned to other kinds of data (what is sometimes called "psychological evidence") – that is, perceptual tests of various kinds that we need not go into here. The outcome of these tests convinced him that his theoretical constructions had "psychological reality."

Sapir was sharply criticized in subsequent years (e.g. by Twaddell 1935) for venturing to claim that his constructions had "psychological reality" instead of putting them forth merely as fictions convenient for some purpose. But another question arises: Why didn't the "linguistic evidence" suffice to establish "psychological reality?" Perhaps the answer is that it was too weak; after all, phonology is a finite system with limited predictive content. But that does not seem to be the right answer. In fact, in this case the "linguistic evidence" may well be more persuasive than Sapir's "psychological evidence." Furthermore, it is clear from the ensuing debate up to the present that no matter how powerful the "linguistic evidence" might have been, it would not have sufficed to establish "psychological reality." Some new *category* of evidence is required, and this, however weak and inconclusive, could support a claim to "psychological reality."

In short, the evidence available in principle falls into two epistemological categories: some is labeled "evidence for psychological reality," and some merely counts as evidence for a good theory. Surely this position makes absolutely no sense, but it remains implicit in discussion of the matter by psychologists and linguists to the present (see, for example, Chomsky 1980, Chapter 5). I suspect that something of the sort also may lie behind the wariness about inner mechanisms or "the psychological form in which competence exists" expressed by many philosophers concerned with language and mind – for example, those discussed in Chomsky 1980, Chapters 2 and 3.

What we should say, in all these cases, is that any theory of language, grammar, or whatever, carries a truth claim if it is serious – though the supporting argument is, and must be, inconclusive. We will always search for more evidence and for deeper understanding of given evidence, which also may lead to change of theory. What the best evidence is depends on the state of the field. The best evidence may be provided by as yet unexplained facts drawn from the language being studied, or from similar facts about other languages, or from psycholinguistic experiment, or from clinical studies of language disability, or from neurology, or from innumerable other sources. We should always be on the lookout for new kinds of evidence, and we cannot know in advance what they will be. But there is no distinction of epistemological category. In each case we have evidence – good or bad, convincing or not – as to the truth of the theories we are constructing; or, if one prefers, as to their "psychological reality," though this term is best abandoned, as seriously misleading.

There are two final points on which I would like to comment briefly in connection with the notion "knowledge of grammar." The first has to do with consciousness, the second with learning.

As I am using the term, knowledge may be unconscious and not accessible to consciousness. It may be "implicit" or "tacit." No amount of introspection could tell us that we know, or cognize, or use certain rules or principles of grammar, or that use of language involves mental representations formed by these rules and principles. We have no privileged access to such rules and representations.

I have already indicated why I think it is reasonable to suppose that the rules of grammar are mentally represented and used in thought and behavior, much as in the case of the "cognizing missile" discussed earlier (but also with crucial differences). Furthermore, this approach to knowledge and understanding has not simply been proposed as a possibility, but explored in some detail with, I think, a considerable measure of success. To the best of my knowledge it is unique in this respect. I know of no other account that even attempts to deal with the fact that our judgments and behavior accord with and are in part explained by certain rule systems (or to be more accurate, are explained in part by theories that attribute mental representations of rule systems). Unless some principled objection to this approach can be discovered, to reject it out of hand in the absence of any coherent alternative is simply a variety of dogmatism, deserving of no comment. The critic's task is to show some fundamental flaw in principle or defect in execution, or to provide a different and preferable account of how it is that what speakers do is in accordance with certain rules, or is described by these rules – an account that does not attribute to them mental representation of a system of rules (rules which in fact appear to be beyond the level of consciousness). If someone can offer such an account of how it is that we know what we do know, say, about reciprocals, or judge as we do judge, and so on, there will be something to discuss. Since no such account has been forthcoming, even in the most primitive or rudimentary form, there really is nothing to discuss.

The issue is not simply whether we should use the word "knowledge" in connection with knowing rules. The issue is whether this is a permissible approach to developing a theory that will account for parts of what everyone agrees to be knowledge, or, if one wants to drop the term, that will account for what is taken to be in the domain of knowledge, thought, language, action, and behavior.

Searle (1976) remarks correctly that speakers of English do not recognize the rules of grammar as being those they follow, and also that the rules proposed are "abstract and complicated" and "are a long way from having the intuitive plausibility that ordinary grammar-book rules have." Let us consider the merit of these objections – they are offered as objections – to an approach of the sort I have been outlining.

Since the rules are not recognized by speakers as being those they follow, and in fact appear to be inaccessible to consciousness or introspections, Searle concludes that "for the most part the rules remain mere hypotheses." With the conclusion I of course agree, apart from the words "mere" and "for the most part." What he should have said, simply, is that "the rules are hypotheses," or that theories attributing mental representations of these rules offer hypotheses. That is to say, the theories in question have empirical content, not a criticism, in my book at least. The words "mere" and "for the most part" suggest that Searle has seriously misunderstood the work on which he is commenting, apparently taking it to be an attempt at some sort of conceptual analysis.

There is a further problem. Note that Searle argues that "for the most part the rules remain mere hypotheses" on the grounds that speakers do not recognize them as being rules they follow. As already noted, this is no argument at all. But suppose that someone were able to construct an alternative theory involving rules accessible to consciousness that had something like the coverage or explanatory force of theories involving inaccessible rules. That would certainly be an interesting result, though it obviously would not move us beyond the domain of "mere hypotheses" – that is, beyond the domain of statements with empirical content. A person's judgments about what he does have no uniquely privileged status; they simply constitute evidence to be set alongside other evidence. But again, the question is academic. So far as I know, explanatory principles with any merit bearing on the domain of facts of the sort that I have been considering are, in general, inaccessible to consciousness, and there is no reason to expect otherwise. The doctrine of accessibility in any of its traditional or contemporary forms seems to me entirely without antecedent plausibility, and without empirical support.

As for the fact that the rules do not have the intuitive plausibility of those of ordinary grammar books, that is true and surely to be expected, for reasons that have been discussed many times. Ordinary grammar books are concerned with a domain of facts that is virtually complementary to what is most significant for the project I have been discussing. That is, ordinary grammar books, quite properly for their purposes, tacitly assume a principled grammar (generally without awareness) and deal with idiosyncracies, with the kinds of things that could not be known without experience or instruction. For example, no grammar book devised, say, for teaching English, would, or should, deal with the simple properties of questions or of reciprocals that I have mentioned. Rather, it should deal with basic facts of word order and inflection, or with the fact that "each other" is a reciprocal in contemporary English as distinct from languages that do not formally distinguish reflexive and reciprocal, and so on. Such comments on the language no doubt have great intuitive plausibility, but there is surely no reason to expect that this will remain true when we consider such principles as locality or opacity in an attempt to explain facts many of which any ordinary grammar book simply disregards, as it should, insofar as these facts and the principles that underlie them can be assumed to be available to the person using the grammar, once the idiosyncracies are presented.

To conclude these remarks, I would like to turn briefly to the notion of "learning." I have been suggesting that knowledge of grammar, hence of language, develops in the child through the interplay of genetically determined principles and a course of experience. Informally, we speak of this process as "language learning." It makes sense to ask whether we misdescribe the process when we call it "learning." The question merits some thought, I believe. Without attempting to inquire into too many subtleties or to settle the question, I would like to suggest that in certain fundamental respects we do not really learn language; rather, grammar grows in the mind.

When the heart, or the visual system, or other organs of the body develop to their mature form, we speak of growth rather than of learning. Are there fundamental properties distinguishing the development of physical organs and of language that should lead us to distinguish growth, in the one case, from learning, in the other? Perhaps, but it is not obvious. In both cases, it seems, the final structure attained, and its integration into a complex system of organs, is largely predetermined by our genetic program, which provides a highly restrictive schematism that is fleshed-out and articulated through interaction with the environment (embryological or postnatal). There are certain processes that one thinks of in connection with learning: association, induction, conditioning, hypothesis-formation and confirmation, abstraction and generalization, and so on. It is not clear that these processes play a significant role in the acquisition of language. Therefore, if learning is characterized in terms of its distinctive processes, it may well be that language is not learned.

Can we distinguish learning from growth in terms of the nature of the state attained – say, a system of belief or knowledge, with facts or principles stored in the memory accessible to mental computation in the case of learning, or something of this sort? If we do, then it is not clear that any coherent notion of "learning" will remain. It is entirely possible that significant components of such cognitive states are "wired-in," taking their explicit shape in the mind in perhaps something like the way that the distribution of horizontal and vertical receptors in the visual cortex is fixed within certain bounds by the character of presented visual experience. Knowledge of behavior of objects in visual space or of principles and rules of language, and much else, arises in ways that do not seem crucially different from other forms of growth or selection from a set of highly restricted alternatives, so far as we know.

Dennett (1975) has suggested that we think of "learning" as what he calls "self-design." In some cases of transition from state to state, the new design "*exists ready made* in the old design, in the sense that its implementation at this time is already guaranteed by its old design" (presumably, given some triggering event). In other cases "the old design does not determine in this way what the new design will be"; rather, the design process generates alternatives that are tested "against a whole array of requirements and constraints" (Dennett 1975, citing Herbert Simon). If the "whole array of requirements and constraints" is taken to be confirmation by evidence, "simplicity" in some sense relevant to choice among theories, and the like, then self-design (that is, learning) seems pretty much like what Peirce called "abduction" – a process in which the mind forms hypotheses according to some rule and selects among them with reference to evidence and, presumably, other factors. It is convenient sometimes to think of language acquisition in these terms, as if a mind equipped with universal grammar generates alternative grammars that are tested against the data of experience, with the most highly-valued one selected; I have occasionally used this metaphor, but I don't think that it should be taken too seriously. If we take it partially seriously, then under this concept of learning as "abduction" or "self-design," the question whether language is learned or grows will depend on whether the mind equipped with universal grammar presents a set of grammars as hypotheses to be selected on the basis of data and an evaluation metric, or whether the steady-state grammar arises in another way – for example, by virtue of a hierarchy of accessibility (stated, perhaps, in terms of the very same evaluation metric) and a process of selection of the most accessible grammar compatible with given data. The distinction between such alternatives lies so far beyond conceivable research that the question whether knowledge is the result of learning or growth is hardly worth considering, if learning is characterized in these terms. It is rather doubtful, in fact, that there is much in the natural world that falls under "learning," so conceived, if the metaphors are taken seriously.

There is an interesting discussion of related questions by the immunologist Jerne (1967). He distinguishes between instructive and selective theories in biology, where an instructive theory holds that a signal from the outside imparts its character to the system that receives it, and a selective theory holds that change of the system takes place when some already-present character is identified and amplified by the intruding stimulus. He argues that "Looking back into the history of biology, it appears that wherever a phenomenon resembles learning, an instructive theory was first proposed to account for the underlying mechanisms. In every case this was later replaced by a selective theory." The primary example that he deals with is the development of antibodies by the immune system. This was first assumed to be a kind of learning process in which the antigen played an instructive role, the reason being that the number of antigens was so immense, including even artificially synthesized substances that had never existed in the world, that no other account seemed conceivable. But this theory has been abandoned. An animal "cannot be stimulated to make specific antibodies, unless it has already made antibodies of this specificity before the antigen arrives," so that antibody formation is a selective process in which the antigen plays a selective and amplifying role.

As another example, Jerne cites the familiar Darwinian account of a factory well covered with brown moths, later found to be covered with white moths after it is painted white. Darwinian theory offers a selective account based on the fact that the moths of lighter color were already present before the new signal arrived. "In this case, the signal that entered into the system, that is, the color change, was not even received by the moths, but by the birds" that fed on them.

After reviewing a number of such examples, Jerne turns to some speculations on the central nervous system. He notes certain analogies to the immune system. Both systems develop through experience, with change of state induced by outside elements. Both appear to be learning in response to these external events, and the changes are not transmitted to the offspring. He suggests that learning from experience may be "based on a diversity in certain parts of the DNA, or to plasticity of its translation into protein, which then controls the effective synaptic network underlying the learning process." If such speculations are valid, "It thus remains to be asked if learning by the central nervous system might not also be a selective process; i.e., perhaps learning is not learning either." The air of paradox in this last remark can be dissipated if we think of the term "learning" as a rigid designator, perhaps commonly misapplied; or, to take a standard example, as analogous to such terms as "witch," commonly applied at one time but always misapplied.

Jerne suggests, finally, a distinction between learning and selection in terms of level of analysis, a distinction that would explain why accounts in terms of learning come to be replaced by accounts in terms of selection as inquiry proceeds. Keeping to the interaction of the whole system and an external signal, we see what appears to be an "instructive process"; the system changes and the change is caused by the stimulus. Thus in the case of the moths, one might say that the signal of painting the wall white " 'instructed' the population of moths to mimic the color change," even though the moths never received the signal. But processes that are "instructive" at the system level, in this sense, "imply selective mechanisms, through which products that were already present in the system prior to the arrival of the signal are selected and amplified." So when we analyze an "instructive" process, we find that learning is not learning either.

I don't think that the notion of selection from preexisting materials is rich enough to provide an analysis for the large-scale interactions that are loosely called "learning," but it may be a step along the way. It is possible that the notion "learning" may go the way of the rising and setting of the sun.

Outside of cognitive capacities that are part of our intrinsic nature, modification and fixing of belief may proceed by trial-and-error, conditioning, abstraction, induction, and the like; that is, in domains in which the mind is equipped with no special structure to deal with properties of the task situation (See Chomsky 1975a, Chapters 1 and 4; 1980, Chapter 6). Learning theory will then reduce to the study of tasks and problems for which we have no special design, which are at the borders of cognitive capacities or beyond, and are thus guaranteed to tell us very little about the nature of the organism. This is not, of course, to demean the content of what is learned. What is significant for human life is not necessarily significant for the person inquiring into human nature. In the case of "language learning," mechanisms of association (etc.) may be involved in the acquisition of idiosyncracies (e.g., specific inflectional patterns and choice of vocabulary items), though even here it is highly likely that powerful intrinsic constraints guide the course of development. For example, the rate of vocabulary acquisition is so high at certain stages of life, and the precision and delicacy of the concepts acquired so remarkable, that it seems necessary to conclude that in some manner the conceptual system with which lexical items are connected is already substantially in place.

From the point of view of the study of human nature, the most interesting aspects of learning (as distinct from growth of knowledge and belief) may well turn out to be those to which Dennett's remarks direct our attention: essentially, Peircean abduction. In some domains – acquisition of language, object perception, and so on – the growth of our knowledge just happens to us, in effect. The mental faculty grows from its initial to its steady state without choice, though not necessarily without effort or willed action [see Gyr et al.: "Motor-Sensory Feedback and Geometry of Visual Space" *BBS* 2(1) 1979]. In other domains – the natural sciences, for example – the growth of knowledge involves deliberate inquiry involving hypothesis formation and confirmation, guided no doubt by "abductive" constraints on potential hypotheses as well as other equally obscure factors that enter into choice of idealization and the like. The basic elements of rational inquiry may have some of the properties of such cognitive systems as the language faculty, though the ways in which they are employed are surely quite different: scientific knowledge does not grow in the mind of someone placed in an appropriate environment. The study of human knowledge should, it seems, consider a number of rather different types of systems: the growth of "natural" faculties such as those that provide common-sense understanding of the physical and social world or language (Moravcsik 1979); learning by association, conditioning, induction, and the like on the periphery of fixed cognitive capacities; deliberate inquiry employing "abductive" constraints on intelligible hypotheses and other elements of so-called "scientific method." In each of these domains, elements of our knowledge appear to be innate, and still other elements, ungrounded, in any reasonable sense of the term.

If we hope to assimilate the study of human intelligence and its products to the natural sciences insofar as possible and to learn something about human nature in this way, we would do well, I think, to devote particular attention to those domains in which rich and complex mental structures arise under minimal exposure to the environment, language being a striking example, though for reasons discussed elsewhere (Chomsky 1980) no more "typical" than others.

As I have tried to show here and elsewhere (Chomsky 1980) it makes sense to think of other cognitive systems on the model of the human language faculty, despite the fact that knowledge of language is not a "central case" of knowledge and, on the assumption of modularity, cannot be expected to involve principles that enter into other cognitive systems. My own feeling is that a theory of mind should proceed by tentatively identifying such cognitive systems and submitting them to detailed study to determine their specific properties. Some such systems may be like language in that they are properly conceived in terms of grammar-like theories of rules and representations. It may turn out that, in the domains where we speak of "knowledge of *X*" (knowledge of language, of music, of mathematics, of the behavior of objects, of social structure, of human characteristics, etc.), with the consequences of such knowledge in the form of expectations or knowing that such-and-such, there is a mentally-represented system of this nature that can be taken to be an object of knowledge, just as there is good reason, I believe, to think of what we know as a grammar when we speak loosely of "knowing a language." These cognitive systems serve as vehicles for the exercise of our various capacities and thus enter into our thought and action, as when we come to understand what is said to us, or what is happening around us, or what some other person is doing – say, reading a book or pursuing a goal (see also Chomsky 1969; and Premack and Woodruff 1978a, b). To identify these cognitive systems and to discover their properties and modes of interaction, we must be willing to entertain fairly far-reaching idealization and to attribute internal structure, sometimes in the form of rules and representations, to the human mind, including substantial innate structure, which might take various forms: principles, rules, systems of representation, schemata, modes of functioning and integration, and so on. We must, in short, be willing to approach the study of mind much in the manner of the natural sciences.

In the book from which this paper is abstracted I have been trying to make two basic points. First, a variety of objections of principle that have been raised to such an approach are not well-founded, while alternative approaches to similar problems that seek to avoid attribution of internal mental structures as vehicles for the exercise of cognitive capacities are fundamentally flawed. And second, in some domains of human knowledge it is possible to obtain nontrivial results by studying the rules and representations of cognitive systems. A framework of the sort I have been discussing seems to me natural and appropriate for the study of such products of the human mind as human language, or the study of the use of language. I would like to end by speculating that if the so-called "cognitive sciences" do develop in a serious and fruitful way, it will be within a framework of essentially this character.

Open Peer Commentary

Commentaries submitted by the qualified professional readership of this journal will be considered for publication in a later issue as Continuing Commentary on this article.

by **József Andor**

Medical University of Pécs, Foreign Language Department, Pécs, Hungary

Some remarks on the notion of competence

In my view, Chomsky's analysis of the mental state of knowing a language in terms of different components (i.e., a "grammatical" and a "pragmatic" component) is a bit too rigid and unnatural. I think it inadvisable to accept the view according to which one can have full grammatical competence without a functioning pragmatic competence. My guess is that much of what was considered to belong to the domain of performance in terms of Chomsky's Standard Theory (or the later Extended Standard Theory) has now been incorporated into the "pragmatic competence" notion outlined here. I do not see, however, whether such a division of competence has any empirical support or psychological validity (a topic Chomsky seems to address for the first time since the development of generative models of natural languages). In Chomsky's view, "grammatical competence" pertains to aspects of form and meaning — that is, the rules of syntactic and semantic structures. He has nothing to say, however, about the precise nature of the mental organization of these domains.

In my view, the above domains cannot be represented in a single component, since their mental organization is not uniform. Moreover, it is incorrect to separate them rigidly from pragmatic factors in the way that Chomsky does in his target article. I assume that "linguistic competence" refers to a set of cognitive domains or macrocomponents in the mind that are of heterogeneous nature. These mental macrocomponents are composed of cognitively-based chains of knowledge structures developed on the basis of our empirical knowledge of the world.

I assume that the constructs and categories of recent "frame semantics" and "cognitive semantics" models play a critical role in the description of our linguistic competence. In the cognizing process we store knowledge in terms of sets of scenes and frames (van Dijk 1977; Fillmore 1975; Minsky 1977), and the activation of such knowledge involves bringing certain frames to the fore by putting them into perspective (Fillmore 1977). I accordingly take it that our competence is mainly of a semantic nature, but that the semantic chains stored are closely related to empirical experience and knowledge and are thus strongly influenced by sociological, cultural, as well as psychological factors – the former two are greatly overlooked by Chomsky in the present paper.

The way we make assertions about our environment by linguistic means, and our evaluation of our own experiences and the experiences and assertions of others, depend greatly on the limitations of our knowledge structures, the level of our factual knowledge, our cognitive capacity, and our sociocultural background. Hence, we do not express (or need to express) everything in our memory during the communicative interaction (e.g., in conversation), since we possess a set of mentally-conditioned filters that help us decide what to bring into perspective in given situations under certain conditions, and what to leave verbally unexpressed. The same set of cognitive filters acts upon our judgment of the coherence relations of textual units.

Of course, it would require further research to investigate to what extent such filters are influenced by our knowledge of the complex system of syntactic operations of a given language, or by our naive conception of that system, or whether the filters themselves are dependent on semantic or pragmatic factors (only). What is crucial here is that we certainly possess such a set of stored filters, as their operation can be traced and analysed in an account of the success or failure of surfacing procedures and criteria in the individual evaluation and judgment of the acceptability of verbalised grammatical strings. (Preliminary data on such research can be found in Andor 1978; Kintsch 1977; Kintsch and van Dijk 1978; Schlesinger 1977; Schlesinger 1979.) Everyone has his own cognitive filters, since the development of knowledge structures based on scenes and frames is highly individual in nature, depending upon age and the above-mentioned factors.

Thus, I would rather propose that Chomsky's pragmatic competence be integrated with semantic component of competence; and I would definitely separate the latter from the (cognitively based?) level of the ability to master syntactic rules, both mentally and at the surface. I do not think that our knowledge of grammar can be described in the sense Chomsky assumes. It is true that our knowledge of rules has a great influence on the way we think (hence it is often the case that we think in terms of sets of rules without ever "surfacing" our ideas verbally), but still, I consider our semantic and pragmatic knowledge (and intuitions) to have a more critical role in comprehension than syntactic factors. This is the point where the relevance of semantic and pragmatic factors as well as evidence from syntax should be tested psychologically, and their role in a systematic approach to a typological analysis of context (a still imprecisely defined domain) requires further research.

by **Richard F. Cromer**

MRC Developmental Psychology Unit, London WC1H OAN, England

Empirical evidence in support of non-empiricist theories of mind

It is incumbent on those who make a case for an unpopular position within a particular theoretical climate to present more convincing arguments and a greater amount of evidence than would be required merely to challenge relatively minor problems in current theory. Otherwise, there is the danger that the unpopular position will be ignored or dismissed for reasons not entirely relevant to the real issues involved. The theoretical orientation for which Chomsky argues in his target article is of some interest, and psychologists' growing awareness of the role of "growth" as opposed to exclusive interest in learning is important, but Chomsky does himself a disservice by stating too much of his case in the form of assertions frequently not backed up with supporting evidence and therefore open to criticism.

It should be stated at the outset that there is a good deal of empirical evidence to support Chomsky's claims, but he fails to cite it. Indeed, some recent data from early child language studies (and from studies of cognitive growth in general) that have been overlooked reveal precisely the kind of evidence that should give most psychologists pause when they are tempted to reject theories of growth and innate mechanisms outright. Thus, rather than to criticize the form of the particular assertions that Chomsky makes, it would seem of interest to call attention to some of the evidence that could be seen as supporting his overall position.

Perhaps the best illustration of how some of Chomsky's arguments have the unfortunate effect of weakening his claims occurs with what he calls "the argument from the poverty of the stimulus." This argument is used to make strong claims for the richness of internal structure. However, there are various possible interpretations of "poverty of the stimulus." It would appear that what Chomsky means by the poverty of the stimulus is that many aspects of the rule system that the child eventually "possesses" (or that describes adult language knowledge) simply do not appear in the input data, however well-formed, short, simple, and "grammatical."

Most psychologists and psycholinguists have taken "poverty of the stimulus" in a rather different sense, based on claims that Chomsky had made earlier (Chomsky 1967) about the input stimulus being composed mainly of pauses, hesitations, and false starts. They argue that data from mother-child interaction studies demonstrate that the input to the child is not a degenerate stimulus of this type. It is made up of short, well-formed structures and constitutes an ideal stimulus for inducing grammatical regularities (see, e.g., various contributions to Snow and Ferguson 1977). They conclude that since Chomsky's arguments for innateness are primarily based on the poverty of the input stimulus (in this sense of ill-formed utterances), his position has accordingly been falsified. This conclusion, however, ignores important aspects of the child's productions. These include some of the findings from the early work in the 1960's, which formed part of the basic argument against simple associationistic theories of language acquisition.

To state the earlier work in the form relevant to the present argument: the child may well be receiving clear input stimuli consisting of grammatically well-formed sentences, but what he produces diverges drastically and systematically from that input. His utterances are not merely "telegraphic," as was first thought. Rather, his productions evidence rules of formation that differ from the rules descriptive of the input stimulus. The child hears clear and well-formed negatives, but his production of negatives can be described in terms of a series of stages (Bellugi 1967) the earliest of which is the placement of a negative morpheme like "no" or "not" in front of affirmative utterances, thus producing "no wipe finger," "not fit," and the like – forms not present in the short, clear, reduced input. In spite of being exposed to well-formed questions, the child forms his interrogatives, for example, without auxiliary inversion. In spite of exposure to clear examples of pronominal self-reference, the child again moves through a series of stages (e.g., "Why me spilled it?", "What me doing?") in which his utterances do not match the adult model (Bellugi 1971).

Child language data are replete with structures evidencing the child's own productive system. These production data are important evidence, first, that children are not matching the adult model, no matter how clear the input; and second, that children are not deriving the same rules that adults use in their knowledge of language particulars. Thus, even on this interpretation of "poverty of the stimulus," recent mother-child interaction studies, in spite of contributing a great deal of important information concerning communication and language in the broader sense, have revealed little concerning how the child acquires the structural properties of grammar. Production data from children serve as important evidence that language acquisition is a more mysterious process than most learning theories, or even modern "cognitive" theories lead one to believe.

It is, of course possible to argue from the production data that children form a series of inductions or generate hypotheses against which to test language input data. Chomsky merely asserts that hypothesis-formation and confirmation would not explain the language-acquisition process. He does so primarily on philosophical and logical grounds (but we also know from such arguments that Achilles cannot outrun the tortoise). Since Chomsky's claim concerns the methods by which children come to possess the linguistic rules used to describe adult utterances, it is useful to seek evidence from language-acquisition studies on this point. When this is done, it becomes apparent that nothing in the language-acquisition data so far is at variance with Chomsky's emphasis on innate, unfolding factors in language acquisition. Since many psycholinguists appear to believe that studies have shown precisely the opposite, it is instructive to see why this is so.

Under Chomsky's influence from linguistics and Piaget's contributions from genetic epistemology, research on child development and language acquisition has moved away from viewing the child as a passive organism dependent on the input stimuli for all development. Instead, the child has come increasingly to be seen as an active organism interacting with environmental variables. This change coincided with Bruner's earlier seminal work (Bruner, Goodnow, and Austin 1956) on the use adults make of "strategies" in concept-attainment tasks. Child language research took up the idea and began to focus on the strategies children use during the language-acquisition process. For example, in an influential paper, Bever (1970) noted various perceptual strategies that are used for interpreting sentences. One of these, used by four-year-old children, consists of interpreting NP_1-V-NP_2 strings so that NP_1 is the actor, leading to incorrect interpretation of reversible passive sentences. Some children have been found to use this strategy across a variety of structures (Dewart 1975), and it is more correctly characterized as "treat the first animate NP as the actor."

A number of other strategies have now been examined. These include a "probable event strategy" (Strohner and Nelson 1974), in which two- and three-year-old children will often incorrectly interpret improbable active sentences (e.g., showing the horse jumping over a fence when in fact the given sentence is "The fence jumps the horse"); a related "congruency strategy" (Wilcox and Palermo 1975), in which the child places the toy items in the most usual relations, thus misinterpreting incongruent sentences like "Put the boat *on* the bridge"; strategies of carrying out the easiest motor response in sentence comprehension tasks (Dewart 1975; Huttenlocher et al 1968; Huttenlocher and Strauss 1968; Wilcox and Palermo 1975); strategies in which children base sentence interpretation on avoiding noun phrases serving a "double function" (Bever 1970) or "non–parallel function" (Sheldon 1974) or requiring "rule change" (Maratsos 1973); strategies in which the order of mention is taken to reflect the actual order of events in time (Clark 1971; 1973a; Cromer 1968; Ferreiro 1971; Ferreiro and Sinclair 1971); and a variety of strategies based on "localized" circumstances, such as where certain items should be placed in relation to horizontal surfaces and containers (Clark 1973b), and other extralinguistic considerations (Donaldson and Balfour 1968; Donaldson and McGarrigle 1974).

In a comprehensive review of the work on developmental language strategies (Cromer 1976b), however, it was argued that in most instances the concept of "strategy" has not been very instructive concerning how the child acquires the structures of language. This can be most succinctly illustrated by noting some results from Dewart's series of experimental studies on children's comprehension strategies (Dewart 1975). In her experiments, Dewart attempted to influence children's interpretations in various ways. For example, noting that children were influenced by situational context, she manipulated the context of passive sentences by preceding them with short verbal phrases that made the particular passive either appropriate or inappropriate. In an experiment examining motor strategies, she placed a toy in the child's hand and found that some children performed the sentence-comprehension task on the basis of moving the "mobile" toy and treating it as the "actor" regardless of its noun-phrase position in passive sentences. What is of especial interest in these experiments is that all children were initially tested on their knowledge of passive sentence structure in a "no context" condition. The children who evidenced adult knowledge of passive-voice sentences were not influenced by inappropriate linguistic contexts preceding the utterances or by having the incorrect toy in their hand when the sentence in the comprehension task was uttered. The children who evidenced contextual strategies or a mobile-toy strategy were those who did not yet know how to perform the passive in the adult manner, as gauged by their performance in the no-context condition.

The same point is illustrated in Strohner and Nelson's findings (1974) on the strategies children use at various ages. In their experimental task, the two-year-old children used the probable-event strategy mentioned earlier. By age four, however, they used word-order strategies. And by age five, most children interpreted sentences correctly even when they violated probable events. These children would show the fence jumping over the horse, since that is what the adult interpretation of the syntactic structure called for, and they did so in spite of knowing much more about real world event probabilities than the two-year-old children. The strategies used by younger children, then, are ways of behaving when forced by psycholinguists to perform on comprehension tasks when they do not yet comprehend particular language structures. They in no way lead the child to an understanding of those structures. Indeed, they lead the child to give a systematic set of wrong responses; they in no way explain the acquisition process. The strategies that have been investigated are not hypotheses about specific language structures that the child is trying out.

Furthermore, child language production studies do not give evidence of hypothesis-testing and confirmation. It would be difficult to know what would count as confirmation for the child in any case. Studies of production have repeatedly shown that children use some structures that are not being used by the adult models around them and that are therefore "disconfirmed," sometimes for years, before the child moves on to other rule systems.

The general notion of reinforcement or feedback in development is likewise challenged by empirical evidence. When Chomsky postulates a more prominent place for "growth" in various natural faculties (as opposed to learning by association, conditioning, and induction), he is putting forward a position that is uncogenial in the current intellectual climate. But the unpopularity of a position is no argument against its empirical validity. A single example from an area of cognitive growth other than language may be used to illustrate some of the problems that exist for the more usual notions of hypothesis-testing, confirmation, and feedback. Many experiments have been carried out in the last decade on the acquisition of Piagetian concepts such as conservation. Many of these experiments employ instructional techniques in an attempt to show the possibility of accelerating development. A common assumption is that what can be accomplished in the experimental laboratory, by fairly direct methods of reinforcement or feedback, is accomplished more slowly in real life situations in which the feedback is more incidental and not so direct. According to this view, for example, the child becomes a "conserver" on operational tasks because of his experience of the invariance of quantities in everyday, real life transformations.

Years ago, however, Smedslund (1961) carried out an ingenious experiment. He induced weight-conservation answers in nonconserving children with an experimental technique involving the transformation of plasticine and the use of balances on which the transformed pieces could be tested. Later, however, he surreptitiously took away some of the plasticine during its transformation into other shapes. The trained conservers immediately reverted to their original answers when the scales no longer balanced. They were not bothered and simply gave nonconservation answers as they had originally done. By contrast, many natural, untrained conservers in true Piagetian style resisted these attempts at "extinguishing" answers based on their operative cognitive structure; they argued that some of the plasticine must have been stolen or had been lost on the floor.

These results are usually cited in the context of arguing against experimental induction of true operational concepts. But they reveal something else that is equally significant. Not only were naturally conserving children resistant to direct feedback, but the nonconserving children in this experiment were given direct evidence on nonconservation. Feedback in real life is incidental and unconcentrated, but these children were given direct, concentrated, first-hand experience that the weight of objects changes with their transformation! Smedslund does not report on the later natural conservation development of the children in the experimental group, but it would be surprising if these children were found to be significantly retarded in their operational ability or if later, as young adults, they believed in nonconservation of weight due to their direct experience of manipulated feedback. It is not clear how developments in language or in cognitive growth in general occur, but the assumptions on which most theories of learning by association, conditioning, induction, and feedback rest have by no means been shown to describe or explain those developments.

Another approach in child language has been to hypothesize that language acquisition is explainable in terms of general cognitive growth (Beilin 1975; Cromer 1968; Macnamara 1972). A straightforward cognitive theory of language-learning would assert that the acquisition of particular linguistic structures depends on prior cognitive growth, allowing the formation of certain concepts by the child. It is assumed that, as the child develops certain meanings and intentions, he notes how these are encoded in the speech he hears about him. In this view the child forms associations between the situational events and the language he hears. It is said to be a "modern" associationist viewpoint, since the child is viewed as taking an active part in the process, as opposed to the view of the passive child of the earlier behaviourist theories (Donaldson 1978).

The basic problem with cognitive theories such as Donaldson's is that, once the child has the concepts, there is nothing more to explain about language acquisition. All the child has to do is to see how the concepts are encoded in the input language. This input language has now been shown, by careful mother-child interaction studies, to be clear and free of hesitations, false starts, and the like. But the problem with this naive theory is that the productive utterances by normal children in their natural environment are at variance with its claims. Empirical research on the utterances children produce while acquiring language simply does not support associationist claims. (This argument is developed more fully elsewhere; Cromer 1974; 1976a; in press). In addition, there is evidence from studies of adult aphasia, some of which Chomsky does quote in his target article, showing that the retention of certain concepts does not necessarily ensure the retention of linguistic structures in which these concepts are encoded. Indeed, some aphasic individuals have been trained in a communication-symbol system in spite of severe linguistic impairment (Glass et al. 1973). Hughes (1972; 1974/75) obtained similar results with children with cogenital or early acquired aphasia. There is, then, a good deal of empirical evidence, from both normal, unimpaired children acquiring language and from aphasic children and adults, that concepts and their encoding in language are not identical. Cognitive theories that do not allow for specifically linguistic processes are seriously flawed and are simply not supported by empirical research.

So far, it has been argued that the main thrust of Chomsky's arguments — namely that the language faculty and perhaps other aspects of the human cognitive system should be characterized in terms of growth rather than learning — is not at variance with what is known from empirical research. Although he has stated many of his claims merely as assertions, it has been shown that there is evidence that can be cited in support of his views. However, the kind of innateness in which Chomsky appears to believe is not the only possible one.

There are other ways to view innateness and growth. In comparing and contrasting the views of Chomsky and Lenneberg, Catlin (1978), has characterized two basic approaches to innate structures. In one, the preformationist view attributed to Chomsky, the various innate properties are in some sense fully formed at the beginning of development. Environmental factors play little or no role in the formation of universal grammar. Thus Chomsky takes universal grammar as a given property that influences the acquisition of particular languages. By contrast, Lenneberg's view is characterized by Catlin as "epigenetic"; environmental influences are seen as playing a role in development as certain innate aspects unfold and interact with the environment.

At present there is no empirical way to judge between these two ways of viewing possible innate factors in language. Chomsky, by focusing on universal grammar, appears to undervalue environmental influences. But those who are interested in child language are concerned with the acquisition of a particular language. For this task, whatever may be innate about universal grammar must interact with the stimulus input. Much of the continuing research in mother-child interaction, strategies, and cognitive theories of language acquisition continues at some level to emphasize associationistic types of principles, which are discredited by empirical evidence. Alternative approaches to the study of child language are possible, however. The cross-cultural studies of language by Slobin and his associates (Slobin 1978; 1979), and the experimental studies by Karmiloff-Smith (1977; 1978; 1979), which detail the interactions between what a child brings to the language-acquisition task and the particular structures of the language he is acquiring, represent true advances in the attempt to understand this still mysterious process. A description of these studies in terms of an epigenetic-interactionist viewpoint can be found elsewhere (Cromer, in press). Although the question is still open, nothing in the empirical evidence is at odds with some notion of species-specific innate factors in language.

by **Robert Cummins and Robert M. Harnish**

Department of Philosophy, University of Wisconsin, Milwaukee, Wisc. 53223; Department of Philosophy, Department of Linguistics, University of Arizona, Tucson, Ariz. 85721

The language faculty and the interpretation of linguistics

Chomsky thinks there is a language faculty (LF) characterizable via a system of rules and representations, the structure of which is largely innate or innately determined. With all this we are inclined to agree. But we have a pair of related concerns about Chomsky's characterization of the LF: (i) Chomsky assumes that linguistics is about the LF, and this assumption is undefended and dubious. (ii) Given (i), we need, but do not have, a proper characterization of what the LF contributes to "linguistic capacities" broadly conceived.

We are puzzled as to why Chomsky is puzzled when he writes; "What is 'psychological reality' as distinct from 'truth in a certain domain'?" and "Why didn't [Sapir's] 'linguistic evidence' suffice to establish 'psychological reality'?" If linguistics is about the mind, or one of its faculties – if it is about psychological states – then, of course, evidence for the truth of the theory is evidence of psychological reality. But linguistic evidence itself can't tell us whether linguistics is about the mind, and that's the issue – subject matter – that linguistics is *about*. The "psychological reality" dispute boils down to this: should the theoretical terms playing an essential role in the results of linguistic analysis and description be interpreted as about mental states, or not? After all, most practicing linguists were brought up on the following words:

> From now on I will consider a *language* to be a set (finite or infinite) of sentences, each finite in length and constructed out of a finite set of elements . . . The grammar of L will thus be a device that generates all of the grammatical sequences of L and none of the ungrammatical ones (Chomsky, 1957, p. 13).

Theories of language constructed to the letter (and in the spirit) of such remarks need no more be about the mind than a piece of set theory is. A linguist may decide to characterize a psychological competence or capacity, or to provide a minimal axiomatization of all available linguistic properties and relations. The resulting systems need not have much in common: Why assume they are about the same thing?

Chomsky's puzzlement about the "psychological reality" issue strongly suggests that he thinks there is no alternative to supposing that linguistics is psychology. And it seems clear that he thinks this because he is a "conceptualist" about language – i.e., because he thinks there is no such thing as a natural language independent of speaker's psychological states, hence nothing for a nonpsychologized linguistics to be *about*. Talk about language is just disguised talk about shared psychological states. This may be the right view to take, but it hasn't been adequately defended.

The view does have consequences, however, for on this view, whatever truths linguists uncover must be psychological truths: there is nothing else for them to be. Indeed, they must be truths about the LF. It seems to us that this approach runs a serious risk of either saddling linguistics with unmotivated psychological constraints, or imputing to the LF psychologically unmotivated aspects of linguistic descriptions. For instance, given that language is a social as well as a biological phenomenon, it will have a conventional as well as a genetic aspect, and linguistic descriptions will describe both. It may or may not be true that promises obligate, but should the fact that this is not biology matter to a linguist? Also, as we pointed out earlier, the simplest axiomatization of the linguistic facts need not restrict itself to formal apparatus that is psychologically motivated. These points will be obscured (or prejudiced) if the problem of interpreting linguistics is assimilated to the problem of assessing its empirical support.

This brings us to our second concern about the specification of the LF. Faculties, of course, are functionally identified, so if we want to know how Chomsky envisions the LF, we will do well to ask how Chomsky thinks the LF will figure in an analtyical explanation of the capacity to use and assess language. (By the capacity to use a natural language, we mean the capacity to speak and understand it, to communicate with it – in short, to employ it in speech acts. By the capacity to assess a natural language we mean the capacity to judge grammaticality, intersentential relations, and the like.) We are to imagine the Use and Assessment System (UAS) analyzed into three components: 1) the LF proper, which provides syntactic and phonological analyses and relates these to representations of "meaning"; 2) a conceptual system, which "involves the system of object reference and also such relations as 'agent,' 'goal,' 'instrument' . . . ," and perhaps aitiational analysis of concepts; and 3) the Use system proper, the characteristic capacity of which is presumably the capacity to perform and recognize speech acts.

Chomsky suggests that a system consisting of only components (2) and (3) might be able to communicate, even be a party to communicative conventions. Let's call the result of deleting the LF from the UAS the Communication System (CS). Adding some ritzy supporting equipment, we get a system that could refer, predicate, tell the general what it saw and order the troops to move out. Well, maybe. Anyway, Chomsky certainly leaves this possibility wide open. We think it is the capacities we have assigned to the CS that most philosophers and psychologists have (traditionally) had in mind when they thought about linguistic capacities. The following research problems appear, on Chomsky's conception, to be attackable without bothering about the LF: a) a theory of denotation, truth, and, to some extent, meaning; b) a theory of speech acts, including referring and predicating; c) a theory of communication and linguistic conventions; and d) a theory of understanding.

If the LF isn't implicated in the explanations proferred by these theories, what *is* its explanatory role? Chomsky appears to leave this to future empirical inquiry, supposing, presumably, that the LF can be sufficiently characterized in terms of its content. Indeed, it is clear enough what outputs the LF can make available to the UAS. What has yet to be articulated is an hypothesis concerning how those outputs enter into the explanation of the capacities of the UAS. (We can see how it enters into the explanation of language assessment, but that is surely not the main act.) Lacking such an hypothesis, we don't know why we should favor Chomsky's analysis of the UAS to some other analysis. When we have a clear idea what the outputs of the LF are for, we will have a useful fix on what the LF is. Chomsky is offering a "modular" approach to mind – i.e., a functional analysis of mental capacities. But the LF hasn't been functionally identified.

The same point can be seen from a slightly different perspective. On Gricean accounts (and what else is there?), the CS is up to its ears in propositional attitudes, cognitions, and subtle reasoning. That is why it is a methodological battleground. The LF, by contrast, looks comfortably computational: comfortably nonrepresentational, in fact. The only reason to characterize the outputs of the LF in terms of their representational contents is that the availability of these representations is required to explain the capacities of the UAS. This is just where the light is dimmest: we don't know which representations we need, hence what to impute to the LF, until we have a fairly clear picture of how the LF is supposed to interact with the other components of the UAS.

by **Daniel C. Dennett**

Department of Philosophy, Tufts University, Medford, Mass. 02155

Passing the buck to biology

I think that Chomsky's claim that universal grammar is inately fixed *in the form of explicit rules* is gratuitously strong. That is, although on a sympathetic interpretation it could turn out to be true, the evidence and argument so far adduced support only a milder and less specific version of the thesis: (roughly) there are innately fixed structural features – design features – that specifically constrain the development of linguistic competence in the child. So stated, the thesis is no news at all; what *is* news, and not entirely welcome news, is that the constraints are much stronger than one might have thought. Since I know from discussions that other commentators – e.g., *Searle* – will produce good arguments against Chomsky's strong version, I will take the opportunity to comment on why even a milder version of his thesis is in a certain respect unwelcome. (We should always seek Truth, but what we find may nevertheless be regrettable on occasion.) This is intended not as an objection to Chomsky, but as reflections on the context in which his arguments take place.

We can all agree, these days, that extreme *tabula rasa* theories of learning or cognitive development are false. Even Skinner acknowledges the necessity of a modicum of genetically transmitted structure for fixing the effects on the organism of his postulated reinforcers. And no one flies to the opposite extreme and denies that there is phenotypic learning (or differential development) in response to different environments or perceptual histories. The truth lies somewhere in the middle, and the disagreements concern the amount, specificity, and detailed structure of the innate contribution. Perhaps no one supposes there is a larger innate contribution than Chomsky does, and perhaps the facts will eventually bear out a position close to his, but his polemics sometimes ignore the perfectly reasonable motivation behind the contrary perspective – what we might call the *minimalist* research strategy.

Moving more and more structure into the category of *innate* may help us to get a more realistic picture of the individual capacity for cognition and learning, but what is innate must have been "learned" in evolutionary history, so the task of explaining the genesis of the design in the organism remains. Views imputing a minimally-prestructured organization to the infant (that easily succumb to empirical refutation) may nevertheless be rehabilitated if they are interpreted as *rational reconstructions* of the genesis of intelligence. View them, perhaps, on the model of social contract myths, or Rawls's "original position" thought experiment, which are attempts to explain the genesis of social or political structures, rules, or principles at a level of abstraction that renders them immune from disconfirmation by brute historical fact. Of course, there is no guarantee that such a high level of abstraction will yield any reliable or theoretically useful results, but there may be no practical research alternatives – due to the simple inaccessability to research of the actual processes one wants to describe.

Let me add one more specimen to this garden of analogies. Suppose the evidence mounted in support of the hypothesis that life did not in fact evolve on Earth from the lifeless soup of preorganic molecules; rather, the Earth was seeded at some early time by living "spores" (or whatever) from elsewhere in the universe, perched on some meteor, or intercepted while floating by. There is something profoundly unappealing about this hypothesis, and it is easy to say what it is: if it is true, then the fascinating question of how life developed from nonliving "materials" (as it must have, *somewhere*) becomes drastically less accessible to study. For example, if we will not be able to rely on well-grounded assumptions about the prevailing conditions on Earth during various periods of its early history as boundary conditions for candidate scenarios of this momentous development, then our efforts to devise and confirm the right scenario will probably be too unconstrained to permit anything but "unscientific" speculation. We would *like* to be able to view the infancy of the Earth as a *sufficient tabula rasa* for the genesis of life; "gifts" of life from elsewhere would be most unwelcome, for it would be immensely more difficult to infer the genesis of *their* design than if the entire process could be assumed to occur locally. Unwelcome though such a hypothesis would be, it is empirically possible, and one can imagine being able to prove that it was true – e.g., from arguments that fixed a maximal speed of mutation and selection and showed that there had not been enough time on Earth for the *whole* process to have occurred locally.

Chomsky's arguments, from the poverty of the stimulus and the speed of language acquisition, are analogous; they purport to show that there must be *large* gifts of design in the infant if we are to explain the speedy development of the mature competence. And while we can take solace in the supposition that we may someday be able to confirm the presence of these innate structures by direct examination of the nervous system (like finding fossils of our extraterrestrial ancestors), we will have to accept the disheartening conclusion that a larger portion than we had hoped of *learning theory,* considered in its most general form as the attempt to explain the transition from utter ignorance to knowledge, is not the province of psychology at all, but rather of evolutionary biology at its most speculative. The more the infant brain can be viewed as a *tabula rasa,* the more accessible to *experimental* research the *ultimate* mysteries of learning will be; if the facts constrain psychologists to pass the buck to the evolutionary biologists, we will have to settle for more abstract and speculative answers to the ultimate questions. No *a priori* argument could refute Chomsky's empirical contention about the amount of innate structure actually to be found in the infant, but it is nevertheless reasonable to hope, for the sake of science, that he has overstated the case.

Note

D. C. Dennett is a Fellow for 1979–80 at the Center for Advanced Study in the Behavioral Sciences, Stanford, California 94305.

by **Michael T. Ghiselin**

University of California, Bodega Marine Laboratory, Bodega Bay, Calif. 94923

Evolutionary anatomy and language

Anatomy, as I see it, may fruitfully be defined as the study of organization. As such it does not partake of the static character of morphology but has a functional aspect as well. It includes not only the study of organized beings, but the processes – such as selection and learning – that organize them. Nor does it seem expedient to limit its scope to the body. Languages as well as people have more than just morphology – and surely both have evolved. One might well expect to find some kind of parallelism between language and other evolving systems.

Chomsky and I agree that biological and linguistic entities have much in common. But as a comparative anatomist I question some of his premises and conclusions. In particular, he wishes to establish that there exists a separate linguistic faculty radically different from other biological entities. My response will be cast as a dilemma. By analogy with what we know about organized beings in general, he has gone too far. If this be so, either the effort to ground linguistics in anatomy must be abandoned, or else language is probably more closely integrated with other faculties and shares more principles with them than Chomsky maintains.

Chomsky rightly points out that language diversification is analogous to speciation. Indeed the parallels between linguistic entities and biological ones could hardly fail to escape the attention of evolutionists. Languages "speciate." A language (such as French) corresponds to a species (such as *Homo sapiens*). So too with a dialect and a race, and an organism and an idiolect. One could list a host of other analogies, such as that between clines – character gradients within species – and their equivalents in dialect geography.

However, we seem to differ upon a fundamental ontological issue. I am a professed realist, whereas Chomsky advocates a peculiar version of conceptualism. It is not just classes that Chomsky treats as "idealizations" – a view I would dispute as well – but also individuals. An uncertainty as to what exists "in the mind" and "in nature" can scarcely fail but raise havoc with theories that depend upon what biologists call "natural classification" for their epistemic power and ontological legitimacy. Chomsky treats the individual organism as an "idealization reflecting a particular way of looking at things and processes," but he seems to be asserting that the individuals themselves have no properties "apart from our mode of conception." (I wondered if this were merely an unfortunate ambiguity in one sentence, but I found levels treated analogously elsewhere.) It seems rash indeed to treat an organ or an organism – individuals in the logical sense – as conceptual. If I dissect any creature or a part of it, its properties do come to me prepackaged, in the sense that they existed apart from my thinking about them.

It has lately been recognized that species are, in the logical sense, individuals, not classes (Ghiselin 1974 and earlier works cited therein; Hull 1976). This thesis has numerous implications for all sciences in which change is significant (Hull 1975; Reed 1979; Ghiselin 1980). Individuals have no defining properties (or essential attributtes in the old terminology). Much error has resulted from an understandable, but nonetheless mistaken, interpretation of biological and linguistic groups alike as classes. Trying to "define" French, Noam Chomsky, or *H. sapiens* is a hopeless task, beyond an ostensive definition as with other proper names. "Human language" has no defining properties for the same reason that "Noam Chomsky's idiolect" and "tellurian life" have none: they are individuals again. If we are to define any classes here, they must be like the class of species, or classes of language-like or life-like individuals. This is precisely what one would expect of entities that change through time. But it runs counter to the hopes and expectations of those who have attempted to treat species and languages as if they were classes, often in the vain and egotistical hope of putting our species in some privileged metaphysical position. Although *H. sapiens* has acquired a lot of pecularities (none of which are defining) gradually through time, many people wish to have a point at which there arose a "set" of "essentially human" beings. Might not the same have occurred with this "human language" entity? It is ironic that the ability to speak was long taken to be a "defining" property *Homo*, a view that went well with the doctrine that only man can u language and therefore think. When apes were taught a sort language, the effort was not abandoned; it turns out that apes are n really using the linguistic faculty. Rather they are aping it with t cognitive one. Apes think now. [See *Cognition and Consciousness Nonhuman Species: BBS* 1(4) 1978.]

In disowning our relatives, it helps to erect a modular theory of t linguistic faculty. Ideally this should be separate from every oth faculty and rest upon *principia sui generis linguae*. But how great is t difference, really? Consider some analogies.

For Chomsky, language is modular in the sense of functioning apa from other faculties. But consider the analogy of the nervous a endocrine systems. They are so intimately interdependent in functi that we are hard-pressed to say where one ends and the other begi They work together as one integrated apparatus. Nor can we say th they share no common principles: consider inhibition and excitatio Why should not the same be true of language and cognition generall Chomsky never tells us precisely how independent they are, though suggests empirical means of resolving this issue. Destroying one org might lead to its functional replacement by another. But what of t converse equipment – destroying the nonlinguistic "organs" to see language is unaffected? We can substitute hearing for vision, to considerable extent, when the corresponding organs are destroye But observations on the effects might only tell us peculiarities of eac organ, and not whatever they have in common. If one blood vess becomes blocked, another may take over its function. But it is not cle how any peculiarities of the new arrangement would bear upon t question of whether we were dealing with organs working upo different principles.

Another sense of "modularity" is not used by Chomsky. This is t occurrence of "iterative" structure – modules like the virtually identic units in a motel. For good reasons, many organized beings exist a groups of parts having a great deal in common, albeit with variations seems easier to construct and control structures if the parts have t same arrangement and principles of functioning and organizatio Anyone who examines the vertebrate nervous system can hardly fail be impressed by its segmental character. That segments work on t same principles can be demonstrated by experimentally adding supe numerary limbs to embryos: these function more or less normally. the anterior end of the body specialization and diversification ha extensively occurred – but we cannot rule out common principles. If t right and left halves of the brain differ, the principles are close enou to make the differentiation hard to detect. It would seem, indeed, th Chomsky has carried the principle of division of labor far beyond wh is biologically or economically reasonable (Ghiselin 1978). Our tee are iteratively organized, with a gradient from front to rear. Incisors one thing, molars another, but premolars both grind and slice. All t teeth work upon common mechanical principles, resemble one anoth structurally, and develop in much the same way. The faculty mastication should not be divided outright into slicing and grindin even though the distinction is a real one. Nor should the teeth b denied their use in speech.

Finally, I wonder if the search for a single, universal grammar mig be somewhat misguided. Nature abounds in alternative modes organizing herself. There seem to be several ways of proceeding fro a zygote to a fully developed organism. The obvious example is in t distinction between determinate and regulative eggs. The organis can be "represented" as a kind of spatial pattern in the cytoplasm, as a series of events that trigger subsequent events. And some of t rules are not, strictly speaking, in the nucleus. The trabeculae of t bones, for example, are arranged in a pattern well-adapted to resisti stress. But abnormally healed fractures show that the arrangemen quite flexible, evidently involving a response to the stresses the selves. Hence it becomes very problematic, for one versed in develo mental mechanics, to decide to what degree, and in what sens "universal grammar is an element of the genotype." For all we know may be that the zygote is instructed to produce language one way another, that there are few ways of doing so, and that it "discover which among various possibilities happen to work. In such an extrem case, organisms would have to "invent" or "discover" the principles, and select among alternative ones every generation – much as some animals must discover what is edible. Chomsky rightly considers many such possibilities, but they need to be pressed. I do not believe that an adequate answer will be obtained until we possess an empirical approach in which truly decisive tests of the hypotheses are proposed and executed. We need something analogous to an experimental embryology, not just a physiology, of language.

Acknowledgements

I thank Edward S. Reed and Jan David Wald for advice and comments on a rough draft.

by **Gilbert Gottlieb**

North Carolina Division of Mental Health, Raleigh, N.C. 27611

Elaboration of maturational and experiential contributions to the development of rules and representations

Although no one can yet pretend to understand the precise nature and extent of "innate" influences on psychological development, many of us readily share Chomsky's notion (even if it is necessarily vague in detail) that human linguistic and cognitive development are constrained in certain ways, that "predispositions" abound, and that traditional conceptions of learning are ill-suited to deal with these significant complexities. In fact, the behavioral and psychological development of nonhuman animals is similarly constrained in all cases yet investigated – nowhere do we find indefinite malleability or infinite plasticity – and traditional concepts of learning are equally unsuited to the task of understanding the species-typical motor and perceptual development of nonhuman organisms. Based on the present evidence from a variety of species, the "blank slate" theory of mind should be relegated to a minor station in scientific discourse: indefinite plasticity is the exception, not the rule. Even in those cases where patterned sensory experience plays a crucial role in the attainment of species-specific perceptual outcomes, the influence of such experience is sharply delimited: exposure to only certain patterns of stimulation is effective in channeling perception, and perceptual development can be channeled only into certain paths and not others (e.g., Gottlieb, in press; Wiens 1970). In those cases where the range of malleability seems more extensive (Jaisson 1975), temporal factors ("critical periods") circumscribe the duration of such susceptibilities in the developmental history of the organism.

Thus Jaisson's ants are more flexible in their perceptual development than we might have imagined, and humans are probably more constrained than we had been led to believe, yet experience (not to say conventional learning) plays an important role in normal psychological and behavioral development. So, what then? I think it is obvious that we must begin to formulate the various roles of experience in species-typical development in ways that are compatible with the facts of neurosensory and neuromotor maturation. This is a task that seems highly relevant to Chomsky's doubts about the adequacy of traditional concepts of learning in helping us to understand what he calls "the growth of grammar in the mind." Along this line, perhaps I may be forgiven for calling attention to the fact that newly formulated concepts of the role of experience in the species-typical development of animals (Gottlieb 1976a; 1976b) are beginning to find some application in the study of phonological development in humans (Aslin and Pisoni 1980). In brief, what Aslin and Pisoni have done is relate recently delineated functions of early experience in animals (maintenance, facilitation, induction) to hypotheses concerning phonological development in humans (which they term universal theory, attunement theory, and perceptual learning theory, respectively). The attempt here is to identify the specific roles of early experience in three different versions of how phonemic perception might arise during the course of human development. This is an experiential framework that explicitly takes maturation into account and thus seems relevant to Chomsky's contention that ". . . we do not really learn language; rather, grammar grows in the mind." Although I admittedly do not follow the details of Chomsky's proposal as well as I would like to, his analogies of language acquisition with the role of experience in establishing receptor networks in the visual cortex make me think he may find the concepts of maintenance, facilitation, and induction congenial to his program of thought.

by **Gilbert Harman**

Department of Philosophy, Princeton University, Princeton, N.J. 08540

Two quibbles about analyticity and psychological reality

1. About analyticity. Certain propositions are sometimes said to be "analytic" or true by definition. Favorite examples are: *a bachelor is an unmarried man* and *a cat is an animal.* But how can anything be true merely by definition? A definition is an assumption or hypothesis of a certain sort. To say that something can be true by definition is to say that one can sometimes make something true by assuming it is true. How can one do that? No adequate answer to this question has ever been given.

Examples of allegedly analytic truths are often not even true; for instance, there are bachelors whose divorces are not yet final (Harman 1973, Winograd 1976). Other examples we can easily imagine rejecting, as we would reject *a cat is an animal* given the discovery that cats are radio-controlled robots from Mars (Putnam 1962).

Chomsky suggests that there are better examples of analytic connections "involving properties of the verbal system," such as, *if I persuaded you to leave, then at some point you intended to leave.* But, in order to persuade you to leave, I might keep you ignorant of the fact that what you are doing is leaving, so that at no point do you intend to leave. And, even if some such connection held between persuading and intending, it would not be an analytic connection. We could conceivably discover that intentions are a myth and that people never intend to do anything (maybe Skinner is right after all). Given such a discovery, we could still suppose that people sometimes persuade other people to do things.

Of course, as Chomsky observes, intuitions about such examples are "dubious" and uncertain. So let me stress that the real issue concerning analyticity is not whether examples of putative analytic truths can be found, but whether assuming or supposing or postulating that something is true can ever make it true. I do not see that Chomsky's work sheds any light on this.

2. About psychological reality. Chomsky claims that it is pointless to distinguish the question of psychological reality from that of truth, and he asserts that no similar distinction is made in the natural sciences. But, given any theory we take to be true, we can always ask what aspects of the theory correspond to reality and what aspects are mere artifacts of our notation. Geography contains true statements locating mountains and rivers in terms of longitude and latitude without implying that the equator has the sort of physical reality the Mississippi River does. Similarly, we can describe some part of the universe, given a choice of spatiotemporal coordinates, recognizing that the special role of that choice of coordinates in our description is an artifact of our notation. And we might present a theory in axiomatic form without assigning any physical significance to the distinction between axioms and theorems.

Sometimes we are not sure about the physical reality of some aspect of a theory, even given strong evidence for the truth of the theory. A different sort of evidence may be needed. The postulation of quarks gives a structure to the proliferation of subatomic particles, but physicists demand a different sort of evidence in order to establish the physical reality of quarks.

Chomsky implicitly recognizes the point as it applies to linguistics when he acknowledges that one linguistic theory may be a "notational variant" of another. Aspects of a true theory not shared by its notational variants are not taken to have psychological reality. The "linguistic evidence" for a given linguistic theory is like the evidence that led to quark theory – namely that the theory brings order to a given domain. That by itself may not indicate what aspects of the theory correspond to reality and what aspects are artifacts of notation. We might wonder, for example, whether the grammatical structures of sentences have psychological reality or are mere artifacts of our notation, so that a notational variant of our theory could assign different structures to sentences. Click experiments, as in Bever et al. (1969), might be relevant here (subjects who heard clicks while they were listening to sentences tended to hear the clicks as displaced in the direction of major clause boundaries).

I completely agree with Chomsky that this does not mean we have to appeal to psychological *experiments* to provide evidence for psychological reality. The following argument from Chomsky (1975a) makes the point. Consider the hypothesis *H* that in acquiring a language one acquires an internal representation of the rules of grammar, where these rules are similar to those Chomsky or one of his associates would give for the language acquired. It turns out that some of these rules could not be learned through trial and error, since errors are never made. People acquiring the language never violate these particular rules and never have to be corrected. *H* implies that these rules must be built-in ahead of time before it is determined what language is being acquired. This means that the rules in question are either universal rules, applying to every language, or at least that they represent the "unmarked case" applying to a language unless someone acquiring the language would hear that the rule is violated. The hypothesis *H* therefore leads to predictions about other languages, where these predictions are based entirely on evidence from a single language such as English. To the extent that these predictions are confirmed (and to a surprising extent they are), that is evidence for *H* and therefore evidence for the psychological reality of the rules of grammar – evidence that is not based on psychological experiments.

by **Patrick T. W. Hudson**
Institute for Perception TNO, Soesterberg, The Netherlands

Minimalism in cognition and language: rich man, poor man

Let us be clear. In his article Chomsky states plainly that his terminology may be confusing, and he goes on to offer to replace the loaded term "know" by "cognize." Furthermore, he makes it quite clear that when he uses the words "mind" or "mental," he is making no claims, unlike a true dualist, to some special level of existence, but rather to "an abstract characterisation of the properties of certain physical mechanisms." He wishes to defend the abstract level of much theoretical linguistics as being a proper scientific enterprise, much like modern particle physics, which has an equally or even more abstract vocabulary. As this all seems so unexceptionable, one wonders what all the fuss is about. Nevertheless, recently psychologists, at least, have reacted against Chomsky's formulations of what is here called cognitive science. They wish to deny the special status of the language faculty and, accordingly, the modular approach to mind.

Why should this be so? Strict behaviourists we may ignore; they are committed to no internal structure and therefore no modularity; what is more they have their minds (whose existence they deny) made up. There are, then, two possibilities within those who subscribe to some form of richly structured system; one group, which Chomsky advocates, prefers relatively independent and highly structured modules, the other prefers a homogeneous rich structure that defines heuristics for the creation of high-level knowledge from external sources. At one level it is, as Chomsky reiterates, obvious that some prior structure or knowledge is present, and that not merely at the level of physical structures such as limbs. Anyone who has handled neonates will verify that the newborn baby can walk. That is to say, that he possesses the crucial information for walking, which can be seen (given adequate support and encouragement) by placing the child upright on a flat surface and letting him walk. This information cannot be actively used, however, until many months later. In such an example we see that the core knowledge about walking – about forward locomotion by the coordinated actions of the legs, whilst later requiring sufficient strength and knowledge about balance and the operation of muscles – does not, of necessity, include that latter knowledge as part of the information for walking per se. So it is, I take from this article, with language.

I understand that the core knowledge, which grammarians refer to as universal grammar, also requires other systems of knowledge for its effective use. These systems may be identified as the conceptual system and perhaps specialised systems for handling speech sounds in perception and production. These may be interdependent in operation but, at some level of analysis, may be considered entirely on their own. Our task, then, is to characterise and describe both the general structures that may be available to many systems and the specific structures that mark out and define a particular structure. So far so good. Chomsky, arguing from the poverty of the stimulus and of the world – factors that can only underdetermine the possible outcomes of generalised systems – wishes us to accept that the distinct modules, or faculties, that he proposes are themselves richly structured. Certainly current work (reviewed by Pinker 1979) on the learnability of languages suggests that the initial state of a language-learner must contain a number of fixed assumptions if he is to make any headway at all. These assumptions, or preset parameters are another description or form of the knowledge that the language-learner brings to the problem of learning a language. It seems that what Chomsky proposes here is that this internal structure is rich, which implies that it contains many preset values, fixed for a given organism.

This appears to lead us to a paradox, or very close to one. Chomsky has championed the minimalist approach to linguistics: the use of as few explanatory constructs as possible to adequately explain the data. The minimalist approach currently seems to be very successful in linguistics, and Marcus' (1978) use of the determinism hypothesis has shown elegantly how one can go further. One of the major lines of the argument presented in Chomsky's target article is against those psychologists who wish to explain as much as possible of the development and end-state of the cognitive system with only a few restricted operations and assumptions. At the same time we appear to see Chomsky advocating explanatory theories with a minimal and poor structure as strongest in linguistics, while at the same time advocating the most liberal of approaches when considering the wider sphere of cognition, of which he takes language as an example. How can he criticise minimalists for carrying out a program he recommends?

Maybe there is no paradox after all. Maybe we have been misled by a lack of clarity in the definition of "rich," just as many psychologists have been misled by (their own) misunderstanding of such terms as "know," "mental," and "incorporate" when they tried to understand and use generative grammar. What may be meant by "rich" here is that, in the limiting case, a cognitive structure will be distinct and rich even if it possesses *only one* extra structural element. In the ideal case we may attempt to relate specific cognitive structures to the outcome of that one specific value associated with a crucial parameter. Certainly in development one small specific factor at a critical time may have large and far-reaching effects; for instance, the merest push is sufficient to set a balanced pole to fall in the direction of the initial push. Marcus (1978) argues that the constraints of linguistic theory, which at that level must be accepted as pure stipulations, can be seen to fall out naturally as a consequence of building a parser single-mindedly following the determinism hypothesis, of allowing no back-up or alterations in a natural language parser. If this is at all true, we can see with greater clarity what is meant by the rich structure – what are the fixed assumptions that enable the system to work and, possibly, to be developed at all.

This argument is not with what Chomsky says but how he says it.

by **George Lakoff**
Department of Linguistics, University of California, Berkeley, Calif. 94720

What ever happened to deep structure?

There are two levels on which to view Chomsky's work: 1) the technical apparatus and empirical considerations that enter into the detailed linguistic descriptions he advocates; and 2) the broad general claims he makes concerning the nature of the mind. The technical apparatus and empirical considerations have changed drastically in the past twenty years. In the mid-1960's, he viewed linguistic structure as based on "deep structures" with semantic significance, and transformations that mapped deep structures onto "surface structures." In the past several years the role of transformations and deep structures has become virtually nonexistent. Deep structures have lost their semantic significance and come to look more and more like surface structures; and correspondingly, transformations have virtually disappeared. In 1965, generative grammarians had found hundreds of transformations. A decade later, Chomsky (1975c) was speculating that there might only be one. This would make the deep structures of most sentences identical to their surface structures. There is no mention of deep structures and transformations in the present work. The reason for this will be discussed shortly.

Outside of linguistics, the academic public knows virtually nothing of the technical details of Chomsky's linguistics. What seems to stick in most people's minds are his claims about innateness. On the whole, his view of innateness has not changed. Nor has his view on what he now calls "modularity." His views on modularity are extreme and, I think, fundamentally mistaken. Briefly, modularity in Chomsky's linguistics means the following:

1. The syntactic rules of a language are matters of pure form. They are completely independent of:
   a. meaning;
   b. the way people use language to communicate;
   c. the processing of language, in both perception (hearing and understanding) and production (speaking); and
   d. any other aspect of human experience.
2. The syntactic rules form a "module" that interacts with, *but is independent of,* other modules.
3. One of the things that principally separates man from the lower primates is the presence of a syntactic module with the above properties. Hence, Chomsky grants in his target article that primates may have part of a conceptual system and may be able to communicate, but he holds firm on their lacking a syntactic system ("even the rudiments of the computational structure of human language").
4. The syntactic rules (which are independent of meaning, communication, processing, and experience in general) form the most central and important part of the "human language faculty." Apes don't have syntax and so are "in this regard like humans without the language faculty."
5. What is innate in humans, as opposed to lower primates, is the presence of an independent syntactic "module" – that is, the capacity to structure language in terms of pure form in the way humans do.

Chomsky suggests that this view of modularity and innateness is grounded in empirical linguistic research. That is highly questionable, to say the least. The way Chomsky uses the modularity claim amounts to a matter of definition. The principal reason that Chomsky's views on the technical details of linguistics have shifted so radically is that he is trying to maintain his views on modularity in the face of mountains of evidence to the contrary. Perhaps the main reason that deep structures and transformations have disappeared is that it turned out to be impossible to both keep them and maintain the principal assumption of modularity: the independence of syntax. Most of the research showing this was done by a group of the first generation of Chomsky's followers: myself, Paul Postal, John Robert Ross, James D. McCawley, Robin Lakoff, Charles Fillmore, David Perlmutter, Edward Keenan, and others. What we found was that meaning and use (communicative function) effected virtually every rule of syntax. In order to keep the central modularity assumption – the independence of syntax – Chomsky has progressively redefined and narrowed the domain of syntax so that rules that were traditionally part of syntax (e.g. case assignment, pronoun agreement constraints, the occurrence of negative polarity items) were redefined as being in "semantics" – that is, in a different module. Only by constant redefinition and narrowing of the facts for which the syntactic module is supposed to account has Chomsky been able to maintain modularity.

What is new in Chomsky's present essay is terminology, analogies, and metaphors – mostly from the realm of biology. The biological analogies have no linguistic content – that is, they make no new empirical claims about the structure of language. The analogies are almost entirely gratuitous. When Chomsky analogizes between the linguistic and conceptual systems on the one hand and the visual and circulatory systems on the other, we are in the realm of rhetoric, not science. But as rhetoric, it is effective – at least so far as academic politics is concerned. The term "mental organ" for the linguistic faculty is artfully chosen. The effect is to say: "I'm doing for the mind what biologists are doing for the body." The use of biological terminology and bibliography has the effect of suggesting rapport with biologists – simply by speaking their language. It also has the effect of making it seem to those who don't know the linguistic details that Chomsky's views have biological backing.

It is particularly striking that the biological analogies Chomsky uses come not from contemporary biology – molecular biology, genetics, and so on – but from earlier biology – the biology of separate systems – the circulatory system, the visual system, and so on. Perhaps the greatest leap forward in contemporary biology has come from transcending the separate systems approach and asking what is in common: cell structure, DNA structure, and so on. For me, the most exciting question at present in the cognitive sciences is what *similarities* there are in the various cognitive faculties – language, thought, vision, motor control, and so on.

Chomsky downplays what he calls "loose analogies, perhaps in terms of figure and ground, or properties of memory." But the similarities are there, and they are more than "loose analogies." In my own research I have been particularly impressed by the structural similarities. For example, syntactic rules can be stated in terms of the same kind of network structures that are used in cognitive and visual representations in cognitive psychology and artificial intelligence (Lakoff 1977). And Johnson and I (1980) show coherences between the metaphorical structure of the conceptual system on the one hand and syntactic structure on the other. My own feeling is that cognitive science will best progress by transcending the independent modules approach and instead looking at the interdependencies and similarities among the various aspects of cognition.

by **John C. Marshall**

Neuropsychology Unit, University Department of Clinical Neurology, The Radcliffe Infirmary, Oxford, England

The new organology

Imagine a man who denied that "the moral and intellectual acts of man flow from understanding and will, independent of the body" (Hollander 1920, p. 232). Such a man might regard the brain as part of the body and conjecture that "the brain is the organ of the mind" (Hollander 1920, p. 214). This, in turn, might lead him to propose a research program in which psychology was to be rescued from metaphysics in order to become a branch of general biology (Lewes 1871). Pursuing an analogy between organs of the body and organs of the mind, our scholar would see little point in drawing too sharp an epistemological distinction between bodily organs, perceptuomotor systems, and cognitive faculties: "For sight and hearing are just as much psychic talents as are the different kinds of ideas" (Lesky 1970, p. 303). If, indeed, the maturation of, say, the visual system were analogous to the maturation of, say, the language system, the scientist would perhaps claim, as a general principle, that "the development of the mind of the child, far from being a mere moulding of it by the impression made upon it by its environment is . . . an unfolding of latent potentialities" (Hollander 1920, p. 249). The claim that the "elementary qualities of the mind are innate" would nonetheless require, of course, that these qualities must be "drawn out and cultivated" by the environment (Hollander 1920, p. 249). This viewpoint might lead one to expect that "the primary mental powers do not develop simultaneously" (Hollander, 1920 p. 249). One might rather expect to discover that "some develop early in life and speedily reach maturity, while others are late in coming into activity" (Hollander 1920, p. 249).

Our scholar could state the explanatory goal of his research program as follows: "We have to discover the fundamental powers of the mind, for it is only these that can have separate organs in the brain" (Hollander 1920, p. 238). The hypothesis that there are *separate* powers to be discovered would not, however, lead one to suppose that they cannot interact. On the contrary, one might expect overt behavior to be consequent upon the interaction of distinct faculties: "Indeed, we believe that the total nervous system is a combination of many; that all these individual systems differ in their office; that the offices are related to the development of the organs; that more or less of a bond, and therefore of reciprocal influence, exists between all the individual systems" (Lesky 1970, p. 309).

Continuing this line of investigation, one might suppose that the innate propensities and faculties are "unequally shared by the different species of animals" (Hollander 1920, p. 237). It could thus be the case that man alone possesses "the faculty of speech and unlimited educability – two inexhaustible sources of knowledge and action" (Hollander 1920, p. 246). Stated in more general terms, "the internal and external worlds" of different species will "vary to infinity, diminishing or increasing in the same proportion as the number of these organs diminishes or increases" (Hollander 1920, p. 237). "The external world," then, "is known only to the extent of our perception of it, which varies according to our own individuality" (Hollander 1920, p. 238). But it assuredly does not follow from this view that man is a prisoner of his biological endowment. On the contrary, one might plausibly argue that the proliferation of biological constraints protects man from manipulation by the environment: "The more complex the organization, the greater the choice, the greater the freedom" (Hollander 1920, p. 245). In this sense, one might claim that man, in principle, is more free than the other animals, for "all the facts quoted by metaphysics in favour of a free will are only met with in the conditions where the intellect predominates over the instincts and sentiments" (Hollander 1920, p. 245).

Speculating yet further, we might suppose that creativity itself is a basic human trait: "The same organ, which in the nightingale produces singing, in the beaver the faculty to build, produces correspondingly in man music, architecture . . . etc." (Hollander1920, p. 252). In short, "the arts and sciences were not invented because of the necessities arising for them, but because of our innate disposition" (Hollander 1920, p. 252).

Despite their obviously libertarian stance, men who argue in the above fashion have often been accused of denying the existence of free-will or of "explaining it away." Those who stress the biological prerequisites of meaningful experience are, for reasons unknown, peculiarly prone to such attacks. What defence could our scientist offer at his trial – a trial at which he is accused of propagating the materialist doctrine that "we are the instruments rather than the masters of our actions; we are slaves to our internal impulses" (Hollander 1920, p. 244)?

He might reply that "it is highly important to know how the soul operates in this life in its alliance with the body, and according to what law it acts" (Hollander 1920, p. 214). He could argue that "the most decided and undeniable experience teaches us that the brain, of all parts of the body, is the one by means of which the mind manifests its powers" (Hollander 1920, p. 214). When his judges proclaimed that the postulate of modularity denied the unity of the soul, he would simply ask "why should not the soul require for its various attributes structurally distinct parts of the brain" (Young 1970, p. 19)? If nature has constructed particular organs for seeing, hearing, salivating, producing bile, "why should She have made an exception for the brain" (Young 1970, p. 19)? When this failed to convince his opponents, he might point out that he was only studying the structure of the faculties and, as a good Aristotelian, the material not the effective causation of behavior: "The brain centres determine the disposition or tendency but not the actions themselves" (Hollander 1920, p. 245). Finally, he could quote Herder's remark "that force and organ are indeed most intimately connected, but not one and the same thing" (Lesky 1970, p. 307). And he might stress yet again that the activity he was pursuing was *empirical* science: "We, physicians and surgeons, limit our investigations to the facts cognizable by the senses, and leave everything else to the clergy and revelation" (Hollander 1920, p. 214). None of this would save him, especially so when he remarked that "the pervasive religious ideas of man and revealed religion would have been absolutely impossible if the human species had not been endowed with the appropriate nervous apparatus for having these experienc (Young 1970, p. 16).

The position I have outlined in the preceeding paragraphs is course, the *old* organology. All quotations and paraphrases have b taken from the writings of Franz-Joseph Gall. It is not too diffic however, to see the family resemblance between Gall's work and t new organology of "Rules and Representations."

In 1802 Gall was thrown out of Vienna by decree of Emperor Fran I; Pius VII excommunicated him in 1817 and placed his books on t *Index;* the medical establishment of Europe denounced him as charlatan or worse: "Mankind must revolt when it hears tha preacher of fatalistic theories promulgates teaching which would abhorred even by the most savage people without morals and religi (Walter 1805). It is not, however, without interest to note that ev major position for which Gall argues has become a commonplace modern neuroscience. (It is, of course, a vulgar error to suppose t Gall was primarily a craniologist: "They call me a craniologist, and science which I have discovered, craniology. I rather think that the w men have baptised the child before it was born. The object of researches is the *brain*" (Hollander 1920, p. 344)).

In particular, Gall's central hypothesis – that cognition is modula has accumulated wide-ranging support. Studies of the psycholog consequences of local damage (Coltheart, Patterson, and Mars 1980) reveal patterns of deficit and preservation that fractionate t mind into categories far more specific than those that the ninetee century neurologists were prepared to conjecture; electrophysiolog recording suggests similarly that many areas of cortex are organi into specialized cytoarchitectonic fields within which cells respond t very narrow range of stimuli (Zeki 1978).

We would hope, however, that the new organology will go sign cantly beyond the old and raise questions that could not even formulated within the conceptual framework of nineteenth-cent investigations. In what ways is this hope being realized? Young (19 p. 29) writes: "If one asks what Gall had to say about *how the br functions* as opposed to *what are the functions* of the brain, he h little to offer." Young's assessment is perfectly fair. It is here t recent work in linguistics has indeed progressed beyond the boun aries of Gall's organology. As Chomsky notes, core grammar aims provide an "abstract characterization of the properties of cert physical mechanisms, as yet almost entirely unknown." Yet the stat of the principles of core grammar continues, I suspect, to worry eve those psychologists and neuroscientists who are quite prepared endorse Chomsky's "realist" philosophy of theoretical constructs. L us take the example of WH-movement. Free movement of WH-phras into COMP, subject to the subjacency principle and interacting with t successive cyclic application of movement rules undoubtedly provid a rich account of question formation. The theory as a whole incorp rates principles of mental computation that show promise of be "genuinely explanatory in that they unify a variety of . . . generalizatio and ground them in a system that has a certain degree of deducti structure" (Chomsky 1978, p.16). So far so good. But very natural by virtue of their professional background, psychologists must b concerned with the role of the grammar in the perception a production of language. The grammar is not in and of itself characterization of these abilities, and has never been claimed b Chomsky to be so. The failure of the derivational theory of complex (and the analysis-by-synthesis algorithm upon which that theory w based) has regrettably led psychologists to ignore, almost totally, t developments that have taken place in linguistics over the last decad This can be seen most clearly by looking at any recent textbook o psycholinguistics. One consequence of this neglect is outlined b Chomsky: It does indeed appear that many "psycholinguisti phenomena that have been uncovered in recent years have little o nothing to do with the properties of the language faculty. The phenom ena are no less interesting for this reason, but their interpretation ha typically been chaotic and *ad hoc* to the point of despair.

Much current work in "artificial intelligence" thus conflates prope ties of the language faculty (e.g. the structural configurations tha determine sentence-internal anaphora) with, for example, one's know edge of the appropriate way to behave in restaurants. The *a priori* likelihood that the intersection of these sets constitutes a theoretically coherent domain is not great. Similar considerations apply to the brief vogue that "generative semantics" enjoyed in the eyes of psychologists. That any "performance datum" will necessarily reflect much knowledge and skill over and above the knowledge represented in linguistic theory misled psychologists into accepting that all human knowledge should be encoded into a monolithic structural derivation. The "psychological reality" of the overt phenomena was thus confused with a theoretical claim about the distinctness of different knowledge sources. The related vogue for "case grammar" in studies of language-acquisition was based on an even more disastrous confusion. From the fact that one often wants to know "who did what to whom with what," it surely does not follow that the primary categories of the grammar must be "Agent," "Object," "Instrument," and so on (see Smith 1979). It requires more than fiat to equate the child's ontology with the syntactic categories that are available for the expression of thought. And it is thus hardly surprising that so much of the traditional language-acquisition literature has so signally failed to move beyond superficial description of what the child typically says at age n, n + 1, n + 2, and so on. Contrariwise, when the distinction between language and effective communication has been honoured, significant results have emerged; the work of Dennis (1980), in which she shows that the parsing principles of isolated left and right hemispheres are quite distinct, is a case in point. Such findings in no way rule out the possibility that, in the normal intact brain, the two hemispheres cooperate in assigning structural descriptions to incoming signals.

In the long run, however, the concept of distinct yet interacting systems will only make *practical* sense (and psycholinguistic sense) to psychologists once we can see, in principle at least, how the grammar might be incorporated within the performance machinery. Minimally, the grammar must constrain the class of mechanisms that could be postulated for parsing and producing utterances (Fodor 1978).

Contact might also be achieved at the level of surface filters. A well-known example is filter 20 of Chomsky and Lasnik (1977); the constraint *($_{NP}$NP tense VP) interacts with the on-line perceptual strategy of interpreting structures that can stand as independent clauses as main clauses of the construction. A set of analyses where the free operation of a useful heuristic gives the wrong result is thereby blocked by the filter.

The necessity of a psychological interlevel (in the narrow sense of "psychological") becomes even more apparent when we consider the relationship of the grammar to biology. All primary data are performance data; with the possible exception of "pure" anatomical studies, no investigation of the brain constitutes an exception to this fact. It follows that there will be no coherent explanation of "physiological" data that does not require us to "filter" the results through an abstract theory of a performance device that tells us what the software system is doing. Talk of single cells acting as line detectors is emphatically not a description of the visual system. A singlecell connected to perhaps 10^4 *other* cells is what the neurophysiologist is recording from, and the system whereby the organism computes a representation of a visual object escapes us (but see Marr 1976). One of the most striking examples of what happens when a systemic characterization is ignored can be seen in a recent paper by Ojemann and Mateer (1979). In this work the "syntax center" is located by electrical stimulation during craniotomy. Further comment is unnecessary.

I have discussed some positive aspects in which Chomsky's "biologism" is closely related to Gall's. These aspects are basically strategic. We should, I agree, use the metaphors of growth and environmental triggering and regulation when looking at language-acquisition. There is, I think, a significant analogy between the mechanisms of speciation and the ways in which, in core grammar, "a small change in parameters may lead to what appears to be a radical change in the resulting system" (Chomsky, target article).

A revealing example of such a mechanism can be found in Bard's model of zebra striping patterns (Bard 1977). The immunological analogy, on the other hand, I find less compelling (Adinolfi 1978). True, molecular recognition and antibody production is a "selectional," not an "instructional," process. But I am unaware of any deeper parallel between immune responses and "learning" in the central nervous system. Similar considerations apply to the putative parallel between classical (Mendelian) genetics and the conditions on formal structures that are captured in universal grammar (Jenkins 1978). Exploitation of this surface analogy really cries out for an analysis of what it could possibly mean to "reduce" linguistic theory in a fashion analogous to the reduction of classical to molecular genetics that has been such a conspicuous success-story of twentieth-century biology (Goosens 1978).

The problem, then, for Gall and for Chomsky is that none of the above seems to bring us any closer to biology in the *narrow* sense of the term. By "narrow sense" I mean a concern with the neurophysiological realization of the mechanisms in question.

In 1808, Pierre Flourens produced the definitive critique of Gall's theory. He paid tribute to Gall's outstanding achievements as an anatomist, and he was not totally unsympathetic to what Gall was trying to do as a "biological psychologist." He did, however, remark that Gall's works contained not "one word of *special anatomy,* of *secret anatomy,* of what might be called *anatomy of the Doctrine;* or, in other terms, as it would be expressed at the present day, of *phrenological anatomy.* . . . The anatomy of Gall's memoir is nothing but very ordinary anatomy" (Young 1970, p.25).

The current revival of studies in the anatomy of language (Galaburda, Sanides, and Geschwind 1978) is likewise concerned with ordinary anatomy. Transposed into modern terms, I take it that Flourens was asking for the anatomy of WH-movement. Such crude and naive "translationism" is widely regarded as either conceptually incoherent or empirically ridiculous. These feelings will cut little ice, however, until a viable alternative is proposed; some way surely has to be found in which results phrased in the languages of linguistics, psychology, and physiology can be made to bear upon each other. I am inclined to believe that Chomsky has posed *the* crucial questions in the biology of language. And it is precisely this belief that leads me to wonder what would happen if workers in related disciplines took Chomsky's biologism literally.

Acknowledgments
I am grateful to Robert May and to Mary-Louise Kean for their critical comments on an earlier draft of this paper.

by **Robert J. Matthews**

Department of Humanities, Cook College, Rutgers University, New Brunswick, N.J. 08903

Language learning *versus* grammar growth

Chomsky suspects that we may well misdescribe language acquisition when we call it "learning." In certain fundamental respects, he suggests, we do not really learn language; rather, grammar grows in the mind. I share Chomsky's suspicions: it seems to me very likely that future empirical research will force us to conclude that language acquisition is best described as the growth of a mental organ. But though I endorse Chomsky's words, I am not certain that we envision the same eventuality. In this commentary I should like to examine Chomsky's account of the distinction between language learning and grammar growth. The distinction, I shall argue, is nontrivial: believing that language acquisition is a matter of grammar growth rather than learning is tantamount to abandoning a rationalist account of language acquisition.

The question that interests me here is this: What sorts of discoveries about language acquisition should lead us to conclude that language acquisition is a matter of grammar growth rather than grammar learning? (I use the expression "grammar learning" rather than "language learning" in order to make clear that I don't think that the issue here has much to do with the characterization of linguistic capacity.) Chomsky does not address this question explicitly; however, his discussion of a related question (p. 13) suggests that if language acquisition is not to be characterized in terms of such processes as are typically associated with learning (e.g., association, induction, conditioning, hypothesis-formation and confirmation, abstraction and generalization, etc.), then it may well be that language isn't learned. His discussion of Peircean abductive procedures develops this suggestion. In effect, he seems to say that if language acquisition is a matter of generating hypotheses and then testing them against primary linguistic data (in conjunction with an evaluation metric), then it's learning; otherwise, it's growth. The basic difference between these two abductive procedures seemingly has to do with the role of experience in language acquisition: in the learning case, experience serves as evidence against which hypothesized grammars are evaluated; in the growth case, experience serves directly in the selection of the most accessible grammar. Quantitatively speaking, convergence on a target grammar is presumably much more rapid in the second case.

Although we know very little as yet about abductive procedures of this second sort, we do know quite a bit about procedures of the first sort, enough at least to know what kind of empirical evidence might bear on the claim that language acquisition instantiates such procedures (cf. Pinker 1979). Indeed, we know enough so that if this is what we mean by "learning," then language acquisition is not learning. The reason is that, given both the complexity of the grammars attained and the poverty of the data on the basis of which grammars are attained, learning can be achieved only if there are *very* severe constraints on the form of acceptable grammars (cf. Matthews 1979). In fact, these constraints are so severe as to render grossly inappropriate any characterization of the acquisition process in terms of hypothesis-testing. Data serve not so much as evidence for a hypothesized grammar as they serve as a series of triggering events that determine the course of grammatical development. Chomsky apparently has something like this in mind when he talks of "grammar growth": universal grammar narrowly constrains the category of attainable grammars, but with parameters that remain to be fixed during language acquisition. Differences in acquired language manifest differences in the fixed values of these parameters. Preference for talk of "grammar growth" thus reflects basic assumptions about the role of experience in language acquisition. These assumptions are rationalist inasmuch as this account of the role of experience presupposes that the learner comes to the acquisition task with extremely rich innate linguistic structure.

Although I do not object to Chomsky's decision to mark his rationalist commitment by the expression "grammar growth," I do believe that there is a more straightforward sense in which language acquisition may turn out to be a matter of grammar growth rather than grammar learning. The straightforward sense of the distinction, like Chomsky's, has to do with the vocabulary in which a theory of language acquisition will be couched. Just as empiricists assume that an adequate theory of language will be framed in terms of notions of association, induction, generalization, and so on, so too have rationalists made particular assumptions about the appropriate vocabulary for an adequate theory of language acquisition. Specifically, rationalists assume that the same vocabulary used to characterize the structures attained in language acquisition will be suitable for characterizing the actual attaining of those structures. This extremely strong assumption is effectively embodied in the condition of explanatory adequacy that is levelled against linguistic theory; however, it is most clearly manifested in the abductive accounts of language acquisition that Chomsky employed in the past (cf. Chomsky 1965) but now rejects for other reasons. Such accounts were supposed to "represent a hypothesis about the innate intellectual equipment that a child brings to bear in language learning" (Chomsky 1962, p. 530). In employing these accounts, Chomsky committed himself to the assumption that an adequate theory of language acquisition would employ the intentional idiom of knowledge, belief, intention, and the like as its theoretical vocabulary. For, on these accounts, the learner is characterized as knowing (innately) certain principles, as selecting certain hypotheses from within the class of possible grammars, as testing those hypotheses against primary linguistic data, as choosing the optimal grammar from among those compatible with the data using an evaluation measure, and so on. Of course, Chomsky did not deny that there are any number of other descriptions of the development of linguistic capacity in the child. But he apparently believed (hypothesized) that the *appropriate* vocabulary for a theory of language acquisition would characterize the relevant states and processes of the learner in intentional terms.

Chomsky now seems willing to abandon the hypothesis-testing idiom of these earlier accounts; however, this should not be construed as an abandonment of the intentional idiom itself. For he still believes that the innate structure (or schematism) necessary for language acquisition can be characterized intentionally in terms of what the learner innately knows (or cognizes): a system of universal grammar is a set of highly restrictive principles cognized by the learner. Thus, for example, if on the basis of linguistic research Chomsky hypothesizes that all natural languages satisfy his structure dependency principle, and if he further hypothesizes that this fact about natural languages relfects a constraint imposed by the innate structure of human linguistic capacity, then on his rationalist account of language acquisition, he will characterize the learner as standing innately in a certain relation to the content "that all natural languages satisfy the structure dependency principle." Whether this relation is appropriately characterized as one of *knowing* is, as Chomsky says, of little interest here. The crucial point is this: innate structure relevant to language acquisition is characterized intentionally in terms of both the *content* of a state and the learner's *relation* to that content (e.g., knowing that p, hypothesizing that q, etc.); the various processes and state-changes thought to characterize the acquisition process are defined over such contents. The rationalist's insistence that it is content rather than mechanism that is innate expresses a commitment to the intentional idiom as providing the appropriate characterization of innate structure (cf. Matthews 1979).

It seems to me quite possible that, when functionally interpreted, the intentional idiom characterizes the etiology of behavior at a level of abstraction appropriate for capturing the behaviorally relevant similarities common to the possibly disparate physiological processes that eventuate in a single type of behavior. It is surely some measure of the appropriateness of this idiom that explanations and predictions of folk psychology – our only marginally successful psychology to date – are couched in it. But why assume that this is the appropriate level of abstraction for a theory of language acquisition? Although being in certain intentionally characterizable internal states may be relevant to an organism's behaving in the way that it does, why suppose that there is an intentional characterization of how the organism comes to be in those states? In particular, why assume that there is such an account for language acquisition? We don't suppose that there is an intentional description of the construction of a mechanical chess-player just because the behavior of this device is best explained and predicted in intentional terms. Proponents of intentional accounts make much of the intentional character of folk psychology, but folk psychology is notably silent on matters of learning. For all that folk psychology tells us, "learning" might be more akin to muscle-building, which is to say that a theory of "learning" would employ a nonintentional, presumably physiological vocabulary, describing learning as the growth of a mental organ under suitable conditions of sensory stimulation. In such an eventuality, talk of "grammar growth" would be quite appropriate, since the characterization of linguistic development would be of a piece with characterizations of the development of various organs. But, in so characterizing language acquisition, one would in effect be abandoning a rationalist account of language acquisition. This in itself is probably sufficient to insure that my notion of "grammar growth" is not Chomsky's. But if it is not, one must wonder why he finds his notion appropriate. More importantly, why does he find it plausible? Surely nothing in his present paper contributes to this finding.

by **James D. McCawley**

Department of Linguistics, University of Chicago, Chicago, Ill. 60637

¡Tabula si, rasa no!

Chomsky has often argued that treatments of the human mind as initially a blank slate have not done justice to the mind. However, they have done even less justice to slates. No slate (or paper, or magnetic tape, etc. – I will use "slate" to refer indiscriminately to all media for writing, since the physical substance is irrelevant to the analogy) is wholly unstructured. Blackboards provide two dimensions, while stacks of transparent plastic sheets provide three, and punched tapes, only one; slates differ with regard to whether erasure is practicable (with blackboards and magnetic tape it is, with clay tablets and punch cards it isn't); and they differ with regard to whether the slate provides devices that facilitate the writing of particular kinds of messages, as do the rulings on graph paper or the staves on music paper, or for that matter, the questions and spaces for answers on a dog license application form. The idea of a completely unstructured slate may indeed be incoherent, in that slates are supposed to provide not only a medium on which messages can be written, but also a medium off of which messages can be read; you might try to write a message on an infinite-dimensional vacuum if you could find one, but no one would be able to read it.

In a conception of the mind as involving a slate, the interesting issue is not whether the slate is ever blank, but what the structure and functions of the slate are. What determines what is entered in the slate and where it is entered on it? Does the slate have different components that differ from one another in internal structure? How are existing entries on the slate used in cognition, action, and further learning?

There is of course a more basic question: namely, does the mind involve a slate at all? In this target article Chomsky speaks as if it does not; his remarks, however, turn out to relate to a different issue. Chomsky expresses serious reservations about the notion of "learning." ("It is rather doubtful . . . that there is much in the natural world that falls under 'learning,' so conceived, if the metaphors are taken seriously"; "It is possible that the notion 'learning' may go the way of the rising and setting of the sun.") He suggests that there is no real difference between learning and growth, and he approvingly quotes Jerne's conjecture that supposed instances of learning may turn out not to be "instructive" changes (in which "a signal from the outside imparts its character to the system that receives it"), but rather "selective" changes (in which "some already present character is identified and amplified by the intruding stimulus"). Selection, however, is only one aspect of learning – and, to my mind, the least interesting aspect of it. Selection (generally called "reinforcement" by those whose accounts of learning have dealt with it) may provide an account of why a person entertaining a particular hypothesis will retain or reject that hypothesis, but it gives no insight into what I take to be the two biggest questions about learning: how does a person come to entertain the hypotheses that he does, and what happens to a hypothesis once selection has operated in its favor – i.e., how is it incorporated into the person's repository of knowledge (= "slate")?

At various places in this target article and earlier works, Chomsky touches on these questions but stops far short of answers to them. He maintains that one's innate language faculty heavily constrains what hypotheses are entertained, but he offers no hint as to where the language-particular hypotheses come from. While an innate language faculty might well make available both the hypothesis "object precedes verb" and "object follows verb," with selection (= "reinforcement") deciding which one wins, it is implausible to suppose that it makes available to the learner a range of hypotheses as to the meaning of *assassinate* that includes "kill in order to remove from political power." His statement that "grammars are internally represented in the brain" appears to make reference to a component of a mental slate, perhaps in a form analogous to a pad of dog license application forms, providing a checklist of features on which languages can differ, with a separate sheet to be filled out for each language that one learns. However, the programmatic accounts of language acquisition that appear in Chomsky's works (e.g. 1965; 1975a) deal only with the end-product of language acquisition and have nothing to say about the developmental steps that would lead to that end-product – in particular, the steps in which entries are made on the slate. Selection surely is involved in language acquisition, but that isn't saying much.

Chomsky wonders whether we can "distinguish learning from growth in terms of the state attained." We can, since learning and growth individuate differently. If one is given appropriate exposure to French, Flemish, and German, one develops command of all three languages but does not develop three larynxes or three pairs of ears. Your genes fix in advance the number of organs of each type that you'll develop, but they don't fix in advance the number of bodies of knowledge (e.g. languages) that you'll acquire through the use of each "mental organ." The possibility of acquiring several bodies of knowledge of a given type is the clearest evidence that I know of for the proposition that the mind involves some sort of slate.

Chomsky asks, "Is the mind organized into distinct cognitive faculties with their specific structures and principles, or are there uniform principles of learning, accommodation, assimilation, abstraction, induction, strategy, or whatever, that simply apply to different stimulus materials to provide our knowledge of the behavior of objects in physical space, our knowledge that certain strings of words do or do not have certain meanings, and so on?" Here he sets up a false dichotomy: there is no reason why the "modules" that Chomsky regards human cognitive capacities as being composed of should correspond in a one-to-one fashion to "cognitive domains." Chomsky's analogy between cognitive capacities and bodily organs indeed suggests that among the cognitive modules there could well be some that have communicative or organizational functions analogous to those of the circulatory system or the nervous system. The "learning theories" of behaviorist psychologists have been implausible largely because they were supposed to accomplish the whole of learning without assistance from anything other than the sense organs; such theories had the absurdity of a theory of food preparation that posits a Cuisinart and a well-stocked refrigerator but no cook. General-purpose "cognitive modules," such as a general mechanism for the construction of gestalts, are plausible if their action is directed by other cognitive modules, even in such a trivial fashion as that in which each cognitive module may direct the gestalt-former to look for specific types of factors as components of the gestalts. Chomsky is willing to admit the possibility of general-purpose cognitive faculties to deal with "domains in which the mind is equipped with no special structure to deal with properties of the task situation." If we have cognitive faculties that are not restricted to particular cognitive domains, I see no reason why they would turn themselves off when language is being acquired.

I will conclude this commentary by taking up briefly Chomsky's remarks on "psychological reality" and "psychological evidence." Chomsky regards these terms as objectionable, in that they misleadingly suggest that there is some special kind of reality that some linguistic analyses have but others do not, and that there is a privileged kind of evidence for linguistic analyses. For Chomsky, any theory or grammar "carries a truth claim" and "in each case we have evidence – good or bad, convincing or not – as to the truth of each of the theories we are constructing"; "psychological evidence" does not testify to any special sort of reality of the theory. This is fine, except that in many cases it is unclear what truth claims, if any, are embodied in particular analyses, or whether there is any sort of reality, psychological or not, that those analyses purport to have. For example, it is not clear what Chomsky and Halle (1968, pp. 233–4) are claiming when they maintain that the word *right* has the underlying form /rixt/. Chomsky and Halle indeed offer evidence in support of this conclusion, but the observations that they cite count as evidence only by convention. Their ingenious argument for *right* = /rixt/ is based on a fact about the pronunciation of *righteous* and is relevant to their conclusion only under the assumption that the relationship between *right* and *righteous* is such that the underlying form of the former must be a constituent of the underlying form of the latter. Transformational grammarians uncritically accept each other's judgements of relatedness among words and share a policy of assigning common underlying elements to related words. Chomsky and Halle's premises, as they stand, tell us not about any English speaker's linguistic competence but about what transformational grammarians are willing to let each other get away with. Linguists, especially transformational grammarians, frequently commit a sophisticated version of the error that Chomsky castigates Searle for: that of taking the "intuitive plausibility" of rules and structures as support for them, with the transformational grammarian's intuition in place of that of the speaker of English. Psychological evidence is essential if one is to know how to interpret putative linguistic facts and whether to take them seriously.

***by* Julius M. Moravcsik**

Department of Philosophy, Stanford University, Stanford, Calif. 94305

Chomsky's radical break with modern traditions

This target article, and the book from which it is extracted, represent what is perhaps Chomsky's philosophically richest writing. Though Chomsky's views have been discussed by social scientists and philosophers for more than two decades, the central ideas have not been adequately understood, and the opposition is often put forward dogmatically, without much empirical support or conceptual argument. In these brief remarks I shall attempt to show just how radical Chomsky's departure is from key modern traditions, both in philosophy and in the social sciences. Chomsky differs from philosophers and scientists in that he argues for what I shall label a "deep" theory of mind, while all other practitioners opt for what shall be labelled "shallow" theories of mind. Chomsky differs from analytic philosophers in his conception of the relationship between science and philosophy. Finally, he differs from philosophers, since his main interest lies in a theory of understanding, while analytic philosophy has been preoccupied with problems of propositional knowledge and the analysis of empirical evidential relations.

"Deep" and "shallow" theories. I shall label as "deep" (without implying any depth in a normative sense) the theories that refer to many layers of unobservables in their explanations, and I shall regard even some of the fundamental facts to be accounted for as lying beneath the level of observability. Such theories are guided by the intuition that the observable appearances can be explained adequately only by the examination of the underlying unobservable aspects of nature. ("Nature does not wear its essence on its sleeves.") What I label "shallow" theories are those that try to stick as close to the observable as possible, aim mostly at correlations between observables, and posit something unobservable only when this seems unavoidable – even then, such theories demand some direct relationships between the observable and the unobservable.

Needless to say, the applications of this distinction depend on historical context, and the dichotomy admits of plenty of borderline cases and "matters-of-degree" judgments.

The history of the natural sciences like physics, chemistry, and biology is a clear record of the success story of "deep" theories. The more sophisticated and complex the underlying system of unobservables becomes in physics or chemistry, the more we seem to be able to account for. In fact, even the application to the practical – i.e. the rise of technology – was made possible only after the considerable "deepening" of physics and chemistry. Earlier in this century the positivists tried to establish rules that would relate the unobservable to the observable in a legitimate scientific theory. It turned out, however, that these rules could not be used to describe even such obviously legitimate sciences as modern physics. Today, the effort has been largely abandoned.

When we come to the social sciences, we encounter a strange anomaly. For while there is a lot of talk about aiming to be "scientific," one finds in the social sciences a widespread and unargued-for predilection for "shallow" theories of the mind; e.g., on both the introspectionist and behaviourist account of the mind, its contents are open to direct observation by the agent and – on the behaviourist view – also by the investigator. There is a reluctance to refer to unobservables, at least as far as complex mechanisms, obeying abstract principles, are concerned. The one notable exception might seem to be Freud; but even in his case, it is better to talk about the positing of unobservable forces, rather than complex mechanisms with clear mathematical structure.

In the face of this widespread agreement on the preferability of "shallow" theories, Chomsky argues unhesitatingly for a "deep" theory of the mind. His argument rests on the analogy between theories of physics and the other successful sciences and what one should try to accomplish in the social sciences. If "deep" theories have been the successful ones in the natural sciences, why should one not expect the same to hold for the social sciences?

Though on the modern scene, Chomsky is alone in advocating a "deep" theory of the mind, it is worth pointing out that this approach has its roots in classical Greek thought. In the *Republic* Plato raises the question of how one should distinguish two allegedly different mental capacities. His answer avoids saying anything about introspective or behavioural data. He claims that an adequate answer must lie in drawing careful distinctions between the respective objects of the mental capacities. This view underlies his early version of innatism (the so-called theory of recollection) and his view that deep self-knowledge is possible only indirectly. We do not know directly the rational contents of our minds; only cooperative investigation can unearth these in the form of theories explaining the observed mental phenomena.

Given the unargued-for nature of the opposition to Chomsky, and of the preference for "shallow" theories of mind, it is worth speculating on the roots of this phenomenon.

Some of the roots are deep and are embedded in our religious and moral heritage. Many of the tenets of the Judeo-Christian tradition presuppose that a human can inspect directly the content of his or her mind. For example, the doctrine of repentance and asking of forgiveness for sins seem to presuppose such strong forms of self-knowledge. Again, the doctrine that intentions are what counts most in the eye of God, and the assumption that what counts most is something that we can have direct knowledge of, rest on the same presupposition.

The same direct form of self-knowledge, and hence a "shallow" theory of mind, underlies some of our moral practices. For example, this seems to be assumed by the practice of demanding that someone should promise to be a certain kind of person (faithful, etc.). How can one know that one will keep such a promise unless one can inspect in one's mind the relevant feelings and intentions?

Apart from these deep-rooted tacit assumptions in our culture, there is a motive for "shallow" theories of mind that is more closely related to one's view of the social sciences. Many social scientists think that their fields will ultimately turn out to be "reducible" to discourse about such material constituents as neurophysiological elements. Given this assumption, a "shallow" theory recommends itself, since if we have such theories – it is felt – the eventual "reduction" will be easier. Crude materialist assumptions suggest that it is all right to have a rich set of commitments to material elements, but that it is not all right to have such posits in the case of the mental.

Once we unearth these sources of the prejudice for "shallow" theories of the mind, we see how groundless they are. Given a "deep" theory of mind, we might have to revise some of the specific ethical practices that we embrace. But, in general, a "deep" theory of mind is compatible both with religious and nonreligious views – with a deontic view of morality or with alternative conceptions.

Similar considerations apply to the "materialist pull." A "deep" theory of mind is neutral with regard to the materialist-dualist controversy. Whether the abstract structures that Chomsky posits to explain a variety of cognitive competences can be reduced to discourse about "material" elements is an open, empirical question. But why should this question attract so much attention from philosophers and social scientists? The materialism-dualism controversy is vastly overrated in importance. Many seem to think that the thesis that humans have a soul, the thesis of immortality, or theses about the uniqueness of the species and hence grounds for human dignity, rest on the outcome of this debate. But the theses of there being a soul, there being immortality, there being something unique about human rationality, are all equally compatible with materialism or dualism.

Thus the resistence to "deep" theories of mind is more of a visceral reaction than a well-argued stance. It is also responsible for the resistance to Chomsky's "modular" view of the mind – i.e., his hypothesis that different structures underlie such cognitive competences as the mastery of a language, the ability to reason, and the capacity to calculate. The basis for Chomsky's preference is the analogy with biology. There we found fruitful the positing of different, highly specific structures to explain a variety of biological functions. The resistance seems to be based on the fact that if we have a "modular" view, then we will probably be committed to a "deep" theory of mind as well. Furthermore, it is felt at times that a uniform account of the mind is more elegant than a pluralistic account. But mathematical elegance may clash with biological feasibility. Biologically, it might be advantageous for the human mind to be a "mathematically messy" organ.

Chomsky and the analytic philosophers. Though the analytic philosophers share the predilection of the social scientists for "shallow" theories of mind, they are divided from Chomsky also in two further respects.

With the rise of specialization there came the demand to spell out what the special domain of philosophy is. We need not go through the many proposals that have been made. Quine (1960) has rejected the distinction between analytic, conceptual, and empirical truths; he has also maintained that philosophy and science form a continuum. The latter deals more with issues closer to the observational level, while the former deals with matters more removed from observation – but the distinction, according to Quine, is one of degree. Chomsky rejects, on the one hand, Quine's views on analyticity largely on the ground that rule-structures must play important and distinct roles in explaining cognitive competences. On the other hand, he agrees with Quine on the philosophy-science continuum. Hence, he holds a position different from that of all the main current schools.

In examining the structure of human cognitive competences, Chomsky is concerned mainly with aspects of human understanding. Modern philosophers have been preoccupied with analyses of what it means to know a proposition to be true, and with the analysis of the notion of viable empirical evidence. They leave questions about understanding to the psychologist. Given Chomsky's philosophy-science continuum view, one can see why he regards this as unsatisfactory. He insists – rightly, it seems to me – that there are important conceptual problems regarding notions like "understanding," "learning," "growth," "development," and so forth, and that the tools of modern epistemology are not adequate for dealing with these.

Conclusion. In these brief remarks I have tried to show how deep the differences are that separate Chomsky from the main trends in current social science and analytic philosophy. I have also tried to show that the opposition to his views rests, by and large, on unarticulated assumptions that are very deeply rooted in our culture. One can only hope that a better understanding of Chomsky's position, and the nature of the opposition, will lead to a shifting of the level of dialogue, to a more rational and better articulated plane.

by **Adam Morton**

Department of Philosophy, University of Bristol, Bristol BS8 1RJ, England

There are many modular theories of mind

Almost any sane psychological theory will account for behavior by reference to internal mental structure. But to say this is to rule out only the most naive *tabula rasa* empiricism or the most clumsy stimulus-response behaviorism. In particular, it does not commit psychology to a framework of discrete modular systems of concepts, whose interaction produces behavior. Chomsky presents an argument that our internal mental structure is indeed modular and that the mind has organs in much the way that the body has; he in fact presents some conjectures about what the modules may be. The argument consists essentially of a restatement of his reasons for thinking that language is based on such an autonomous conceptual system, and an imaginative extrapolation to the more general situation.

There is no doubt, as I see it, that there may be such modules; the idea is not incoherent. And there is now, after twenty years of campaigning by Chomsky and his followers, a presumption in favor of the idea that in language we have such a module. What Chomsky is now trying to do is to develop these insights into a general account of human capacity, of at least enough specificity to advise us what to look for when approaching nonlinguistic skills.

Human capacity breaks down into language, the system of concepts, sociality, musicality, and so on, apparently. But these modules are not at all basic; linguistic competence surely breaks down into phonetic competence, syntactic competence, semantic competence, at least. These things can clearly vary independently, though just as clearly they can only function normally in combination. This is typically the case, as we are discovering in cognitive psychology; spatial ability, for example, almost certainly consists of a number of distinct capacities, which might even occur in different degrees in different populations (Jahoda 1979; Serpell 1979). Any recognizable piece of behavior, or any skill as described common-sensically, will result from the interaction of a number of more primary skills. It is now unlikely, to the point of incredibility, that there should be, for example, primitive arithmetic or musical skills. If there are absolutely primary skills, their number must be very large and their characterization very theoretical. (A little reflection on the systematic but paradoxical disruptions of capacity produced by brain lesions will lead one in the same direction; Wilkes 1980.) And if this is so, then the skills that underlie distinct capacities may overlap. It seems likely that some syntactic competence is put to use in arithmetic and musical skill; there is some neurological evidence for this in the case of music.

What could one learn about the workings of the liver by studying the workings of the heart? Very little. What could one learn about the interaction of the respiratory and circulatory systems by studying the interaction of the nervous and digestive systems? The problem is that, to the extent that we think that the most profitable analysis of something is in terms of autonomous modules, we must think that what we learn from one module is unlikely to transfer in any easy way to another. In what ways are nonlinguistic capacities likely to require a treatment different from those involved in language? Some basic features of the Chomskian analysis of language have much less force when applied elsewhere. The underlying regularities may not be rules; they may not even be manipulations of representations. They are even less likely to involve the possession of implicit innate concepts. One prepsychological reason for doubt on this score is the fact that nonlinguistic skills typically come in degrees; there is not usually something that one simply does or does not have.

These remarks are an exercise in the separation of possibilities. Mental structure does not entail modularity; modularity does not entail the presence of rules and representations. The fact that language is both the best candidate for modularity and the best candidate for a representational mental framework does not tell us much about the nature of other skills. Chomsky gives rigorous arguments for describing language in terms of rules and representations, and heuristic arguments for modularity. But the two conclusions do not combine in the way he wants; if anything, modularity tells against the promise of language as a paradigm of human skill. I do not think that Chomsky has given us what we need in order to imagine a physiology of mind – a study of the interaction of essentially different mental components. To say this is neither to suggest that such a thing may not be found nor to deny the wish that someone find it.

by **John Morton**

MRC Applied Psychology Unit, Cambridge CB2 2EF, England

Language: levels of characterisation

The issue I would like to address is the separateness of what Chomsky terms "abstract characterisation" on the one hand and "physical mechanisms" on the other hand. The distinction is introduced early in the article, when Chomsky is establishing that his use of terms like *mind* and *mental representation* ". . .need in no sense imply the existence of entities removed from the physical world." It is a sad commentary on the quality of the current debate that this qualification has to be made, since such a position is entirely natural for a cognitive psychologist. The essence of the information-processing approach is to describe cognitive function without regard to the substrate. To discuss the "existence" of such functions would be as strange as talking about the *existence* of a particular program in a computer. It is simply not an issue. What is important is the way in which the functions described in a psychological model are implemented in the brain. But note that without knowing what the psychological functions are, we cannot ask how they are implemented.

Chomsky talks about our knowledge of language being represented "in structures that we can hope to characterise abstractly, and in principle quite concretely, in terms of physical mechanisms." It is not quite clear, from this, what form we should expect the latter to take, since shortly after, Chomsky continues "In the same way, a theory of human vision may be formulated in concrete terms, referring, say, to specific cells in the visual cortex and their properties."

There seem to be three possibilities here. The first is an account of human vision where the basic units are physiological. In such an account the stress would be on the responsiveness of single cells, their interconnections and projections, and perhaps their structure. The second possibility would talk about the implementation of the psychological formuation in neural terms. This would include phrases such as "Function F is located in area A" or "It would be possible for function F to be realised in neural terms in the following way. . . ." The third possibility would be the reverse of the preceding one. That is, it would take a neural unit and describe its role in psychological terms. A typical statement in such an account would be "Area A is involved in functions F, G, and H." The extent to which these three possibilities overlap, with respect to what counts as data, seems to depend critically on the extent to which the psychological functions are localised. It seems possible that some simple visual functions might be localised, but it appears to be unlikely that any useful localisation of language function exists (Zangwill 1978). For language function, then, the accounts are likely to be distinct. In any case, a proper psychological theory is insulated from such considerations, since its justification will be, in Chomsky's words, "by success in providing insight and explanation."

One might note here that the use of behavioural data from brain-damaged patients does not of itself constitute a bridging of the gap between the levels. Thus I am aware of no work in which knowledge of the location of a lesion has played a role in the evolution of a psychological theory. For example, Bradley et al. (1979) compare data from normals and agrammatic patients and conclude that there is a separate lexicon for function words. They also postulate that this lexicon is in the left hemisphere. However, none of their strictly psychological arguments would be affected if it in fact turned out to be in the right foot.

It should be clear that the separation of levels I am discussing is neutral with respect to Chomsky's claim that linguistics is a part of psychology. If we are to accept this, then it should follow that psychologists should be able to provide evidence to decide among linguistic theories. It would be interesting to discover what such evidence might look like. Previous attempts seem not to have been too successful, and one recent move to support the claims of a particular grammar (Bresnan 1978) seems to me to have mistaken a computational device (A.T.N.'s) for a psychological model [see Arbib and Caplan: *BBS* 2(4) 1979]. Of course, if linguistics is actually more abstract than psychology, and is better to be considered at a different level, then neither can produce evidence crucial for the other, any more than physiology can decide between psychological models or vice versa. The proper questions would then be of the form "which psychological functions are responsible for linguistic function L?" We will discover, of course, that not only will linguists disagree as to the nature of the relationship between linguistics and psychology, but also, the kind of theory they produce will differ. The mode of interaction of psychology and linguistics, then, would depend crucially upon the level of abstractness of the particular linguistic theory being considered.

by **Howard Rachlin**

Department of Psychology, State University of New York, Stony Brook, N.Y. 11794

Cross purposes

With a little semantic revision ("instinctive behavioral patterns" instead of "mind" for instance), Chomsky's brief discussion of learning of grammar (p. 13) might have come from Staddon and Simmelhag's (1971) behavioristic discussion of "learning" and evolution. But the parallelism is illusory. Prior to this discussion, one encounters Chomsky's response to those critics who have claimed that his grammar "has no psychological reality." Chomsky says that he never meant to distinguish "psychological reality" from what constitutes good linguistics in the first place. A behaviorist would also deny the distinction. A grammar, to a behaviorist, is a theory of behavior. But for the behaviorist the grammar is the theory of the linguist. For Chomsky, the grammar is not only the theory of the linguist, it is also represented, as an organ, called a "mental organ" but real, like the heart, lungs, liver, eyes, held captive *inside* the speaker. This assumption (the physiological justification of which is claimed to be far in the future) is what behaviorists are likely to find unacceptable. Chomsky seems to feel that the facts call for an extreme degree of nativism with regard to language, and that nativism, in turn, requires the concept that the language faculty is a "mental organ." A psychologist, even a nativist, even a cognitivist (let alone a behaviorist) would begin with different assumptions.

At first I suspected that "mental organ" was meant to be a collective name for the areas of the brain known to be necessary for the control of speaking and understanding speech. But Chomsky seems to feel that these areas contain something beyond what (little) we know about them. The language "organ," as Chomsky describes it, seems intended to embody inside the organism, in concrete form, whatever is important in language, structurally *and* functionally. Why is it necessary to (so to speak) hold a behavioral function hostage in the human body? Where does this get us? Do we need to fear that this imprisoned function (in the case of language) will escape and lodge itself in a dolphin or a chimp or even, God forbid, a pigeon? Our bodies themselves are organized in a symmetric way and presumably have evolved that way. Do we then have to have an "organ of symmetry" inside us to explain the way our bodies are? [See Corballis & Morgan *BBS* 1(2) 1978.]

If not, then it cannot be the *innateness* of language that prompts the postulation of an organ for it; nor can it be the fact that language has a complex organization. These things language shares (or might we share) with walking, bicycle riding, genuflecting, and many other functions. To understand better what Chomsky is driving at, I tried to imagine some behavior that people might agree has a large innate component where (unlike the various manifestations of language) there is clearly little variation among cultures. Perhaps, *walking* will do as an example. (There are those who would argue that the common nature of walking is due to common shaping of the environment and common reinforcement for locomotion, but let us ignore such arguments and agree that walking is a largely innate behavior.) There could be constructed a grammar of walking, perhaps not as complicated as grammars of speaking but complicated enough. Certainly each instance of walking is different in some respects from all others elements can be replaced by other elements; limps, skips, hops, struts and other variations have their own regularities yet fit within a general pattern. A given environmental demand (say a low wall) brings forth unique solutions that yet fit within the "grammar." A mime might construct syntacticly correct walking without semantic content (without getting anywhere) corresponding to similar constructions (jabberwocky) of language. Although, to my knowledge, there are no written grammars of walking, there is no reason I can think of why the job, in principle, cannot be done. In addition, in the brain there are structures that, if damaged, will impair walking.

Granting all this, the facts still do not compel the postulation of a physiological "module" that contains somehow all that is or can be important about walking. Perhaps the reason Chomsky proposes such an all-inclusive organ for speech is what he calls "the impoverished environment." This phrase is repeated several times but remains unexplained. Impoverished with respect to what? In what way is the stimulus poor? And how is such an "impoverished" stimulus supposed to "fine-tune" behavior (Chomsky refers to all behavioral shaping such as that which causes differences between Persian and English as "fine-tuning")? The stimulus in question is other people talking and acting. Except perhaps in the case of a poet or linguistic stylist, our linguistic environment seems at least as complicated as our linguistic behavior. Certainly all children (and adults probably) hear or see more sentences than they speak. If by "poverty" is meant merely the fact that I speak certain *particular* sentences that I have not heard, then that is also true generally in my behavior. I might whistle nondissonant melodies that I have not heard, or build a house different from any I've seen, or put on my pants in the morning in a new way, or (to cite a social action like language) relate to a friend in a unique way; all of these conform to the rules governing these behaviors, but not to any specific instance I've seen or heard before. Does this mean that my environment is impoverished? And even if this new (and utterly useless) definition is given to the word "impoverished," does it mean that there is an organ inside me corresponding to these activities? No, it does not. If a stone obeys the law of gravity, will we find the law of gravity "encoded" somewhere inside the stone when we break it open? (The best argument I have seen against such physiological mythologizing comes from Fodor (1975), who in other respects seems to be in Chomsky's hemisphere.)

The postulation of an organ for language in this literal way leads to all sorts of difficulties. "Philosophical" questions that, since Wittgenstein, have been answered by psychologists with "Who gives a damn?" are dredged up again. "May a man know English while he is sleeping?" is one we are asked to consider. Well that, of course, depends on what you mean by "knowing English." If we find it useful (as we do) to say of a person who intermittently speaks English that he "knows English," then a person who is sleeping may know English. If a person speaks English at time A and it is now time B (say, one hour later), we may want to say provisionally that he knows English, and that if and when he comes to speak English again, he will have "known English" in the interim. And that if he dies right now, we will never know for sure (and never care) whether he could speak English at the very time of his death. What is more (or less) mysterious in this respect about "knowing English" than "knowing how to dance?" unless, with Chomsky, one arbitrarily permits an organ inside to play music for one and bans such music for the other?

There are other difficulties: Chomsky says, "Two people might share exactly the same knowledge but differ greatly in their capacity to use it." Here he takes what is essentially a *temporal* difference and makes it into a structural (really, in the absence of physiological content, a spiritual) difference. Knowledge of English obviously involves use of English over an extended time period, whereas impaired capacity to use English involves disability over a smaller time period. To identify such dispositions with structures, like personification of the deity, may serve a psychological purpose but not a logical one.

Similarly, we have the child knowing English grammar but speaking through a "filter" that passes only content words. Where is this filter? Is it outside the child? If not, then do we have another child inside the first child speaking through the filter located in the outer but not the inner child? This terminological swamp is where we arrive by identifying behavior with organs of the body.

As far as actual data cited in Chomsky's target article are concerned, one would have hoped that, after twenty years of research, some could be cited instead of the catalog of possible future experiments (p. 6) and coy references to "not entirely fanciful" data.

On the basis of these not-entirely-fanciful and presumably not-entirely-real findings, a "theory of the mind" is proposed. And, given their lack of observational content, organs of the mind may be juggled at will. Perhaps in a "fuller theory of the mind" there is a mental organ for "computation" and one for "conception." Why not? And a really full theory of the mind will undoubtedly include organs for all behavioral dispositions from amativeness (Gall's first area) to awe (his thirty-eighth). And why should they not interact with each other? From this point on, any scenario is possible. Of course, "mere" behavior tells us "little or nothing" (p. 6, p. 11) about these scenarios; they are entirely divorced from behavior, entirely hidden. Where would one begin to do empirical work on mental states about which behavior is said to be irrelevant?

And so on. To Chomsky there is no comparing of speaking to bicycle-riding, because language has cognitive rules whereas bicycle-riding is merely a "skill." But the *point* of the comparison is that there is no more evidence for the existence *wholly inside the speaker* of cognitive rules for speaking than there is evidence for the existence wholly inside the bicycle rider of cognitive rules for bicycle-riding. Chomsky does not deal with this point. He just denies it.

Why such lack of contact? Why such contempt for the other side? ("mere behavior . . ." "It is a sad commentary on the field . . .," ". . . there is really nothing to discuss.") It is more than "structuralism" vs. "functionalism." Perhaps the difference comes down to one's objective in the complicated network of observation and inference that must constitute any theory of the mind; the behaviorist views inference as useful only in going from observation to observation, whereas Chomsky seems to view observation as useful only (if at all) in going from inference to inference.

by **Bernard E. Rollin**

Department of Philosophy, Department of Physiology and Biophysics, Colorado State University, Fort Collins, Colo. 80523

Innate and *a priori*

In the course of his innovative discussion, Professor Chomsky seems to suggest that, in addition to providing a basis for a new and fruitful approach to empirical psychological research, he is solving, dissolving, or recasting some traditional problems of knowledge and metaphysics. For example, it appears that Chomsky is suggesting that the traditional dispute between empiricism and rationalism, arguing whether there is any knowledge that is *a priori* and synthetic, (i.e., that tells us about the world but is not grounded in experience), is resolved by the empirical results of his linguistic theory.

What Chomsky has shown, persuasively in the case of language, is that there is a set of rules, presumably genetically determined and thus innate, that determine our knowledge or "cognition" of language, and that can empirically be shown to be universal (i.e. to hold across all known languages). His arguments from the poverty and diversity of the stimulus, and from our ability to generate an infinite number of sentences, are certainly plausible, and his claims about the universality of these rules across human languages is empirically testable. His theory does suggest various lines of empirical research that have been and will continue to be fruitful in understanding human language. Thus far, we may admit that Chomsky has given us empirical evidence for the claim that knowledge of language is, in his sense, innate and in traditional terminology also synthetic – i.e. informative about some aspect of reality.

He further suggests that other cognitive systems – mathematics, or our concepts of physical objects – may also be innate, and he asserts that they might be fruitfully studied along the same lines as language. It is not clear, however, that Chomsky has given us reason to believe that the innateness hypothesis is true of these other systems – he has presented neither empirical nor conceptual reasons for supposing that what is true of language is true of other faculties. The argument from poverty of the stimulus, for example, is simply not persuasive when applied to our knowledge that a typewriter won't fly – simplicity seems to dictate that we can explain this strictly empirically. Nor does there seem to be an analogue to the claim about language that empiricism cannot explain an ability to make infinite use of finite means, or generate new types of sentences, and so on. It is also difficult to imagine any empirical research that could test the innateness hypothesis with regard to our knowledge of physical objects, since we experience the properties of objects as soon as we begin to experience. There exist plausible accounts of the development of our knowledge of objects that do not require that we postulate innate structures. Language is clearly a rule-governed activity, and so it is plausible to inquire after the sources of the rules. On the other hand, our knowledge of objects does not appear to be rule-governed in the way language is; there is no obvious "grammar of objects," so the burden of proof is on one who hopes to treat them analogously.

It is worth recalling, as I have shown elsewhere (Rollin 1971; 1976; 1978), that innateness by itself poses no necessary threat to the basic tenets of the epistemology of traditional empiricism – it is *a priori* synthetic truths rather than innate ideas that are anathema. But, in addition to the innateness hypothesis, there seems also to be in Chomsky's argument a metaphysical/epistemological claim, to the effect that it is by way of innate structures that we must experience or know the world; that they are constitutive of experience in a given dimension and thus cannot be grounded in experience, and as such cannot sensibly be said to be refutable by experience. Thus experience depends on them, rather than their depending on experience, and so his claim seems to make them *a priori* and synthetic in Kant's sense, as well as innate. But Chomsky's *a priori* synthetic is markedly different from Kant's, in that while Kant claims that the same *a priori* synthetic categories he deduces can be shown transcendentally to be necessary for any being with an ectypal intellect (i.e., any being at all who gets his cognitive material from "outside" himself, and must synthesize it), Chomsky has no such argument for the universality and necessity of his "categories" or rules.

If we take Chomsky's clearest example, knowledge of language, it seems to be merely a contingent, biological fact, rather than a logical requirement, that this is the shape language must take! There is no reason given that shows that language could not appear in some other form. There is no reason to believe that there could not be creatures, even humans, with other innate structuring principles, perhaps also biologically developed as necessary or accidental results of selective evolutionary pressures. For that matter, one can imagine some new radiation in the atmosphere that would cause changes in the brain and would change one or more of the rules and our knowledge of the rules. Indeed, Chomsky gives no reason why all the citizens of the world could not get together and simply decide to change one of the rules, *by fiat*. All of this shows that while the rules may be innate and synthetic, they are not *a priori* in any traditional sense and thus do not shed light on the traditional philosophical problems associated with this concept.

Chomsky might respond by asserting that any change in the rules would result in something that would not be language. But this would seem to be quite implausible, especially if one could translate across the two differently rule-governed sign-systems. If it is suggested that it is *a priori* impossible to effect translation, Chomsky is left with his own version of a well-known difficulty: Even if we can prove in various ways that basic rules governing our knowledge of language are innate, this does not mean that they are cross-culturally similar in all existing cultures, let alone all possible cultures. It may be that while these sign-systems always require innate principles, different ones might have different principles, as remarked above. But if there were the case, it is difficult to see how we could know this, since *ex hypothesi* the two "languages," ours and theirs, are incommensurable. We could not understand other systems, except as filtered through our own. (There are, of course, those who suggest that different cultures *do* have different cognitive apparatuses. This is not *a priori* impossible. Like major physical and social differences, these could be biologically based. Would it then be possible for us to explain them in Chomskyan terms?) What we have just said of language applies *mutatis mutandis* to any other cognitive system allegedly based on innate principles: for example, to Chomsky's claim about our knowledge of objects.

I have argued, in essence, that any knowledge based on biologically determined rules, even though those rules are innate, will be contingent – empirically changeable, or refutable. This leads us to a further difficulty in Chomsky's argument. Let us suppose that our knowledge of mathematics or logic is determined, in part, by innate knowledge of rules. (Since these areas are clearly rule-governed, this seems more plausible than the idea that our knowledge of objects is so based.) These rules, as we have just seen, are subject to change on the basis of evolution, or as a result of radiation, chemicals, and so on. Would it then follow that what we now call our knowledge of mathematics and logic would change, or worse, be rendered false by empirically caused changes in the brain and correlatively in the innate rules? It is difficult to see how Chomsky can avoid this conclusion, yet it is also difficult to see how any distinction between *a priori* and *a posteriori* knowledge can be sustained if all cognition is based on biologically determined innate principles that are subject to change. If Chomsky is not disturbed by the prospect of destroying this distinction, once again the burden of proof is on him to give reasons as to why we should not mark such apparently major differences.

On the other hand, I grant that the sense in which the innate rules he postulates are empirical or *a posteriori*, or better, contingent, is an unusual one, since they are presuppositional for our current experience. Clearly, if Chomsky is right about the role of these rules in the knowledge of objects, for example, the sorts of changes I envisioned that could take place in the rules are not much like finding the proverbial white raven. If the rule changes were drastic enough (or perhaps if there were any changes at all), our entire way of experiencing the world would change – we would literally live in a world totally different in fundamental ways from the old one – and it is conceivable that the old world would no longer even be comprehensible to us. I believe that Chomsky would assert that our traditional distinction between *a priori* and *a posteriori* requires further refinement to do justice to such dramatic cases, although I am uncertain as to what form this reworking would take. As suggested above, he may in the final analysis wish to eliminate any hard and fast notion of *a priori* altogether, and have nothing stronger than a concept of what is currently presuppositional on the basis of our mental makeup. Again, this is *prima facie* implausible when applied to mathematics and logic.

In sum, I consider Chomsky's analysis viewed as a program and theoretical basis for psychological research to be a salubrious counterforce to the atheoretical, haphazard dabblings that characterize much of behavioral psychology. I should like to see more discussion of the analogy between knowledge of language and knowledge of objects, as well as a sketch of what empirical research would be like in domains like knowledge of objects or knowledge of mathematics. I should further like to see Chomsky extend his program to animal psychology as well, since his arguments from poverty and diversity of stimuli and uniformity of result apply to the mental lives of animals as well as men. Indeed, *prima facie* at least, it appears that the evidence for innate principles in animals' minds is even stronger, since animal minds obviously develop uniformly within species despite vast environmental and experiential differences – there is here no cultural overlay to mask the similarities. The possibility for intra- and interspecies communication would be a natural target of study, for example [see *BBS* 1(4) 1978]. On the other hand, I do not think that the implications of Chomsky's analysis for traditional metaphysics and epistemology are quite as dramatic as they may appear to be at first blush.

Acknowledgments

I have benefitted from discussions with Professors Kenneth Freeman and Ron Williams.

by **David M. Rosenthal**

Department of Philosophy, Graduate School and University Center, City University of New York, New York, N.Y. 10036

The modularity and maturation of cognitive capacities

The two central themes in Chomsky's discussion that I want to consider are his hypothesis that "the mind [is] modular in structure," and his inviting idea that we acquire much of our knowledge in a way more like ordinary biological growth and development than like learning, as learning has traditionally been conceived. In section I, Chomsky notes that the question of modularity is distinct from the issue of whether the mind has rich initial structures. But he also claims there that "there is a relation between the views, in part conceptual," for the assumption of modularity "leads to the natural conclusion that [the various] systems [of mind] are intrinsically determined." He does not, however, offer support for the converse connection and, indeed, notes that "[o]ne might hold that there is rich innate structure but little or no modularity."

It may not seem immediately clear, however, why Chomsky believes that, by itself, the hypothesis of modularity has any bearing at all on whether there are rich initial structures. Nor is it clear just how strong he takes the relationship between the two hypotheses to be. In some passages he seems to mean nothing more than that "opinions [about the hypotheses] cluster. Those who tend toward the assumption of modularity also tend to assume rich innate structure." It does seem clear that our mature mental capacities, and the mature structures we can assume underlie those capacities, reflect diverse families of principles. If one took modularity to consist in this relatively uncontroversial observation, and if that observation somehow made plausible the idea of rich initial structures, then perhaps innateness would inherit some of the uncontroversial and common-sense character of modularity. But the clustering of these two views, so understood, does not actually occur. Many would agree that our mental structures, when fully developed, reflect divergent principles without being at all inclined to endorse the idea of rich initial structures.

But at other points Chomsky writes as if modularity is a substantially stronger thesis, which actually entails the assumption of rich initial structures. For example, at the outset of section III he claims that "[i]f the modular approach is incorrect," the study of interacting mental systems would "reveal . . . that these systems involve the same principles and develop in the same way from a common basis." And early in section II, having offered as an example of modularity the distinction between the computational and conceptual aspects of language, he goes on to urge that it might be possible "to distinguish these systems much as we distinguish the visual and circulatory system." This analogy between bodily organic systems and the modularity of mental structures is repeated often (e.g., eight paragraphs later and throughout the second half of section I), and it reinforces the impression that such modularity is a matter not only of the diversity of mature capacities but also of their genetically controlled development from diverse initial structures.

Perhaps this seems clearest in a passage that comes directly after Chomsky has distinguished modularity from rich initial structures in section I. For he writes that "we are . . . led to the conclusion that intrinsic structure is rich (by the argument from poverty of the stimulus) and diverse (by virtue of the apparent diversity in fundamental principles of capacities and mental structures attained)." Here as elsewhere, Chomsky evidently uses "intrinsic" in a way that implies innateness; for richness of "intrinsic structures" amounts to the richness of initial structures. But then it is these initial structures that, on the thesis of modularity, are said to be diverse; the "diversity in fundamental principles of capacities and mental structures attained" is offered as evidence for the diversity of rich initial structures. So it is not simply that "opinions [about modularity and rich initial structures] cluster"; modularity actually includes the assumption of rich initial structures.

The foregoing quotation also calls attention to a striking pattern of inference that recurs in Chomsky's discussion – namely, the passage from "diversity in fundamental principles of capacities and mental structures attained" to some corresponding diversity in initial and innate mental structures. It is possible, of course, that fundamental principles of our mature mental structures could illuminate features of the initial structures from which mature structures develop. And perhaps with language this is the case. For one might take the presence of highly abstract but strong constraints, to which the otherwise divergent grammars of all human languages seem to conform, as being evidence of common features in the relevant initial structures. And Chomsky is surely right that, insofar as we currently understand these constraints, "[i]t would be surprising indeed if we were to find that the principles governing these phenomena are operative in other cognitive systems."

But the inference from the principles governing mature mental capacities and structures to those governing initial mental structures is far less credible in the case of the other "mental faculties" that Chomsky touches on, such as "knowledge of . . . music, of mathematics, of the behavior of objects, of social structure, of human characteristics, and so on." In the case of music, there is a great diversity of musical systems, which might seem to echo the diversity of human languages. But here the common features in these diverse systems appear to derive from well-understood apects of the mechanisms underlying the production of sound, rather than from anything innate in the organism. And the presence of features common to otherwise significantly diverse systems is wholly absent in the case of mathematics, though according to Chomsky "it seems reasonable to suppose that [the number] faculty is an intrinsic component of the human mind."

Chomsky's brief treatment of the "number faculty" merits special attention. For he claims that "[t]he capacity to deal with the number system . . . is surely unlearned in its essentials." If Chomsky has in mind, here, either the "second-order sense of 'capacity' " that he isolates at the outset or the sense of "capacity" as a mental faculty, his claim that our capacity to deal with the number system is unlearned will be trivial and uninformative; for it is a matter of meaning that, in these senses, capacities are unlearned. So Chomsky must have in mind the more interesting but surprising claim that our mature capacity to deal with the number system, though in many respects the product of training, is, "in its essentials," innate. Without some idea of what the "essentials" of this capacity are, however, even this claim would lack significant import. But Chomsky tells us that "[t]he very essence of the number system is the concept of adding one, indefinitely," presumably inviting us to infer from the "very essence of the number system" to an understanding of what the "capacity to deal with the number system . . . [is] in its essentials." Chomsky is of course right that "the concept of adding one, indefinitely" is sufficient to distinguish our number system from the rudimentary numerical abilities of other terrestrial species. But it is far from clear that understanding the essence of our number system can, by itself, help us to understand our capacity to deal with that system.

Chomsky would presumably agree that the essence of our number system, for example, cannot by itself reveal details of specific mechanisms that underlie the relevant capacity and mental structure. But he would insist that the essence of our number system does serve to characterize that structure at the appropriate level of abstraction. As he puts it at the outset of section III, his discussion "proceeds as an inquiry at a certain level of abstraction into the nature of certain mechanisms . . . , now largely unknown. The level of abstraction is appropriate insofar as it enables fundamental properties of [cognitive] systems to be isolated, analyzed, and explained; and insofar as results obtained at this level provide a guide for the study of mechanisms." An accurate and revealing description of a subject matter with which mature humans can deal is, of course, useful and perhaps ultimately necessary for a satisfactory study of the human capacity to deal with that subject matter. But that is because such a description defines the problem under investigation. Such a description does not, as Chomsky makes it appear, constitute the beginning of an actual theory about the nature of the target capacity, even "at a certain level of abstraction."

Chomsky believes that such descriptions can "provide a guide for the study of [underlying] mechanisms, much as the study of chemical properties provides a guide for inquiry into atomic theory" (*ibid.*). Presumably the intended analogy is that, just as atomic processes must be able to give rise to chemical properties, so neural mechanisms must be able to generate and deal with the number system. This naturalism with respect to mental phenomena is thoroughly laudable. But the usefulness of chemical properties in studying atomic structures depends on our having some active grasp of atomic mechanisms. One could not have predicted how these mechanisms would turn out to be able to achieve their effects at the level of chemistry merely by knowing how those effects can be systematized, at their own distinctive level. Indeed, some fair success at the level of atomic theory seems to have been necessary before we could arrive at an accurate and revealing systematization of chemical phenomena. And in general, the ways of systematizing phenomena at higher levels of organization have often proved to be false guides to the nature of phenomena at underlying, lower levels of organization. Moreover, since the mechanisms that produce effects at higher levels of organization are largely unpredictable on the basis simply of a knowledge of those effects, the idea of higher and lower "levels of abstraction" seems straightforwardly inapplicable in this context. For it is not as though we could arrive at descriptions at higher levels of organization by way of some process of abstraction. And it is even less credible that we might arrive at acceptable descriptions of our mathematical or musical systems by abstracting from innate mental structures or from their underlying neural mechanisms. So, although more abstract descriptions often do illuminate concrete details, this process cannot be expected to help in the present context. It is these concerns, and not some antinaturalist attitude towards the mental, that are responsible for the worry that Chomsky's theories may well lack psychological reality.

The worry about psychological reality also arises in connection with the question of whether Chomsky's approach can capture, in a non-question-begging way, the distinction he appeals to in section III between having knowledge and having a skill. The example he offers of two missiles does not help here. For we know that the "cognizing" missile "incorporates an explicit theory" simply because it was so constructed. What we need is some clear idea of how to tell when a system of mechanisms incorporates rules and representations which does not simply reduce to the question of whether the system can be described as if it did. Chomsky supposes that in many cases "there is a mentally-represented system of [a grammar-like] nature, which can be taken to be an object of knowledge." But he suggests no clear way of trying to decide whether what we have is merely a set of mechanisms that act as if they embody rules and representations, or a set of mechanisms that actually does.

Even when modularity is understood as a thesis about the divergent principles of initial as well as mature mental structures, modularity and the hypothesis of rich initial structures do not jointly imply Chomsky's striking speculation that "a central part of what we call 'learning' " is essentially a matter of biological growth and development. Nor is it obvious that the hypotheses of modularity and rich initial structures would, if true, even provide evidence for this bold thesis about learning. Even if our acquisition of knowledge depends in some way on initial mental structures that are both rich and diverse, such acquisition might nonetheless be more like a process of imitation and adaptation, for example, than like the maturation of a bodily organ. And slight differences in initial structures could lead to great diversity in mature structures whether the fundamental mechanisms involve interactions with the environment or internally-controlled processes. The idea that acquisition of knowledge is basically a matter of biological maturation does of course imply modularity and rich initial structures. But the converse does not hold; the idea of learning as growth is a further step in the analogy of our cognitive capacities with physiological systems. However, this view about the acquisition of knowledge does fit well with the idea that systematizing the objects of our knowledge is likely to shed light on the nature of the operative mental capacities and structures. For, if "a central part" of our knowledge develops in the manner of genetically-determined maturation, it is then reasonable to expect that the character of our ultimate objects of knowledge will be informative about initial mental structures. And if one were convinced that the ultimate objects of knowledge would be revealing in this way, the model of genetically-guided development would very probably seem appealing.

If "a central part" of our acquisition of knowledge is essentially a matter of biological maturation, the relevant genetic instruction will then determine to some degree what it is possible for us to know. Chomsky does not shrink from this consequence, writing at the close of section I that "the very same intrinsic factors that permit [cognitive] achievements also impose severe limits on the states that can be attained." Chomsky's frequent references to biological determinants of our perceptual cognitive capacities lend some support to this idea. For it is obvious that the range and character of our perceptual knowledge is a function of our organic perceptual apparatus.

But Chomsky also holds that much of our nonperceptual knowledge is acquired by a process essentially like biological maturation, though often it remains unclear whether, in a particular passage, he is simply asserting modularity of initial structures or also affirming the idea of learning as growth. But he does suppose that "[i]t is entirely possible that significant components of [many] cognitive states are 'wired in,' " and his examples of this (e.g., at the end of section III) range well beyond perceptual knowledge. Chomsky's formulations of this idea tend always to involve phrases such as "significant components," "a central part," "in its essentials," "substantially in place," and the like; this results in some measure of obscurity about just what this idea involves. Presumably he would urge, as he does in a related context, that we are dealing with "a difficult empirical question, only partially clear, which can become more precise only in the course of finding some answers to it."

However the issues are sharpened, Chomsky's idea "that significant components of [many nonperceptual] cognitive states are 'wired in' " will require that what propositional knowledge we can have is, to some degree, a function of our biological endowment. And an apparent consequence of this view is that cognitive beings with suitably diverse biological endowments will diverge in what propositional knowledge they can have and, hence, would have to differ even in the range of propositions they could comprehend. If this were so, it would be possible for organisms to exist whose cognitive capacities were comparable to ours in richness and range, but with whom communication would be severely obstructed because of differences between them and us with respect to what propositional content can coherently enter into speech and thought. And it would perhaps also be possible for organisms to exist who could comprehend all our thoughts but think things literally unthinkable by us. This striking idea, though it appeals to biological rather than divine determinants, is reminiscent of Descartes' vexed view about the nature of the eternal truths, according to which it is a contingent fact about our minds that we "cannot conceive a mountain without a valley, or an aggregate of one and two which is not three" (Kenny, p. 236; see Frankfurt, especially section VI, for a highly illuminating discussion).

Many, however, would contest the very intelligibility of this difficult thesis, arguing that one respect in which propositional cognitive capacities differ from perceptual cognitive capacities is that, given time to define terms and explain theories, a certain threshold intelligence is all that is needed to comprehend any proposition whatever. Perhaps progress could be made in giving some measure of intelligibility to the idea that biological endowment can cause variation in what propositions are thinkable. But cognate views, such as the idea that different human languages might embody distinct conceptual schemes, or the idea that children may have something like distinct conceptual schemes at successive stages of cognitive development, have been notoriously unyielding in the face of efforts to confirm them, or even give them clear content. Much of the difficulty in giving content to such views results from the complex interactions that hold between meaning and belief, which Chomsky touches on midway through section II, though perhaps some nonarbitrary ways can be found to sort out these factors.

Whatever the case on these questions, it seems clear that, if essential aspects of our propositional knowledge have biological determinants in some nonvacuous respect, other cognitive beings could diverge from us in respect of what propositional content could enter their mental lives. The difficulty of making clear sense of that possibility, therefore, should make us cautious about whether clear sense can be given to Chomsky's idea of learning as growth. One might maintain, of course, that if such divergence were a genuine possibility, our difficulty in comprehending that possibility might simply be the result of biologically determined limits on our knowledge. But there is no reason to suppose that these limits would be responsible for this particular case of our being unable to comprehend something; for one would expect some things to be incomprehensible no matter what one's cognitive capacities, and such divergence might be a case of this sort. Such self-supporting defenses aside, therefore, the apparent incomprehensibility of this sort of divergence in the ability to entertain propositions gives us compelling reason to seek an alternative to any theory of the acquisition of knowledge that has this consequence.

by **Geoffrey Sampson**

Department of Linguistics, University of Lancaster, Lancaster LA1 4YT, England

Chomsky's evidence against Chomsky's theory

Contrary to Chomsky's claims, there *is* good reason to argue that "the brain is unique in the biological world," in that its complex products are determined hardly at all by fixed mechanisms but emerge as attempts to make sense of experience in ways unpredictable on the basis of initial state. Furthermore, it is Chomsky himself who has drawn attention to the evidence for this view in various of his works on language, even though he interprets his evidence in a very different way.

Some of those who have discussed Chomsky's writings over the years have taken the issue as one to be settled by pure philosophical analysis. Chomsky himself, in this piece as elsewhere, insists that the question is an empirical one. He argues that the evidence overwhelmingly favours his view that complex products of human minds, such as - most obviously – language, are determined in advance by genetic inheritance in much the same sense as that in which the structure of the eye, say, is predetermined; by presenting the question as one to be settled empirically, he implicitly concedes that the view he opposes is in no way *logically* incoherent or objectionable – it just turns out (Chomsky claims) not to fit the facts.

Let us therefore reconstruct this alternative, non-nativist account of how an individual might acquire his first language. So far as I can see, if we assume that the individual begins life with no inherited "knowledge" (or "cognizance") of language (and I shall not pursue the purely conceptual discussion of how it is legitimate to use the word "know" – I concede all that material to Chomsky for the sake of argument), then the most plausible account of language-acquisition will be one in harmony with the evolutionary epistemology of Sir Karl Popper: we must envisage the child formulating small-scale, fallible hypotheses in reaction to (but not predictable from) the experience impinging on him; many of these hypotheses will be weeded out as incompatible with further experience, but some will be retained; these will be built on in formulating higher-level, more general, but still fallible hypotheses, and so on. If we assume that initial state does not determine the details of the mental construct that eventually emerges, then we must presumably say that the hypotheses are invented creatively rather than being drawn from a range predetermined by the machinery of mind. Of course, there are conceptual problems with this account (e.g., what is it to "formulate a hypothesis" when one does not yet speak any language?); I believe these objections can be satisfactorily answered, but the important thing to note here is that such objections are not available to Chomsky, for whom (to repeat) the question of innate knowledge of language is an empirical question that cannot be settled by ruling out the alternative view on conceptual grounds.

If this non-nativist, evolutionary account of language-acquisition is reasonable *a priori*, is there anything that would count as evidence for its truth? Yes. H.A. Simon (1962) has argued that, for reasons having to do with the mathematics of probability, any complex product that has emerged from a process formally akin to that of Darwinian evolution will be expected to manifest certain fairly general structural properties, having to do with the notion of "hierarchy." In a number of my own writings (Sampson 1978; 1979; 1980) I have argued that, on the Popperian view just sketched, the acquisition of syntax would be an evolutionary process in the sense required for Simon's argument to be applicable – indeed, Simon's argument ought to apply better to the case of language than to various of the domains discussed by Simon himself (Sampson 1980, p. 147). (Furthermore, on this view, the individual's acquisition of language would in its general outline recapitulate the process by which languages emerged within originally languageless communities.) If the Popperian, non-nativist view is correct, then we would expect the languages that have developed in each human community, and likewise the individual mental representations of their mother-tongue that are attained by each member of a speech-community, all to share certain abstract grammatical properties (while differing in detail); they should all be based on hierarchical structure, and the syntactic operations found in them should always be "structure-dependent" ones.

In many of his best-known works (e.g. 1968, Ch. 2; 1973, Ch. 1; 1976a; Ch. 3) Chomsky argues that these properties are universal; indeed, it is their universality that he uses as his central evidence for innateness. But, while the hypothesis of an innate, fixed language faculty predicts only that there will be *some* linguistic universals, the Popperian hypothesis of fallible evolution of knowledge predicts, via Simon's argument, certain *specific* linguistic universals, and just those are what we observe – so the Popperian account must be preferred. That is, while observation of linguistic universals might in general count as evidence for Chomsky's nativist view of mind, *the particular universals cited by Chomsky are evidence against, rather than for, that view.* A detailed examination (Sampson 1980, Ch. 9) of Chomsky's and other linguists' theories of linguistic universals shows that these match beautifully the properties that Simon's argument leads us to expect, if languages are created from scratch rather than being largely built-in in advance.

The uniformity of certain properties of language is Chomsky's principal argument for nativism, both here and in his previous writings. In the final section of this piece he alludes to a subsidiary argument when he says that "the rate of vocabulary acquisition is so high at certain stages of life, and the precision and delicacy of the concepts acquired so remarkable, that it seems necessary" to accept a nativist account of the acquisition of vocabulary. This is a specialized variant of a form of argument for nativism that Chomsky has used elsewhere: children learn their first language so rapidly and so well that they must know much of its structure before they start – they could never finish in the same time if they had to begin from scratch. Any argument of this form seems to me wholly empty, since it falsely assumes that we have some way of measuring the attainments that would be expected if the nativist hypothesis were wrong. I do not know the peak rate at which children acquire vocabulary, but suppose it is twenty words a day. On what grounds does Chomsky call this a "high" rather than a "low" rate? How fast should children be able to learn new words if their "conceptual system" were *not* "already substantially in place": five a day? one a day? one a year? Similarly, how much less precise and delicate would concept-acquisition be on this latter assumption? – indeed, in what units could this "precision" conceivably be measured and compared? Without discussion of these issues (which Chomsky never gives), this strand of the nativist argument collapses.

I conclude that the traditional view, according to which minds differ from bodies in being creative and not limited to a well-defined range of predetermined possibilities, is on Chomsky's own evidence the correct view.

by **Roger C. Schank**

Department of Computer Science, Yale University, New Haven, Conn. 06520

An artificial intelligence perspective on Chomsky's view of language

Is there any reason to believe that there is a separate "language faculty" that is one particular component of the mind? I take Chomsky's main point in this article to be that there is such a component of the mind, which he believes can be clearly distinguished from all other components. The arguments that he makes in its favor indicate that he holds a great many presuppositions about language, and what a theory of language must explain, that seem quite tenuous.

What ground must a theory of language cover? What is a theory of language supposed to explain? To those of us who work in artificial intelligence (AI), the answer to these questions seems quite clear. We wish to produce specific models, or algorithms, that simulate the communication process – i.e., that produce language in response to inputs. Three issues arise with respect to the above goal and Chomsky's position. First, would any model that an AI researcher constructed, that successfully modelled a human speaker/hearer, necessarily embody a theory of language? Second, would any purely formal competence theory of the kind Chomsky advocates aid the development of a working computer model? Third, what would be the relation between the presuppositions of an AI theory of language and the presuppositions that Chomsky presents in his paper?

Answers to the first two questions are frequently polemical. Chomsky and others have stated that a computer model of language comprehension and production, even if perfect in its input-output behavior, could still possibly be of no interest to the linguist and could possibly fail to embody a theory of language. AI workers, on the other hand, often simply assert that their programs are their theories and leave it at that. What is in fact the case? Are AI workers also serious language theorists? If they are not, should they be?

I will, of course, claim that they are, but I shall try to do so by using Chomsky's own arguments from this target article.

First, let me point out that it is very easy to say that mechanisms that perform certain tasks need not embody theories of those tasks. We are better off asking whether people embody theories of language. An AI response to such a question is that, while people do speak and comprehend, they do not have conscious theories of how they do so. Our task as language theorists then is to determine exactly how they do speak and comprehend. Thus, a theory here becomes equivalent to a model. When Chomsky states that knowledge of language is not a "task-oriented device," he is quite mistaken. Certainly, people have knowledge of their language that they employ in the task of communicating. But if that is what he is referring to, then how can that knowledge be anything other than task-oriented? People know their language so that they can communicate!

The theory that Chomsky is interested in constructing can only be relevant if it is somehow related to what people are doing with language, and consequently to computer simulations of the communicative process. Chomsky accepts this point by claiming that grammars are actually knowledge that people have, which they use in performing language tasks. But he continues to make excuses for why it isn't possible for him to "construct specific models," or why he cannot "attempt a systematic presentation of such a model." He seems simply to be hoping that some sort of hard evidence will be found to support his theories. But, while AI workers and psychologists continue to find evidence that supports theories quite antithetical to Chomsky's (Bower et al. 1979; Graesser et al. 1979; Nelson and Gruendel 1978), he dismisses his lack of evidence with statements such as "Data that remain unexplained by some coherent theory will continue to be described in whatever descriptive scheme one chooses, but will simply not be considered very important for the moment." He does this because he holds the belief that "substantial coverage of data is not a particularly significant result; it can be obtained in many ways."

It is remarkable that Chomsky holds such beliefs. If the construction of specific models that amount to "substantial coverage of data" is so easy, then why hasn't Chomsky or any of his followers ever constructed one? The answer to this question is found on p. 4 of the target article, where Chomsky poses a question that he regards as a difficult one for a theory of language. Before I begin to discuss this question, however, let me first point out that, although I use many of the same terms that Chomsky does, this nonetheless appears to involve next to no agreement on what a theory of language must explain. Let me reassert my view here:

A theory of language must explain how people can comprehend sentences that they read or hear, and how they respond appropriately. In my view, a theory of language must embody a theory of comprehension, a theory of production, a theory of memory, a theory of motivation and behavior, and much more. Thus, I believe that there is no possibility of constructing a model of the process of communication without having fully developed theories of all these concomitant processes. For me, then, a theory of language *per se* is a vacuous idea, impossible on the face of it and pointless on a deeper level. A theory that accounted for language and language alone could not possibly account for communication. Since communication is at the heart of language, such a theory would be only so much formal apparatus, explaining nothing; it would exist as something to prove theorems about but not as something to build specific models upon.

My point is that there is a reason for Chomsky's failure to produce specific models despite a purported interest in those models. The reason is that it is impossible to produce a model of language alone, and that is precisely what Chomsky has stated his goal to be. The phenomenon of language alone, apart from beliefs, goals, points of view, and world knowledge, simply does not exist. Language cannot be studied apart from its use and still maintain any of its important properties. The only properties it would continue to have in such a study would be those associated with its formal structure, as distinct from its communicative content. This is, of course, precisely what Chomsky has chosen to study. To see this better, consider Chomsky's example on p. 4 of his article.

On p. 4 Chomsky presents a problem in the "process of forming questions." He states that the process first selects "some Noun Phrase in a sentence," which is then "replace(d) . . . by an appropriate question word." This is then placed at the beginning of the sentence to form a question. He states, "Thus, on the model of the sentence 'John saw a man' we can form 'Whom did John see?' "

Embodied in this example are the rudiments of Chomsky's theory of language. His view is that the process of forming questions is a matter of formal procedures, which transform declarative sentences into interrogative ones. Chomsky uses the word "process," but it is not clear what he means by this. In AI, an adequate theory of a process is one that explains that process in terms of step-by-step procedures that can be tested computationally. Thus, it is the processes that people use when communicating that ought to be of interest to the language theorist. This is also the source of the "psychological validity" issue about which Chomsky seems in such a quandary. Let me clarify this by presenting an AI explanation of question-formation.

When people form the question "Whom did John see?", we have to imagine that they do so because they desire to know the answer to this question. In other words, the input to the process of question-formation is not a declarative sentence, but rather a desire for information. This desire for information, combined with a belief that the person to whom the question will be addressed possesses that information, starts the process of question-formation. In this particular case, more information is desired about some person. Once the need to know about some feature of a person is established, the generation process begins. At Yale we have written rules that are explicit enough to enable computers to both ask and answer questions in quite natural English. Such rules are not all that mysterious. However, they depend on the ability to use knowledge of the situation in question as well as knowledge about what information our source has that can identify the "who" for us (Lehnert 1977). To do this, we examine our knowledge of this "who," such as prior situations that this "who" has participated in. For example, we might have available to us the information that John went to a psychiatrist, and thus we want to know his name. Alternatively, we might have heard that John spotted a famous celebrity and we want to know his name or status. Equally plausibly, we might know that John was arranging for an appointment for himself with an official, and we want to know the rank or position of the official, rather than his name. One of these desires is already known to us prior to forming the question, and is instrumental in the process of forming the question. Thus we might generate, "Whom did John see?", or perhaps "Whom did John spot?", or "Whom did John have an appointment with?"

In none of these cases are we transforming a declarative into an interrogative. One can, of course, imagine a formal procedure that generates questions by the use of such transformations, but of what explanatory use would such formal rules be? A procedure that modelled people's question-formation process would not attempt to transform a sentence such as "The lesson was harder than the teacher had told the class it would be," an example that presents difficulties for Chomsky's theory of question-formation. Chomsky has such difficulties because the complexity of his theories leaves him no choice but to claim "innateness" as an explanation of these theories.

Chomsky postulates an important "general property of language" from the fact that people do not actually attempt to transform this sentence into an interrogative that questions "the class." Actually, he does think that people are silly enough to try it, but he allows that they are also smart enough to realize that such a question would be more than cumbersome, and thus somehow they are able to "block" that question!

Chomsky gets into this kind of trouble because he insists on separating a theory of language from a theory of motivation or knowledge-acquisition. Thus he ends up wondering about how people turn off this formal apparatus that he has invented when it goes haywire. To do this he invents *ad hoc* blocking mechanisms that he then decides are "general properties of language." Such general properties must be innate, he reasons, because how could they have been learned?

How, indeed! There is no way to imagine how such mechanisms could be learned, but that is not because they are innate. They need not be learned, because they do not exist. We do not form questions apart from our desire to know something. We do not possess a purely formal question-former that cleverly blocks itself from forming absurd questions. To imagine such a mechanism in people is to believe that people do not think at all, or to believe that their language faculty is apart from their other cognitive faculties and in very poor communication with those faculties.

The issue here, I believe, is to determine what a theory of language should explain. The issue as Chomsky sees it, however, is what form a theory of language must take. Actually, Chomsky doesn't see this as an issue; he simply asserts the answer – a grammar. This belief that grammars are learned causes him great problems, however. For example, he asserts that

> An investigation of the final states attained – that is, the grammars – reveals that the knowledge acquired, and to a large extent shared, involves judgments of extraordinary delicacy and detail. The argument from poverty of the stimulus leaves us no reasonable alternative but to suppose that these properties are somehow determined in universal grammar, as part of the genotype. There is simply no evidence available to the language learner to fix them, in many crucial cases that have been studied. Nevertheless, people obviously speak different languages depending on their limited individual experience." (p. 9)

In other words, these grammars are so complex that no one could ever learn them. I have no doubt as to the truth of that statement, but there is an obvious explanation as to why no one could learn them. We don't learn them because we don't use them. Grammars exist in the minds of linguists only.

Is the stimulus really so impoverished? Recent research at Yale indicates that it should be possible to learn to understand a language by relying upon a well-formed conceptual model before starting to learn language, and by using this method to guide the learning process (Selfridge 1979). In such a view of language learning, the need for a rich set of language experiences is minimized. A rich conceptual apparatus guides the language learner in finding out what he wants to know. And what does he want to know? Not formal properties of language, surely. He wants task-oriented knowledge that will allow him to communicate. This kind of knowledge of language is obtainable through interactions with one's environment in a real setting. Chomsky would have us divorce the conceptual from the language, leaving the poor child truly quite impoverished. As long as Chomsky acknowledges that child language acquisition is an important issue, I cannot see how he can maintain that abstract formal descriptions of language are the proper domain of study for the language theorist.

In the end, of course, we in AI are concerned with psychological reality. Chomsky clearly is not concerned with that problem. Rather, he is concerned with "(truth) at the level of description at which (he is) working." by those ground rules, any game he plays is the right one. But there are means of testing assertions about processes, both computationally and through psychological experimentation. Chomsky strongly disavows both of these tests. One wonders what is left. Surely the "cognitive sciences" of which Chomsky speaks will be in for difficult times if the framework they adopt is "studying the rules and representations of cognitive systems" without regard for how people actually think and speak, and without regard for the natural processes that comprise their ability to do so. Formal systems, in and of themselves, apart from any role in the process of communication, are of no interest at all to cognitive science; if they are of interest to linguistics, then linguistics is in a sorry state indeed.

by **John R. Searle**

Department of Philosophy, University of California, Berkeley, Calif. 94720

Rules and causation

It is a peculiar characteristic of some of Chomsky's responses to discussions of his work that he assumes that attempts to characterize his work must be objections. When I wrote (Searle 1976) that the rules of his grammar are "abstract and complicated" and lacked "the intuititive plausibility of ordinary grammar rules," I was not putting that forward as an objection, but rather as a characterization of his position, and a characterization he could hardly disagree with. The objection of which these characterizations were a part – I would prefer to call it a problem – is rather more subtle. Since he did not understand it from my earlier writings, I will spell (part of) it out.

In the explanation of a natural phenomenon, such as the behavior of a falling body, we specify general laws or hypotheses under which the behavior of the body can be subsumed. The laws describe the behavior of the body; they play no causal role in producing it. But when it comes to explaining human behavior in terms of rules and systems of rules, the situation is quite different. If, for example, we explain my car-driving behavior in part by saying that I am following the rule: "Drive on the right-hand side of the road," that rule doesn't just describe my behavior; rather, my internalization of the content of the rule plays a causal role in the production of the behavior (and it is important to emphasize that it is the *content* of the rule that acts causally, since the form of causation in question involves *following* the rule). In the natural sciences hypotheses merely describe and explain; in the explanation of human behavior the rules *cause* the very behavior that they describe and explain, and they don't explain the behavior unless they cause it. If the rule is one that people are actually following, then the content of the rule must function causally in the production of the very behavior that the rule explains. Now, this distinction has an important consequence for any attempt to analyze cognitive structures as "systems of rules and representations." The claim that the agent is acting on rules involves more than simply the claim that the rules describe his behavior and predict future behavior. Additional evidence is required to show that they are rules that the agent is actually following, and not mere hypotheses or generalizations that correctly describe his behavior. It is not enough to get rules that have the right predictive powers; there must be some independent reason for supposing that the rules are functioning causally.

Well, what sort of evidence would show that people are following rules of language? One kind of evidence would be to make people aware of the rules and get them to see or agree that those are indeed the rules they are following. This is, in fact, the implicit procedure I follow in *Speech Acts,* that Grice follows in "Logic and Conversation," and that is followed in a host of similar investigations. Such evidence, like all empirical evidence, is inconclusive; and it is not the only possible sort of evidence that one is following a rule unconsciously. But for someone who does not have this sort of evidence, there ought to be some other sort. In Chomsky's case I am unable to see what he thinks the evidence is, or even that he adequately recognizes the problem.

In his target article Chomsky asks us to assimilate the study of language to the natural sciences. We are to think of the "mind" as "modular" and as containing three systems for language: the computational system, the conceptual system, and the pragmatic system. In the case of the pragmatic and the conceptual systems we at least have some evidence that people are engaging in rule-governed behavior, and that they can be brought to an awareness of the rules they are following. The rules are *ceteris paribus* rules, and their causal effect is a matter of their content. But what about the computational system? And in particular, what is the evidence that the rules of universal grammar are actually playing a causal rule in the production of the behavior they describe? This question becomes more pressing when we reflect on the following: precisely to the extent that we take Chomsky's analogy with biology seriously, to that very extent we do not need the hypothesis of rules and representations. If we are to think of the language organ on analogy with the heart or the liver, then we do not need the additional hypothesis of rule-governed behavior at all. The heart does not follow rules, nor does the liver; rather, there are certain general functional principles that describe their operations, and there are even semiteleogical statements that we can make about their functioning. But none of these statements or principles are rules, for there is no mental content playing a causal rule in the operation of the heart or the liver. Similarly with many of Chomsky's other analogies: that a certain bunch of neurons fires when my optical apparatus is stimulated by a certain light frequency is a matter of natural fact. There is no question of my following a set of rules. And in describing the very complex antibody-antigen reactions in the human organism, the biochemist describes chemical reactions; he does not have to add a series of mental contents that are producing the behavior. Again, it is a fact about my visual apparatus that I am incapable of perceiving infrared and ultraviolet, but that is not because I follow a rule of "universal visual grammar" that forbids such perceptions; it is just a fact about my physiology.

"Well," one might say, "what difference does it make? Suppose Chomsky had the 'rules' of universal grammar stated correctly; then it wouldn't matter whether people followed these rules or whether the rules were 'mere hypotheses' describing what happens." But unless the principles are stated in the form of rules that actually play a causal role in the production of behavior, there cannot be a level of "mental organs" that are constituted as system of rules and representations.

To see this point, consider an alternative speculation to the ones that Chomsky makes. Suppose that the structure of possible human language is constrained by the structure of the human brain (and notice that I say "brain" and not "mind"). Suppose that quite specific neurophysiological structures in the brain constrain possible natural human languages in the same sense of "constrain" that the neurophysiological structure of the visual system constrains possible visual experiences. Now, if that were the case, then the rules of all human languages would have certain features in common, and we might even be able to state these features as a set of generalizations satisfied by all human languages. And this commonality might give us the illusion that, in addition to the actual rules of real human languages such as French and English, there were some super rules of "universal grammar" that collectively constituted a mental organ. But that would be an illusion created by the fact that actual physical structures in the brain were doing their work, in the same way that it would be an illusion to suppose that the visual system follows a super rule blocking the perception of ultraviolet and infrared. In neither case do the alleged "rules" play any causal role, which is another way of saying there aren't any such rules. In both cases the appearance of a rule would be a product of the fact that the underlying neurophysiological systems place constraints on the actual systems of rules and representations in both vision and language. The super rules, in short, would be artifacts produced by the fact that a common neurophysiological structure constrains actual rules. If such a speculation were indeed accurate, then there would be two levels of description. First, there would be a mental level where we describe how actual rules and representations function causally. Such rules and representations are of course realized in the neurophysiology, but they require a separate level of analysis because of their causal efficacy at the level of rules and representations. And secondly, there would be a neurophysiological level where we describe how any such system of rules is realized in the structure of the brain.

Now, Chomsky's claim is that there is another level of rules beyond all possible introspection – but not neurophysiological, either. My claim is simply that any evidence for such a level would have to show its reality by showing its causal efficacy, and Chomsky has not said anything to show this – or, in this article, even indicated any awareness of the problem. Furthermore, the very biological analogy that he and I both find so appealing tends to undermine the assumption that there is any such level. If there really is a language organ in the brain sufficient to constrain the grammar of any possible human language, then there is no need for a universal grammar; and if there really is a universal grammar, then it can only be because the language organ is not by itself sufficient to produce the constraints without an intervening level of rules.

As my previous discussions of Chomsky have tended to be misunderstood, I will conclude by blocking some possible misunderstandings:

1. I have not claimed that genuine rules must be accessible to introspection. I have claimed that where they are not accessible to introspection there ought to be at least some other sort of evidence that the agent is following the rules.

2. I have not denied that geniune rules could be innate or "wired in." I have rather claimed that genuine rules must play a causal role in the production of the behavior they explain.

3. I have not denied the existence of universal grammar or of a computational module in the mind. I have simply called attention to some unanswered questions in the attempt to establish their existence.

by **Elliott Sober**

Philosophy Department, University of Wisconsin-Madison, Madison, Wisc. 53706

Representation and psychological reality

In this target article, Chomsky provides an interesting formulation of the idea of psychological reality: one that is meant both to clarify what is involved in attributions of psychological reality to linguistic hypotheses and to defend such attributions from the philosophical criticisms to which they have been subjected. Chomsky believes that a grammatical hypothesis is psychologically real in just the sense that a physical hypothesis is physically real. Claims of physical reality, in turn, have two, unproblematic, components: for a hypothesis to be physically real is for it to be physical in its subject matter and true. Applying this analogical analysis to the idea of linguistic hypotheses having psychological reality, we get: (i) linguistics is a branch of (individual) psychology; (ii) for a psychological hypothesis to have psychological reality is nothing more or less than for it to be true.

In this brief space I won't take up the issues involved in (i). There are two conceivable alternatives to Chomsky's psychological interpretation: that linguistic hypotheses describe social regularities, and that they describe abstract mathematical structures that are no more psychological in character than the axioms of number theory. The thought behind (ii) is to show that if one finds plausible so-called "realist" philosophical doctrines about science generally, then one should have no particular qualms about the idea of psychological reality. A realist about a particular science will hold that it is the task of that science not only to construct hypotheses that accurately predict observable phenomena, but also to formulate and confirm conjectures about the underlying causal mechanisms that are responsible for the observed regularities. For Chomsky, hypotheses about the internalized grammatical representations of a speaker-hearer, though couched in mentalistic vocabulary, will nevertheless be about the underlying physical mechanisms (now largely unknown) that are responsible for behavior. By analyzing "psychological reality" in this way, Chomsky hopes to demystify the concept: a psychological hypothesis will be entitled to this label if it is the best explanation available. There is no special problem about the concept, once one has accepted a realist view of psychology.

In this brief space I want to describe how Chomsky's analysis of "psychological reality" departs from what I think is a fairly standard construal of the idea. This familiar formulation arises from distinguishing between someone's following a rule and someone's acting in conformity with a rule. The former idea, but not the latter, involves the idea that the person has some *mental representation* of the rule that plays a certain causal role in determining behavior. Although there may be many grammatical rules to which a person's verbal behavior (ignoring slips of the tongue and other performance interference) conforms, only some of those rules have psychological reality; these are the ones that are internalized in thought and play some appropriate causal role.

If we construe psychological reality in this way, then there are two objections to Chomsky's analogy between psychological reality and physical reality. The first one has to do with their possessing different degrees of intensionality: If *X* is a law of nature that has physical reality, and if *Y* is logically equivalent to *X*, then *Y* too has physical reality. However, if *X* is a grammatical rule that has psychological reality, and if *Y* is a rule that is logically guaranteed to be input-output equivalent to *X*, it does not follow that *Y* has psychological reality. In this, psychological reality resembles other mentalistic properties: for example, someone may *believe* or *doubt X*, but not bear the same relation to *Y*, even though *X* and *Y* are logically equivalent. This difference between the concept of physical reality and that of psychological reality suggests that there are special issues that need to be taken up if one is to clarify and defend the idea of psychological reality. The parallelism between the two concepts is not total.

The construal of psychological reality as involving the idea of mental representation, and Chomsky's analysis, which interprets psychological reality to mean psychological in subject matter and true, differ in a second respect. What is it for a hypothesis to be psychological in its subject matter? One way to answer this question is to imagine a language in which all the predicates express phonological, syntactic, and semantic properties and relations. Any sentence formulable in this language that is not a logical truth will be linguistic in its subject matter. Will each of the truths expressible in this language be psychologically real? I think not. Some constraints on humanly possible languages issue from the physical constitution of the vocal organs, for example. Some phonological rules will be true in virtue of these physical constraints. These rules will not have psychological reality, in that they will not be mentally represented. For a hypothesis to be psychological in its subject matter is for it to be formulable in terms of some proprietary vocabulary. But for a hypothesis to be psychologically real is not simply for it to be psychological in its subject matter and true. A psychological law may be true even though it is not mentally represented; numerous other characteristics of the organism may be responsible for its being true. Thus, if we understand the idea of psychological reality as involving the idea of mental representation, there will be truths of psychology that are not psychologically real. This argument against Chomsky's analysis of psychological reality is quite consistent with the physicalist assumption that hypothesized psychological mechanisms are in fact identical with physical mechanisms. Perhaps every truth of psychology is true in virtue of physical structures of the organism. Nevertheless, only some of those truths have psychological reality.

by **Stephen P. Stich**

Department of Philosophy and Committee on the History and Philosophy of Science, University of Maryland, College Park, Md. 20742

What every speaker cognizes

Much of the story that Chomsky tells in *Rules and Representations*, he has told before. But no matter. The story is a profoundly important one, and it improves with retelling. The basic features of Chomsky's account of the mind are convincing, many of the details are not.

Let me begin with an issue where the distance between Chomsky and some of his critics has grown progressively smaller. Can a speaker be said to know the rules of his grammar? Many philosophers have argued that the answer is no (see Nagel 1969; Schwartz 1969; Stich 1971; Cooper 1975). The basic strategy of the critics has been to note features that are typical of unproblematic cases of knowledge but that are absent in the relation between a speaker and the rules of his grammar. Given these disanalogies, the critics have argued, it would be inappropriate to call the relation between a speaker and his grammar *knowledge* unless there were extenuating circumstances: special features of the case that made the term *knowledge* appropriate despite the disanalogies. Finding no such extenuating circumstances, they conclude that speakers do not know the rules of their grammar. (For a detailed version of this argument, see Stich 1971, secs. IV–VI; for a reply, cf. Graves et. al. 1973.)

Now, suppose that this argument is a telling one, as indeed I think it is. Would Chomsky lose anything worth keeping by simply conceding the point? It is central to Chomsky's view that organisms have rich systems of internal representations that are exploited in varying ways in action, perception, cognition, and so on. It is also important for Chomsky that in humans one of these interacting systems internally represents the rules of the speaker's grammar. But whether the grammatical information is *known* or is merely internally represented in some way that does not count as knowledge surely is of lesser moment. Unfortunately, some of Chomsky's critics (myself included) have written as though the argument showing that speakers don't *know* the rules of their grammar also shows that the rules are not internally represented in some other way that does not warrant the *knowledge* label. However, this additional claim is simply a nonsequitur. Once it is conceded that the argument against *knowledge* of grammar entails nothing about other ways in which grammar may be internally represented, Chomsky could simply drop the knowledge claim altogether. And, indeed, this seems to be the position he adopts in *Rules and Representations*, where he introduces the term "cognize" to cover both knowledge and other sorts of internal representation.

The remaining disagreement with Chomsky on the topic of knowledge turns on a pair of claims he makes about the difference between knowledge and other sorts of cognizing. He suggests, several times, that the only difference between knowing a rule or proposition and internally representing it in a way that does not count as knowledge is that a known rule is accessible to consciousness, while a cognized (but not known) rule is not. He goes on to suggest that the distinction between knowledge and other forms of cognizing is of little importance. I think both claims are mistaken. There is a second feature of the propositions and rules we know (or believe) that distinguishes them from propositions and rules internally represented in some other way. The former, but not the latter, form a system that is *highly integrated inferentially*. Perhaps the briefest way to explain this notion is with a simple example. If I believe both q and *if* p, *then not-*q, and if I subsequently come to believe p, substantial stress is put on my cognitive system, and there is a substantial probability that I will accommodate this stress by coming to believe *not-*q (and ceasing to believe q) or by ceasing to believe *if* p, *then* q. Contrast this with the situation in which I start out cognizing q (but not believing it) and believing *if* p, *then not-*q. We might, for vividness, suppose that q is some appropriate formulation of Chomsky's "principle of locality," represented in the speaker's grammatical-processing "organ." In this case, coming to believe that p will put no comparable stress on my cognitive system, since the cognized representation of q is inferentially insulated from the beliefs p and *if* p, *then not-*q.

The fact that systems of cognized representations that are not believed also are not inferentially integrated with the body of a subject's beliefs or with other systems of cognized (but not believed) representations, suggests a picture along the following lines. Our mental apparatus is divided into a number of distinct components. Among these is a store of beliefs that are well integrated inferentially and generally accessible to consciousness. There are other components that serve special functions: grammatical processing, visual signal analysis, integrating motor activity to produce actions, and so forth. Each of these components may have a rich store of internally represented information. And, of course, the components interact with each other in complex ways. However, each component has only very limited access (or sometimes perhaps none at all) to the information stored and utilized by the other components. This picture (developed further in Stich 1978b) is of a piece with the "homuncular functionalism" advocated by Dennett (1978) and Lycan (in preparation), and is thoroughly congenial to Chomsky's "mental organs" view. The differences between beliefs, which are accessible to consciousness and inferentially integrated, and other forms of cognizing, which are neither, give us reason to suspect that beliefs are subserved by a distinct mental organ.

I fear that all this agreeableness may make for dull reading. So let me now turn to a topic where the clash with Chomsky's views will generate a few sparks. It is Chomsky's view that "grammars are internally represented in the mind." Why should we believe this? Here, I think, the question can usefully be divided into two parts. First, why should we think that some rich system of grammatical information is internally represented in the mind? Second, why should we think that what is internally represented is a *grammar* of the sort that might be produced by a Chomsky-style linguist, whose principal data are the utterances the speaker makes and the judgements the speaker renders about the acceptability of sentences, the relations among sentences, and so on. In answer to the first of these questions, Chomsky offers what I take to be a compelling argument. *It is the only game in town.* We simply have no serious idea how the complex system of linguistic judgements and discriminations that a speaker makes could possibly be made by *any* complex object (be it organism or automaton) that did not have a rich and intricate store of grammatical information. It is, of course, logically possible that some future Newton of cognition will discover a way in which these discriminations and judgements could be made by a system lacking an internal store of grammatical information. But that shows no more than that the internal mental representation hypothesis is an empirical hypothesis; like all empirical hypotheses, it might turn out to be false. It remains the case that there is no currently available substantive alternative to the assumption that the speaker has a great deal of internally represented grammatical information.

But now what about our second question? Granting that some very rich store of grammatical information must be internally represented in the mind, why should we think that what is represented is a Chomsky-style grammar? On my view there is little to recommend the claim that what we have internally represented is a grammar. For there are indefinitely many other ways in which grammatical information may be stored and utilized by the mind: alternative systems of rules, systems of propositions, frames, and still other alternatives as yet unexplored or unimagined. Which of these is actually used is an empirical question. But it is an empirical question that we are only beginning to address. What is more, it is not a question that *can* be answered if we restrict ourselves to the data and techniques of the generative linguist.

An example should make all of this a bit clearer. Winograd's (1972) SHRDLU patently has a great deal of internally represented information about English grammar, but it does not have an internal representation of a Chomsky-style transformational grammar of English. Of course, SHDLU's mastery of English is far from complete. But there is no reason to suppose that an even more impressive SON-OF-SHRDLU, with a much more complete mastery of English, would have to include a grammar of English. Now suppose that student-of-Winograd has constructed such a SON-OF-SHRDLU which, in addition to its greater linguistic prowess, also has the capacity to answer questions about its own linguistic intuitions. Finally, suppose that SON-OF-SHRDLU's intuitions are a fair match to those of a native English speaker, say mine. What sort of a grammar would be written by a Chomsky-style linguist who has been given the task of producing a grammar for SON-OF-SHRDLU? Obviously it would be much the same generative grammar the linguist would produce using me as his informant. Yet SON-OF-SHRDLU has no such grammar internally represented. Is there, then, any reason to think that *I* have an internal representation of the linguist's generative grammar? Clearly, the answer is no.

Before leaving this topic, two points need stressing. First, I take the question of how grammatical information is represented in our minds to be an empirical and not an altogether intractable one. By studying processing times, mistakes, confusions, the effects of brain damage, and so on, we can learn a great deal about how grammatical information is represented. The traditional data of the generative linguist – actual speech and intuitive judgements – are relevant too, of course. What I have been arguing is that a theory that restricts itself to these "traditional" data cannot decide among a broad range of theories about how grammatical information is represented. The second point to stress is that there is some danger that this kind of disagreement can degenerate into a purely verbal dispute. It is possible for Chomsky to reply by insisting that the clause "grammars are internally represented in the mind" did not refer to the sorts of grammars he and other generative grammarians now write on the basis of linguistic evidence. Rather, the reply would continue, it referred to the sorts of grammars that would be written by cognitive simulators who gave due attention to the full range of potental psychological data. I think this is an unlikely interpretation, but if it is nonetheless the interpretation Chomsky actually prefers, then I must agree that grammars (in this sense) are internally represented in the mind. But if we paper over the dispute in this way, it quickly emerges elsewhere. For, given our new sense of "grammar" on which the right grammar simply is the system of rules, propositions, or whatever that actually are internally represented in the speaker's mind, I would contend that we now have almost no idea about the details of the grammar for any natural language.

Let me turn, now, to Chomsky's "argument from the poverty of the stimulus." Here again I am partly in agreement: His question-formation example, his "each other" example, and a large number of other examples that he has elaborated elsewhere make it overwhelmingly clear that the grammatical knowledge the speaker ultimately ends up with (however it may be represented) simply could not be *inferred* from the data available to the child by a rational person, unless that person was initially provided with a rich additional body of information. And to show this is, as Chomsky has frequently contended, to show that no empiricist theory of language acquisition could possibly be correct. However, Chomsky often draws a very different sort of conclusion from the argument from the poverty of the stimulus, a conclusion that the argument simply will not bear. The essential strategy of the argument from the poverty of the stimulus is to locate some relatively abstract principle, like the principle of locality, which has a pair of properties: i) the speaker's language clearly accords with the principle, and ii) it is wildly implausible that in acquiring his language the speaker had sufficient evidence to infer that the principle characterized the language of his elders. Since the data provide insufficient evidence (or perhaps no evidence at all), it follows that innate features of the language-acquisition mechanism must play a large role in the explanation of why the speaker acquires a language that accords with the principle. All this seems correct. But Chomsky often seems to infer something more: viz. that the principle that is radically underdetermined by the evidence available to the learner must be a *universal* feature of all languages. This inference, however, simply does not follow.

Perhaps the simplest way to see the point is to consider a simplified and rather fanciful example. Suppose that there are two quite different categories of languages that humans can learn. All the languages in category A exhibit the principle of locality, the principle of opacity, and the rest of the "universal" principles Chomsky and his co-workers discern in English. The languages in category B, however, exhibit none of these "category A universals"; they all exhibit quite a different set of abstract and nontrivial "universal" features. One of the innate principles of the language-acquisition mechanism instructs the system to restrict itself to either category A grammars or category B grammars as a function of some quite arbitrary feature of the stimulus (or "primary linguistic data"). For concreteness we might imagine the innate instruction to read: "If the word for mother begins with a consonant, the right grammar must have category A features; if the word for mother begins with a vowel, the right grammar must have category B features." Now there is, we will suppose, no way in which a person initially uninformed about this conditional principle could inductively infer, from the data available to a child in an English-speaking community, the conclusion that the language being spoken exhibits category A "universals." The child's innate knowledge is heavily implicated in the explanation of the fact that he comes to speak a language that exhibits category A "universals." But these category A "universals" are not real universals at all. Ex hypothesi there are many languages that do not exhibit them, but that a child put in the proper environment could learn with equal facility. It would be natural enough to say that both category A principles and category B principles are innate. What I have been arguing, then, is that innate principles need not be universal. (For an elaboration of this argument cf. Stich 1978, 1979).

by **Robert Van Gulick**

Department of Philosophy, Rutgers College, Rutgers University, New Brunswick, N.J. 08903

Knowledge and learning

As the concepts of *knowledge* and *learning* play an important role in Professor Chomsky's discussion, it will be useful to inquire further about each of them as well as about their interrelation. I believe the two notions are correlatives, and thus I will try to exhibit their mutual conceptual and theoretical interdependence in a way that, I hope, clarifies several issues raised by Chomsky.

Most philosophical treatments of knowledge add to a requirement of true belief or accurate representation some further conditions concerning the origin or support of the belief or representation. Both justification accounts and causal theories of knowledge place constraints on the sorts of belief production processes that can be thought of as leading to knowledge. Our first point of contact now emerges. For a learning process is a process of acquiring beliefs or information. And the beliefs acquired must more often than not be true if we are to count the process as one of learning. Learning, like knowledge, is a success-loaded concept. Indeed, there seems some basis for understanding learning as a knowledge-acquisition process. Thus the two questions: "When is an information acquisition process a learning process?" and "Which processes are of the sort that can produce knowledge?" are intimately interrelated, though we need not assume them to have a common answer.

In addressing these issues, it will be best to retreat to some notion that abstracts the concept of knowledge somewhat from considerations about its production. I suggest that we focus on the idea of *information possession,* which I take to be more general and illuminating here than that of true belief. Put crudely, an organism (or system) possesses information about some fact if its structure is such as to modify or regulate its behavior in ways that are specifically adaptive, given that fact. Information possession is a matter of the organism's being structured or "shaped" to interact successfully with the world along the lines of its ends or interests. (For further discussion of such a notion of information, see Konrad Lorenz, 1965.)

So conceived, the question of when an organism ought to be described as having knowledge or possessing information seems less pressing. Such descriptions will be of wide applicability in characterizing organisms and their organizational structure. Rather, the interest shifts to determining how the content of the information possessed is to be specified, especially as regards its sophistication, Even if both I and the earth-worm at my feet can be said to possess information about the ground's being wet, the information we possess is clearly not identical in content. The crucial differences between us concern the much wider variety of ways in which the information I possess can adaptively affect my behavior – especially the ways in which its behavioral role depends on its interaction with the other items of information I possess. A high degree of such inferential connection is particularly needed if we wish to think of this information possession as belief or to specify its content by the use of "that" clauses (i.e., "that" followed by a sentence, as in "Jones believes that Smith is tall.").

Given this admittedly crude account of information-possessing, we can detect difficulties with Chomsky's concept of "cognizing." The technical term "cognize" is apparently introduced to sidestep difficulties that might arise concerning the common-sense term "know." Yet it should now be clear that the theoretical problems remain. The language faculty may be said in some sense to *embody* the rules and representations of the grammar in its operation, but how we ought to specify the *content* of what information it possesses or what it cognizes *about* those items will require a detailed account of the processes within the faculty that operate on those items. Chomsky's remark that we would not hesitate to say that any person who became conscious of the grammar and its rules knew them thus begs the question and misleadingly suggests that any controversy here merely concerns conscious or introspective access. Access to consciousness is relevant only insofar as the contents of our conscious mental states are of a sort that reflect their rich inferential relations. It is a long step from claiming that our best theories of the language faculty describe it as embodying thus-and-such rules and representations of a grammar in its processes, to any claim that the language faculty cognizes *that* the rules or grammar are thus-and-such. Much more would be needed to justify the assertion that the information possessed in such cases is at all comparable to that associated with standard intentional attitudes. The shift from "know" to "cognize" leaves intact all those problems about how to specify the content of what is cognized, which can only be resolved by appeal to detailed theories about the internal processes involved. A similar difficulty arises with respect to Chomsky's cognizing rocket, which is said to "incorporate an explicit theory of the motions of the heavenly bodies." One would need to say more about the case, especially about how and in what ways the rocket was able to apply this information it possessed before we could specify the content of that information. It is quite unclear whether or not we should think of it as realizing an informational state at all like that realized by a competent human scientist who knows or cognizes the theory. I do not mean to say that Chomsky argues for such an equivalence; my intent is only to emphasize that the difficult questions here are those concerning the specification of content and that these problems apply as much to the notion of cognizing as they do to cases of knowlege or information possession.

Using our notion of information possession, we might describe a learning process as one that leads to the acquisition of information. Learning might be said to have taken place whenever some real-world feature induced a change in an organism that modified its behavior in ways specifically adaptive to that feature. Such a characterization would be overly broad to provide the sort of contrast Chomsky desires between knowledge acquired by learning and by growth. His own characterization of what is to be counted as a learning process is partly based on a specification of stereotypical cases of such processes: association, induction, conditioning, hypothesis formation and confirmation, and abstraction and generalization. Is there some principled reason for grouping such information-acquisition processes together and thinking of them as distinctively learning processes? What theoretically relevant features might they share?

One thing they have in common is a high degree of generality; they are processes that are not specific to any particular subject matter or type of information-acquisition problem. They can all be thought of as methods for extracting information from environmental inputs, but as themselves embodying the possession of very little or no information about that environment. They thus conform to a traditional empiricist picture, still influential today, which rather sharply distinguishes between factual knowledge and the relatively pure a priori methods for arriving at that knowledge – methods which are thought of as relatively uninfected by actual empirical content. Attacks upon this view have come with the recent interest in causal theories of knowledge and naturalized epistemology. One approach has been to specify the acquisition conditions in accounts of knowledge by reference to *reliable* belief-production processes (reliable, that is, in the sense of leading regularly if not invariably to truth). Crucial here is the recognition that reliability is *context-relative.* That is, whether or not a given production process is reliable depends on the actual real-world environment within which it operates. Part of the motivation has been to answer skeptical challenges to claims of knowledge by arguing that the skeptical counterpossibilities need not be taken into account in assessing reliability. The point of interest for our questions about learning is that, if a given belief production process is reliable in its actual environment but would not be so generally, its presence in an organism can be understood as a case of phylogenetically acquired information about that environment. Thus, insofar as both the methods of the traditional empiricist picture and those methods that seem paradigmatically to be cases of learning processes share a high degree of generality, they might be thought to embody the possession of less information of any specific sort about the environment than do other reliable belief or information-acquisition processes. The result is one Chomsky should find congenial. For the internalization of a grammar might be thought *not* to be a case of knowledge acquisition by learning, either because no new information was acquired or because the information was acquired by a process that was not of a sort appropriately characterized as learning. However, these two possibilities can now be seen to be somewhat collapsible. For while there remain distinctions of degree, the farther we move from paradigmatically learning processes, the more antecedent information we must recognize as having been *embodied in the relevant acquisition process.*

One last point requires comment. In his discussion of selective and instructive processes, Chomsky suggests that, on analysis, it may turn out that learning is not really learning. His reasoning is that, on analysis, instructive processes that he equates with learning may turn out to be realized at underlying levels by selective (i.e. nonlearning) processes. Such discoveries may well occur, but they would not by themselves show that the higher-level processes were incorrectly thought of as learning, anymore than decomposing a feedback process into underlying nonfeedback processes would show that the former was not indeed a feedback process. Descriptions that are appropriate to a process at one level need not equally apply to the processes at levels that underlie their realization. It may well turn out that far fewer of our ways of acquiring information are really learning processes than is generally supposed. But we cannot conclude that learning is not really learning if decomposition should show it not to be learning all the way down.

Author's Response

by **Noam Chomsky**

Department of Linguistics and Philosophy, Massachusetts Institute of Technology, Cambridge, Mass. 02139

The new organology

I borrow the title of this Response from **Marshall's** elegant demonstration that there is nothing new under the sun. I would also like to emphasize his closing remark that "some way surely has to be found in which results phrased in the languages of linguistics, psychology, and physiology can be made to bear upon each other." As a number of the commentaries make clear, current work in epistemology, philosophy of mind, and philosophy of language promises to make a substantial contribution to this inquiry; and vice versa, I believe.

As noted in the abstract, the target article was excerpted from a longer work (Chomsky 1980). Omitted completely was a chapter outlining some ideas about universal grammar (UG) and its realization in the particular case of English, and also considerable discussion of alternative views and other topics. The excerpted discussion of a research program is perhaps a bit misleading, in that it understates the specificity of hypotheses that have been investigated in pursuing this program. These matters go beyond the excerpt, and indeed beyond the book from which the excerpts were taken, which was concerned primarily with conceptual issues rather than systematic presentation of elements of a theory of universal or particular grammar. In the technical literature, particular questions are investigated in considerable detail. My intention here was to discuss a general framework in which these technical studies can be understood, in my view. A number of commentators noted that my discussion was quite short on facts and specific proposals. That is correct, but it reflects in part the specific purposes of this discussion. I gave only a handful of facts as illustration: namely, a few properties of anaphoric expressions and questions. I will refer to these below simply as "sample facts." They are selected, for illustration, from a wide array of similar cases, and there are some fairly definite proposals in the literature, not discussed here, with regard to the rules and principles that may account for them. In short, I would not want to leave the reader with the impression that we can go no further than the qualitative remarks presented here with regard to the nature of UG and particular grammars.

Given the variety of issues raised and points of view expressed in the Commentary, it seemed best to respond individually. This leads to some redundancy, but perhaps it will facilitate comparison of the commentaries and responses.

Andor. Andor suggests that "our semantic and pragmatic knowledge and intuitions" have a "more crucial role in comprehension than syntactic factors." I do not know how to measure relative contributions, but let us suppose that this is true. Nothing follows with regard to what I have proposed. It remains a valid problem to determine the nature of the rules that he calls "syntactic" and to find their precise scope and character, as well as to ask how they interact with "semantic and pragmatic knowledge." Andor states correctly that in this paper I have nothing precise to say about grammar; see the introductory comment, above. Perhaps Andor is right in proposing that our judgments and behavior involve "cognitive filters" in his sense. If he is also right in his beliefs about the nature of these "cognitive filters" (namely, that they are highly individual, based on "scenes and frames" that vary widely depending on external factors), then the study of these "filters" will be more informative about the nature of the environment than about the nature of the mind. If he is right, then, one who is more interested in the nature of the mind will therefore be more concerned with other properties of the attained and initial state, however large or small their contribution to evaluation, expression, and comprehension – specifically, with what Andor calls the "syntactic factors," including those that enter into the representation of sound and meaning.

Cromer. Before discussing Cromer's contribution, I would like to clear away several misunderstandings. Let us distinguish two notions: "poverty of the stimulus" and "degeneracy of the stimulus." The first is the concept discussed in my paper. The stimulus is "degenerate" if the data-base for language acquisition contains expressions that are not well formed. Cromer states that in earlier work I had claimed that "the input stimulus [is] composed mainly of pauses, hesitations, and false starts," referring to Chomsky (1967). He states further that "most psychologists and psycholinguists," misled by these "claims," have understood "poverty of the stimulus" to mean what I am now calling "degeneracy of the stimulus." Perhaps most psychologists have been misled, as he says, but hardly on the basis of the reference he cites, where the discussion in the text concerns only poverty of the stimulus, and the matter of degeneracy is relegated to a footnote (in which, incidentally, there is no claim that the stimulus is "mainly" degenerate). Thus, it is simply not the case that my arguments for innateness, there or elsewhere, "are primarily based on the poverty of the input stimulus" in the sense of degeneracy. The latter is a marginal issue and has always been presented as such.

Cromer points out that "accumulating data" show that when people speak to children, they generally speak simply and carefully – hardly a surprising result, one might have thought. But it definitely has not been shown that these simplified data constitute the data-base for language acquisition, or that such data are a necessary condition for language acquisition. There is no evidence for this, and there is evidence to the contrary (see Newport, Gleitman, and Gleitman 1977). Thus, the fact that people tend to speak clearly and simply to children does not show that the "input language" – that is, the data base for language acquisition – "has now been shown by careful mother-child interaction studies to be clear and to be free of hesitations, false starts, and the like." Furthermore, as Wexler (1978) notes, such simplified data are far from serving as an "ideal" data-base; if the data were restricted in this way, the problem of explaining how a child gains knowledge of language would simply be rendered a deeper mystery. But we need not pursue the matter, since what is crucially at issue is poverty, not degeneracy of the stimulus.

A second misunderstanding appears in Cromer's statement that I argue against "hypothesis-formation and confirmation" as explanation for language acquisition "primarily on philosophical and logical grounds." First, I do not argue against this approach at all; cf. p. 14 of the target article. Rather, I raised the question of how such an approach could be distinguished from a growth theory. Second, I have argued against various specific "learning theory approaches," but on factual grounds.

These misunderstandings aside, one can only welcome

Cromer's insistence on careful attention to data on language acquisition. His commentary is enlightening, in particular, in pointing out the hazards of interpretation, as in the case of strategies used by children who do not yet comprehend particular language structures, and in emphasizing the disparity between the child's rule systems and the data available (analogously, the resistance of natural conservers to direct contrary evidence).

As for the "preformationist" versus "epigenetic" approaches to innateness and growth, I take no stand (here or elsewhere), agreeing that there seems no current possibility of distinguishing them empirically.

There are many different ways to approach the questions of innateness and acquisition; individuals will pursue one or another course on the basis of their evaluation of prospects and their own predilections. It seems to me that for the present, the most productive way to develop precise hypotheses concerning the initial state of the language faculty, hence the basis for language acquisition, is by detailed investigation of explanatory theories of the state attained, applying the useful (though obviously nondemonstrative) argument from poverty of the stimulus. But I would not try to urge this personal judgment on others, and, as my article should make clear, I am completely receptive – as any rational person must be – to results emerging from other lines of study.

Cummins and Harnish. C&H agree that "there is a language faculty (LF) characterizable via a system of rules and representations, the structure of which is largely innate or innately determined." Therefore there is a valid field of inquiry, call it LF-theory, which seeks to determine the nature of such systems in the mature state and their innate determinants in the initial state, these being matters of fact. State of what? I would say: state of the mind, ultimately brain. Thus, hypotheses about the mature system (the grammar) and the innate determinants (UG) are hypotheses about the mind/brain. True, one can imagine alternatives. Perhaps LF really resides in the liver, or is somewhere in outer space. I see no point in pursuing such possibilities, for obvious reasons, and therefore continue to assume that the LF that C&H and I agree exists is "characterizable via a system of rules and representations" that is internally represented in the mind/brain.

C&H appear to disagree with these moves. They feel that "linguistics" need not be "about the LF." With that, I agree. I would not want to legislate the usage of the term "linguistics." It is of no concern to me whether LF-theory is called "linguistics," and contrary to what C&H assert, I see many alternatives to LF-theory; most of what is called "linguistics" is not directed to this end.

C&H argue that the real issue concerning "psychological reality" has to do with the reference of the term "linguistics" – is "linguistics" to be construed as LF-theory, which is a valid field of inquiry on their assumptions, or as something else? About this terminological issue, I have no position and no concern. My concerns, rather, have to do with the issues concerning "psychological reality" raised in the literature to which I referred.

Note that, on C&H's grounds, one could also say that "psychological evidence" (e.g., results of reaction time or click experiments) has not been shown to bear on the "psychological reality" of theories accounting for it, since one might (perversely) construct such theories with no concern for their truth, say, as one or another way of axiomatizing some range of data. Thus in their terms, there is not only a way of construing "linguistics" so that it is not part of psychology, but also a way of so construing "psychology." As in their version of "linguistics," so in this version of "psychology," one need not be concerned about "psychological constraints" deriving from other studies, or about any of the problems that interest someone concerned to discover true theories.

What exactly do C&H mean when they say that there has been no adequate defense for the view that "talk about language is just disguised talk about shared psychological states"? They agree that there are grammars that are largely innately determined, and that these characterize LF. Insofar as "talk about language" is talk about these grammars, hence about LF, it is talk (not disguised) about psychological states, internal mechanisms, and so on. This follows from their assumption that these structures do exist and are innately determined. Their point, then, seems to be that talk about language need not adopt these concerns, which is no more interesting than the fact that talk about some range of physical data need not be concerned with determining true theories. As for the fact that promises obligate, this may be a fact about LF (hence represented in the grammar) or a fact about some other system, in which case knowledge of the fact will be represented outside of LF – a possibility that seems to have no consequences for the questions they are raising.

Perhaps C&H are taking the position that such facts as the "sample facts" could be facts about "English" (an infinite system), even if there were no representation of knowledge of English in the minds of speaker-hearers (or anywhere). They do not try to develop this possibility, so I will not pursue it. Cf. Chomsky (1980) for some discussion.

C&H cite the opening paragraph of my *Syntactic Structures* (Chomsky 1957), where a grammar is defined as a device that generates the sentences of an infinite set (a language). They conclude that "theories of language conducted to the letter (and in the spirit) of such remarks need no more be about the mind than a piece of set theory is." I would like to make clear that *Syntactic Structures* is explicitly concerned with LF-theory. It proceeds from the initial characterization of a grammar that C&H cite, to argue that a partial theory of language understanding can be developed in terms of levels of representation generated by grammars of an appropriate sort, and that the theory of such grammars (LF-theory) can have explanatory power in accounting for the way sentences are assessed and understood. Subsequent work has been devoted to the same ends. C&H assert that this work fails to provide "a proper characterization of what the LF contributes to 'linguistic capacities' broadly construed," and that we "lack a hypothesis" about the contribution of the LF component to the theory of understanding. I don't know what they would consider a "proper characterization," but it is simply not the case that we lack any hypotheses. There are quite specific hypotheses in the literature, from *Syntactic Structures* to the present, about the structures that are generated by the grammar and that enter into the understanding of language – hypotheses concerning anaphora, quantificational structure, assignment of thematic relations on the basis of abstract syntactic structures, and many other topics relevant to a theory of understanding.

C&H state further that I left "wide open" the possibility of a theory of understanding and communication (their CS) without an LF component, and they allege that one can develop theories of CS without consideration of LF. The first point is based on a misunderstanding. I pointed out that one can imagine a system lacking the computational devices of LF that could be used in a rudimentary way to communicate and, more significantly, to express thought over a finite range – e.g., a system with a finite list of names and predicates, the notion of reference and predication, and some associated use system. We might say that such a system has a trivial LF component, in which case an LF component is, to use their terms, always "implicated in the explanations proffered by the theory of CS." As for their second point, all work on their CS begins with assumptions, generally unanalyzed assumptions, about the form and structure of sample utterances that are investigated. There can be no principled objection, but surely such work does not undermine an approach that seeks to examine and account for the unanalyzed assumptions – e.g., the sample facts. One would hope that the latter study would contribute to inquiry into CS based on unanalyzed assumptions about the form and meaning of utterances. There have been some efforts by "communication theorists" to approach general questions about form and meaning in terms of speech acts and similar notions. These are, I believe, a dismal failure, begging all fundamental questions (cf. Chomsky 1975, chapter 2).

C&H believe that the study of "linguistic capacities" can be attacked "without bothering about the LF" – that is, without bothering about the system of rules and representations, largely innately determined, that specify the language and its grammatical properties (which, I have argued, include quite specific properties that enter into representation of meaning, – a conclusion that C&H do not contest). To an extent, this is true. That is, progress can no doubt be made leaving fundamental assumptions unanalyzed, simply considering examples that are taken to have certain structural properties, without concern for the rules and representations that determine these properties – that is, without concern for LF, which C&H agree exists and which determines these properties for the infinite class of expressions of the language. But surely the study of such capacities can only gain by integration into a more general theory that includes an LF-theory, just as the latter can only gain by facing the empirical test of incorporation into a broader theory that includes the study of speech acts. C&H appear to disagree; why, I do not understand.

Dennett. Dennett takes it to be obvious that "there are innately fixed structural features – design features – that specifically constrain the development of linguistic competence in the child," but it is, he says, "gratuitously strong" to propose that such principles as locality and opacity, or other proposed properties of universal grammar (UG), are innate. "Gratuitously strong" in comparison to what? Perhaps in comparison to some other principles that have been advanced to account for the "sample facts" and others like them. If this is what Dennett has in mind, I am sympathetic. In fact, though it is beyond the bounds of this discussion, I think that recent work shows promise of deriving such principles as opacity from deeper principles of UG. But this does not seem to be what Dennett has in mind. Rather, the proposed principles are "gratuitously strong" in comparison to some notion of "learning" and "design structure" that remains unformulated. One can neither agree nor disagree with this thesis, because it has no content at all.

Dennett feels that it would be "unwelcome" and "regrettable" if research were to show that such principles as locality and opacity were innate (in contrast to "design features" about which we have no idea at all), because this would leave "the task of explaining the genesis of the design in the organism" still to be faced. By the same token, it would also be unwelcome and regrettable if research were to show that our genetic endowment determines that we grow arms rather than wings, that we have the kind of digestive and circulatory system we do, that we develop a human rather than an insect eye, and so on. Note that even the most far-reaching proposals concerning innate elements of the language faculty add only a tiny increment to what is generally assumed without discussion to be attributable to innate endowment. Adapting Dennett's qualms to bodily organs, any specific proposal that is put forth about the genetic basis for developing a human rather than an insect eye should be rejected as "gratuitously strong" and "unwelcome" in favor of the thesis that some "design feature" about which no proposal is forthcoming is responsible.

Dennett agrees that "moving more and more structure into the category of *innate* may help us to get a more realistic picture of the individual capacity for cognition and learning." Thus, specific hypotheses concerning innate structure – say, those I suggested – might contribute to a more "realistic picture" of the problem I have been addressing: namely, the transition from initial to attained state in cognitive growth. But he would find this "disheartening" if true because of some unspecified assumption about the kind of "design feature" we are to assume innate, and because it remains to explain the origins of UG – along with the vastly more difficult problem of explaining the evolutionary origin of all bodily structures. It is curious to believe that it would be "disheartening" to discover that some specific structure results from millions of years of evolution, but "heartening" to learn that it arises through interaction with the environment in early life in some manner that remains a complete mystery.

All of this discussion seems to me completely pointless. I am interested in developing what Dennett calls a "realistic picture" of the basis in innate endowment for cognitive growth: a "realistic picture" of his "design features" that "specifically constrain the development of linguistic competence." Dennett offers no reason to suppose that these features should be of the type he has in mind, whatever that may be, rather than of the type to which recent research seems to me to point. Evidence available now seems to me to suggest that certain specific properties, such as those I discussed, are innate as part of UG. Dennett finds this conclusion "disheartening," even if it were to prove "realistic." To me it seems that such conclusions, which would provide the beginnings of an explanation of how we come to know what we in fact know, would be "welcome" and "heartening." In contrast to Dennett, I have no standard by which I can judge whether these proposals are "gratuitously strong" as compared to others that remain unformulated, and I see no reason to accept his view that it would be "welcome" if the attained state could be explained somehow in terms of organism-environment interaction rather than in terms of evolution of the species, whether in the case of arms and wings, the circulatory system, the visual system, or human language.

Ghiselin. When I suggest that the mind has a modular structure, I do not mean to imply that the language faculty is "radically different from other biological entities," as Ghiselin reads me as saying, but rather that, like others, it has certain distinctive properties. Thus, I would not expect to find in the language faculty the principles for face recognition, or in the visual system, the principles of opacity and locality. Conceivably, properties of one cognitive system relate to those found in others, as noted in the target article; thus, opacity and locality involve notions of "prominence" (namely, the special role of the grammatical subject) and boundedness of computation that might well appear elsewhere. We will be able to make better guesses about these questions when a number of cognitive faculties are studied to a sufficient degree of depth. Thus I am suggesting nothing that goes beyond standard assumptions about differentiation of organs.

As for the integration of the language faculty with others, this is beyond dispute. Even the most radical "modularist" takes for granted that the language faculty functions in the most intimate connection with other systems, including those I mentioned. Thus I do not hold that language functions "apart from other faculties." Rather, Ghiselin's analogy to the nervous and endocrine systems is reasonable, for my purposes.

With regard to the matter of individuals, species, and idealization, we are speaking at cross purposes. There is no doubt that when I dissect a creature, its properties exist apart from my thinking about them. This is not denied when I say that an individual organism is an "idealization reflecting a particular way of looking at things and processes in the world, which does not come ontologically prepackaged as a set of individuals with properties (essential or other) apart from our mode of conception." Ghiselin's formulation, which presupposes the identification of the creature and its parts, begs the question I am addressing. That is, once we have organized the world into individuals (in accordance with our mode of conception), these individuals have properties that are real (though we will perceive and comprehend them in a certain way, given our mode of conception). But the world could be viewed differently, with different individuals – a commonplace even within the framework of our modes of conception. We may think of the creature we dissect as an individual thing, or we may take a particle of food it ingests as an individual, tracing its course in an environment, part of which consists of parts of the creature. Or we can, and sometimes do, view the world in quite different ways. Nothing in this denies the reality of properties or things.

Nevertheless, the notion of "the creature I dissect" involves substantial idealization, or, to put it differently, a complex application of our modes of conception. We assume that it is the same creature, despite numerous physical changes; we distinguish the creature from the air it breathes, though the latter enters into its constitution, and so on. None of this troubles the person carrying out the dissection, because he simply adopts a certain mode of conception for specific purposes, though other ways of organizing the world might be appropriate for other purposes.

With this in mind, consider the matter of "defining" such notions as "human language" or "French." The former task poses questions of a familiar sort, as when we speak of the "human visual system" as distinct from that of cats or bees. In speaking in these terms, we abstract away from individual differences and from interconnections among systems, focusing on one (idealized) element of a complex integrated whole (which, as noted, represents one of many ways of organizing the world). We might postulate that some principle for identifying the structure of a body in motion is a property (perhaps innate) of the human visual system, and that some principle (say, opacity or locality) is a property of the human language system. In this respect, it seems to me useful and appropriate to think of the human visual system or human language as analogous to an organ or bodily system, and to try to characterize their properties, thus, in a sense, "defining the human visual system" or "defining human language." "Defining French" is another matter. There seems to me little purpose or hope in that. "French" is not a well-defined linguistic concept, but one that has crucial sociopolitical dimensions.

Note that I am not adopting the view that there are "defining" or "essential" properties of individuals, organs, or species; just properties. We might distinguish properties in terms of the contribution of genetic and environmental components, but that is a different matter. For some further discussion, see Chomsky (1975, chapter 2).

With regard to language diversification and speciation, I think we can now begin to go well beyond the traditional analogues, along lines noted on p. 9.

I am not sure that I see the import of Ghiselin's remarks about iterative structure. These raise questions about physical mechanisms that realize the abstract properties that we may hope to identify in the study of language: questions that are fair enough in principle but that are, so far as I am aware, still well beyond the bounds of direct investigation.

The same is true with regard to Ghiselin's interesting speculations with regard to UG. These are questions for the future. For the present, it seems to me that one primary task is to identify general properties that can plausibly be attributed to UG, one element of the human biological endowment. Any discovery about this topic will leave open many possibilities as to mechanisms and processes. It is implicit in the approach I have outlined that any results that might be obtained in a future "experimental embryology of language" of the sort that Ghiselin recommends would be a welcome contribution to this task, and that the relations between these fields should in principle be reciprocal.

Gottlieb. The work to which Gottlieb refers is not only congenial to the point of view I discussed, as he indicated, but also very suggestive as to how such a research program should be pursued. It would be quite interesting to ask whether concepts similar to those he mentions are relevant to the growth of grammar in the domains of syntax and semantics, as well as phonology.

Harman. The points that Harman makes are well-taken. He is, first of all, right in saying that my work sheds no light on analyticity. I am less convinced about what he calls "the real issue concerning analyticity." The question, I think, is not "whether assuming or supposing or postulating that something is true can ever make it true," but whether the conceptual system that develops in the mind through the interaction of innate and environmental factors in fact involves analytic connections and, correspondingly, determines analytic connections among expressions of language linked to these concepts. Such connections, if they exist, are not assumed or postulated, but just develop without choice. Thus it seems to me that we face a rather obscure factual question, difficult to resolve because intuitions are weak and theory is too tenuous to bear any significant burden. As for the example Harman discusses, my own (weak) intuitions suggest that if I kept you ignorant of the fact that you were leaving, so that at no point did you intend to leave, then I did not persuade you to leave (I did not succeed in persuading you); and that if there are no intentions, there is no persuasion. But I would not want to rest much on these judgments.

With regard to psychological reality, my main point is that no new problems of principle arise in the study of language that are not familiar in the "hard" sciences, and that evidence does not come in two epistemological categories: "linguistic evidence" bearing on "good theories," and "psychological evidence" bearing on "psychological reality." Harman and I agree, I believe, on these points. As for the first, as I noted, there are serious questions about what is meant when we take a theory to be true: "what is the status of its theoretical entities, its principles, its idealizations," and so on. Harman points out some of these questions, quite appropriately – though, I think, as his final example shows, it is misleading to say that "linguistic evidence" merely shows that "the theory brings order to a given domain" in any sense that does not hold as well for a theory of click experiments and the like. He is also right to emphasize that we may ask about the physical reality of elements of a theory that we take to be true, and that psychological reality is on a par with physical reality in this respect. In this connection, he correctly points out an error in my formulation: there is a question of physical (or psychological) reality apart from truth in a certain domain, as Harman explains.

There are interesting examples that go beyond notational variants in a narrow sense. Thus, suppose we assume the trace theory of movement rules (cf. Chomsky 1975; 1977). Consider two theories: (1) generate base structures, which are mapped to abstract S-structures including trace by transformations, with S-structures mapped to phonetic representations by the rules R_1 and to "logical form" representations (LF) by the rules R_2; (2) base-generate S-structures directly, mapping them to phonetic representation by R_1 and to LF by rules R_2 and R_3, where R_3 have the properties of the transformational movement rules (properties distinct from R_2, I believe). These two theories are not notational variants in a narrow sense, but it is not entirely clear whether they have different empirical content within the domain of "linguistic evidence," and it might be argued that on such evidence one should not attempt to choose between these theories but only to aim at a more abstract theory of which these are two specific realizations (for discussion, see Chomsky 1977, chapter 4; 1980, chapter 4; Koster 1978). Real and interesting questions of this sort arise when theories are given a fairly precise form, and Harman's comments are applicable to them.

Hudson. Hudson points out an apparent paradox in my commitment to a "minimalist" program in linguistics and simultaneous rejection of a similar approach to cognition. We should, I think, advocate a "minimalist" approach to cognitive development as well, seeking general principles or elements that enter into separate cognitive systems. Principles and elements should not be multiplied unnecessarily. But I think that to the (limited) extent that we have some understanding of specific cognitive systems, we find that they are based on somewhat different principles, and I think that this is not an unexpected result. In certain domains, central to human life, we attain complex and highly articulated systems of knowledge and belief, yielding subtle and delicate judgments and expectations over a vast (indeed infinite) range, and we do so on the basis of limited evidence. But the domains in question are different in the problems they pose, the opportunities they offer. Therefore, it would not be surprising to discover preprogramming of quite specific types for, e.g., identification of objects in space, expression of thought in language (and derivatively, communication), recognition of personality, and so forth. See **Moravcsik**'s comments on modularity.

Marcus's work, which Hudson cites, is important but does not reach quite as far as he suggests. That is, the constraints of linguistic theory do not literally "fall out" as a consequence of the structure of a deterministic parser. Rather, Marcus's parsing model has properties that are similar to certain constraints suggested in linguistic theory (in particular, the principles of locality and opacity). This is a provocative result deserving closer examination, which would, however, take us well byond the bounds of this discussion.

Lakoff. Lakoff divides his comments into two parts: on technical apparatus, and on general observations concerning the nature of mind. In the first category, his remarks betray very serious misunderstanding of the work he is discussing. The transition in my own work from 1965 to the present is radically different from what he imagines. I am not concerned now with assessment of this work, but with description of it. The mid-1960s theories to which Lakoff refers assumed base rules generating deep structures and transformations mapping these to surface structures. Both the base and transformational systems were quite unconstrained, permitting a wide array of possible base and transformational systems. In the years since, I and others have attempted to restrict the variety of both systems. Thus, so-called "X-bar theory" radically restricts the class of permitted base structures; and conditions on rules as well as "output conditions" on surface structure and "logical form" have permitted a substantial reduction in the class of possible transformations, perhaps approaching the limit of a single rule "Move A," where A is an arbitrary category. Completely misunderstanding these developments, Lakoff assumes that they lead to a reduction of the depth of transformational derivation and to making deep structures look "more like surface structures." But that is not at all true. In particular, the trace theory of movement rules, which is a central part of these developments, increases the abstractness of the structures (call them S-structures) that are formed by transformation and that underlie actual surface structures, and the rule "Move A" has roughly the same scope as the numerous specific realizations of it in the work of the mid-1960s with which Lakoff is familiar.

There have been, to be sure, significant changes in my own view as to the role of transformations. I argued in the late 1960s that it was a mistake to treat derived nominals in transformational rather than lexical terms, thus proposing a restriction of the depth of transformational derivations; and work of Kayne and others has brought to light new transformational processes (e.g., VP-movement in Romance causatives), thus extending the depth of such derivations. Other examples, of both types, can be cited. But the picture has no resemblance to what Lakoff describes.

Note that within trace theory a new question arises about the status of transformations; see the final paragraph of my response to **Harman.** Furthermore, arguments have been advanced in favor of reducing the disparity between deep and surface structure. Cf. Lightfoot (1979). But Lakoff shows no awareness of these issues, and his comments have no relevance to them.

Lakoff further believes that the reason for the allegedly "radical" shift in my views on the technical details of linguistics is that I have attempted to shore up a belief in modularity in the face of counterevidence produced by Lakoff and his colleagues, which showed that meaning and use "affected virtually every rule of syntax." Thus, to preserve modularity, it was necessary for me to "redefine and narrow the domain of syntax." As noted, Lakoff seems completely unaware of the actual character of the technical work to which he refers. Furthermore, where I have proposed restrictions on the scope (rather than the variety) of transformations – e.g., with regard to nominalization – the motivation was completely different from what Lakoff suggests, and in fact was internal to the language faculty, largely syntactic. Semantic facts too were relevant, in precisely the same respect as they have always been in my work, since the early 1950s – a matter that Lakoff has never understood (cf. Chomsky 1972a, chapter 3; 1977, chapter 1). What is more, these proposals were in sharp opposition to the tendency of Lakoff and others to use transformations very broadly – far too broadly, I believe.

Other changes in my own work have been motivated by discoveries about what might be called "the syntax of logical form," a matter that is internal to the independent language faculty, in my view. As for "the independence of syntax," which Lakoff regards as my "central modularity assumption," note that it is not even mentioned in the essay on which he is commenting. For my views on this subject, see many writings from Chomsky (1957) to Chomsky (1977, chapter 1). My actual concern here is with modularity of the system of rules that associates phonetic and semantic representation – what I called "the language faculty." A good deal has been learned, I think, about how elements of this relation are parcelled out among the several components of grammar and about the nature of these components, including their relative autonomy, and my own views have certainly changed on this matter over the past 15 years and will continue to do so, I presume. But there has been little change in my view concerning the aspects of language that fall within this sphere, apart from extensions, as many new phenomena have been discovered and analyzed. One might argue, perhaps correctly, that I have been overly conservative in this regard. My point, however, is that Lakoff's misunderstanding of the technical work is so far-reaching that his comments on it are completely irrelevant.

None of this, of course, impugns Lakoff's positive proposal:

that study of interdependencies and similarities among various aspects of cognition will prove more valuable than investigation of specific properties of particular cognitive systems (about which he is skeptical).

Matthews. Matthews's comments assume a sharper distinction between abductive theories and growth theories than I suggested; cf. p. 13 of the target article. But let us put that aside and turn to his more central points.

We agree that, *at some level*, much of what is called "learning" – and in particular, acquisition of grammar – should be characterized in a "nonintentional, presumably physiological vocabulary, describing learning as the growth of a mental organ under suitable conditions of sensory stimulation" (Matthews). But I do not see that this amounts to abandoning a "rationalist" account of language acquisition in which "the various processes and state changes thought to characterize the acquisition process are defined over ... contents [of a state]," and innate structure "is characterized intentionally in terms of both the *content* of a state and the learner's *relation* to that content" (say cognizing). Rather, these are characterizations at different levels; each may be correct at the appropriate level. Similarly, the fact that I know my name would not be challenged, but rather supplemented, by an account of how this knowledge is neurally coded. We might argue that a physiological account of the growth of a mental organ gives a specific content to the rationalist account in an intentional idiom; it exemplifies, at the level of mechanisms, what is meant by "cognizing."

To cognize (know) a grammar and its rules is to be in a certain state, and to gain that knowledge is to "grow" that state in the mind/brain. Similarly, suppose that a person knows that an object moving along a certain trajectory (say, a parabola) will emerge at a certain point as it passes behind a screen. The intentional idiom is appropriate for describing the person's expectations, predictions, and behavior. But we need not deny that there is a physiological characterization of the state of the person who has this knowledge, a characterization related to this specific element of his knowledge. Furthermore, it might be true that this knowledge is "ungrounded" and simply develops (perhaps as a result of triggering stimulation) as a consequence of certain neural coding that is innately determined, in which case it would be appropriate, I think, to say that the (implicit) knowledge that the object will emerge at a certain point is innate. My feeling is that in substantial domains of human knowledge – roughly, where we speak of "knowledge of" (of language, of the behavior of objects, etc.) – an account in terms of internally represented mental structures is often appropriate, and the properties of these systems may involve significant innate determinants. These mental structures can be regarded as characterizations of certain physical systems, which are the realization of what we describe in an intentional idiom, quite appropriately at a certain level. I see no conflict between reliance on a nonintentional physiological idiom at one level, and on an intentional idiom at another, either with regard to states attained or their growth.

Thus I do not think that the "rationalist" should insist, as Matthews states, that "it is content rather than mechanism that is innate." Rather, both are; the descriptions are at different levels. This rationalist, as Matthews states, will insist that "the intentional idiom [provides] the appropriate characterization of innate structure" at one level of description, but need not deny that there is an account in terms of a physiological vocabulary at a different level of description.

Matthews agrees that the intentional idiom may be appropriate with regard to the state attained (as distinct from the mechanical chess-player, where it is not). This, then, is a matter of fact, rather as in the case of the missile systems I discussed. It seems to me that exactly the same is true when we consider the initial state, or the transition from the initial to the attained state; that is, the appropriateness of the intentional idiom, at a certain level, is an obscure matter of fact. Suppose, to take Matthews's example, that the Structure Dependency Principle is an element of state attained, entering into the determination of specific cases of knowledge that such-and-such a sentence is well-formed with a certain meaning, while another is not. Suppose further that this principle is innate, certainly a possibility. Suppose further that some system of principles of this sort is innate and becomes a basic element of the state attained by fixing certain parameters on the basis of experience. Then it would seem that if the intentional idiom is appropriate (as Matthews assumes it may be) for the state attained, it is also appropriate for the initial state and the transition to the state attained, on grounds of the (assumed) success of a theory of language acquisition given in these terms. My feeling is that the most promising theories are of essentially this form. In no way do they deny realization in terms of physiological mechanisms, nor would they be refuted or shown inappropriate by the discovery of such mechanisms, with regard to the state attained, the initial state, or the transition between them.

McCawley. McCawley believes that in arguing against *tabula rasa* theories I have overlooked the fact that even the most radical empiricism attributes some kind of structure to the mind. But he is wrong. The point he makes has been repeated over and over by everyone who has discussed the topic, myself included (see, e.g., Chomsky 1965, p. 47). What has always been at issue, quite explicitly, is the character of the initial structure.

McCawley then states that my discussion has nothing to say about the origin of language-particular hypotheses and developmental steps. It is true that it has little to say, but then I must return the compliment – indeed, generalize it. I assume that the "origin" of language-particular hypotheses may lie, in part, in parameters associated with the principles of UG. These might be fixed by inspection. Consider, for example, the theory of Rizzi (1978) concerning a complex range of differences between Italian-type languages and English-type languages, which he proposes to explain in terms of a slight difference in the choice of categories relevant to a fixed-locality principle. If he is right, then exposure to certain constructions would suffice to fix the parameter, thus yielding one or another set of conclusions as to the linguistic facts. As for developmental steps, insight awaits more comprehensive and systematic analyses of stages attained prior to the relatively steady state that constitutes mature knowledge – a difficult research task, but one that has been addressed with some success.

As for the particular case McCawley mentions – namely, fixing the meaning of *assassinate* – I am aware of no alternative to the view that the concepts that enter into the meaning and conditions on their interconnections are available prior to the acquisition of the word, and that some of them (e.g., the "aitiational" elements) are primitive. If so, then the innate language faculty does make available a range of hypotheses as to the meaning – a narrow range, presumably, if the word is learned easily on the basis of limited evidence as to use. McCawley regards this as "implausible," without, however, explaining why or suggesting a more plausible alternative.

McCawley has missed the point of my remarks on distinguishing learning from growth in terms of properties of the state attained. As I noted (p. 13), we might do so by speaking of "learning" in the case where the state attained is a system of belief or knowledge, but "if we do, then it is not clear that any coherent notion of 'learning' will remain," for reasons given there. McCawley takes the criterial property of "learning" to be individuation; since our mind can acquire knowledge of several languages, acquisition of language is "learning" (so that if it turned out that "coordinate bilingualism" is impossible, rather only "compound bilingualism," in which knowledge of one language is built on knowledge of another, then first-language acquisition would not be "learning"). Clearly, this does not respond to the point I discussed. In fact, McCawley's proposal raises the problem discussed in a more severe form than mine did. The body can become accustomed to a certain style of food (say, highly spiced). But it can accommodate to several such styles. When I receive eyeglasses with a stronger correction, I slowly come to accommodate and to see without distortion, but I continue to see without distortion when the glasses are removed, so that my visual system is in "two states" in McCawley's sense. If such examples constitute "learning," in accordance with McCawley's criterion, then the prospects for a coherent notion of "learning" seem even dimmer than if we identify "learning" in the terms I suggested.

McCawley's next point is that general-purpose cognitive faculties (say, those involved in learning pairs of nonsense syllables) might also be involved in language acquisition. They might indeed, for example, in acquisition of idiosyncrasies, as I suggested. As for the "false dichotomy" I am charged with setting up, it is not formulated in the quote he cites or elsewhere. The question whether cognitive capacities correspond one-to-one with cognitive domains can be posed only if "cognitive capacity" and "cognitive domain" are independently characterized. I have not done so; nor has McCawley. Therefore, it does not seem to me that he has posed a substantive question.

For a response to McCawley's final point, about the psychological reality of underlying representations postulated in phonology, see p. 12 of the target article. In the case he mentions, certain principles of phonology lead to the postulation of an underlying representation for *right*, *righteous*, *height*, and so on, which accounts for such facts as the vowel quality in *right-righteous* (as compared with *contrite-contrition*), the pronunciation of the *t* in *righteous* (as in *question*, but not *ambition*), and the quality of the vowel and the form of the affix in *high-height* as compared with *wide-width*. As noted on p. 12, phonology is a finite system, so "linguistic evidence" is weak, but not vacuous, for the reasons mentioned there. What McCawley calls "psychological evidence" (distinguishing "linguistic" from "psychological" evidence in ways that don't make sense to me) would certainly be welcome (were it available), as would additional "linguistic evidence" of the sort discussed. The question is whether these kinds of evidence differ in epistemological category. McCawley evidently believes they do, but he provides no basis for the distinction.

Moravcsik. Moravcsik's commentary helps considerably, I think, to clarify the issues that lie at the core of these discussions, just as earlier comments of his have helped me personally to gain a clearer understanding of the nature of my own work, in ways that I have inadequately acknowledged. Moravcsik's work, some of which I cited, brings such concepts as "understanding" and "rule structure" to the focus of concern. His observations on the origins of the preference for what he calls "shallow theories" also seem to me plausible, as well as suggestive as a guide to further inquiry.

A. Morton. A. Morton suggests that "modularity tells against the promise of language as a paradigm of human skill." Here, the excerpting in the target article has been a bit misleading. In the full text (Chomsky 1980) I argue along similar lines that "knowledge of language may not be a central case [or characteristic kind of knowledge]; nor knowledge of or about anything else." There are some cognitive systems that seem to lend themselves to analysis in terms of systems of rules and representations; others may not. Where an approach in terms of rules and representations is plausible, the principles of organization and functioning may vary.

A. Morton also observes that "mental structure does not entail modularity," by which he means that "primary skills" may be variously combined in the systems that I have called "modules." I think the point could be better expressed in terms of "primary elements," since the notion "skill" seems to me too narrow and often inappropriate (locality conditions, for example, are not "skills"). But the basic point is reasonable. We know too little about mental structures to advance dogmatic claims, and we should certainly search for more basic elements that may enter into various cognitive systems, even if the latter are, as I tend to believe, quite tightly organized and internally structured in some cases, containing elements that may well be distinctive and unique. To advance one speculation, consider the language faculty and the number faculty, briefly mentioned in the target article and discussed a bit further in Chomsky (1980). Both involve the notion of discrete infinity; both appear to be outside the capacity of other organisms. It is imaginable that, at some early stage of human evolution, the capacity to deal with systems of discrete infinity by systems of recursive computational rules developed in the mind. This may have given rise to the number faculty (which was exploited only long after) as well as to the language faculty's computational capacity to generate an infinity of expressions, with compositionally-determined structural properties, form, and meaning. This would thus allow a more primitive conceptual system to be used for the expression of thought (and, incidentally, for communication) over a vast domain. If something like this is correct, then A. Morton's speculations are much to the point.

J. Morton. I am in sympathy, generally, with J. Morton's clarification of the notion of levels of characterization, though I would prefer a different formulation at certain points. Take the matter of "linguistics" and "psychology," and the question whether "linguistics is actually more abstract than psychology." Perhaps we can approach these questions in terms of David Marr's discussion of the varying levels in terms of which one may analyze and understand "a system as complex as a nervous system or a developing embryo" (Marr and Nishihara 1978). He and his colleagues distinguish four levels:

> At the lowest, there is basic component and circuit analysis—how do transistors (or neurons), diodes (or synapses) work? The second level is the study of particular mechanisms: adders, multipliers, and memories, these being assemblies made from basic components. The third level is that of the algorithm, the scheme for a computation; and the top level contains the *theory* of the computation. (Marr and Nishihara 1978)

They suggest further that "the top level is the most neglected [and] also the most important," and that current research is misguided in constructing algorithms without appropriate prior understanding of the top level. Their own impressive work on vision lends substance to this analysis (see, e.g., Ullman 1979, and the references cited in Marr and Nishihara). It is this work, incidentally, that I had in mind in several of the analogies to the visual system in the target article.

Adopting this framework, we may consider the study of grammar and UG to be at the level of the theory of the computation. But the same is true of some work in artificial intelligence (e.g., Marr and his coworkers), and parts of what everyone calls "psychology" (e.g., Osherson 1976; 1978; Keil 1979; Kosslyn, forthcoming). I don't see any useful distinction between "linguistics" and "psychology," unless we choose to use the former term for the study of the theory of the computation in language, and the latter for the theory of the algorithm (taking the other two levels to be the neurophysiology of language). As work progresses, we surely hope to find all sorts of interconnections, eroding any initial sense of disciplinary boundaries. For this reason, I am uneasy with J. Morton's proposal that perhaps "... linguistics is more abstract than psychology, and is better to be considered at a different level ...," and with a distinction between "psychological" and "linguistic" functions.

The question of localization of language function is one on which I am not competent to comment, but my own reading of the literature leaves me less skeptical than J. Morton appears to be. But even if it were to turn out that there are no specific areas of the brain in which language function is localized, still there are surely specific mechanisms involved in the representation of language knowledge and the capacity to use this knowledge, and we can hope to discover these. Hence the question of localization, while an interesting one, does not seem to me to have overriding importance. In fact, one might argue that localization is interesting only insofar as it leads to the discovery of specific mechanisms and their organization.

Rachlin. Rachlin has not read my article very carefully. For example, I nowhere refer to all "behavioral shaping" as "fine tuning" (cf. p. 3); nor do I regard behavior as "irrelevant" to "empirical work on mental states" – an obvious absurdity. There are many similar errors, but I will put these aside, and turn to the major points.

Rachlin's final comments come close to the heart of the difference between us. His "behaviorist views inference as useful only in going from observation to observation, whereas Chomsky seems to view observation as useful only (if at all) in going from inference to inference." If we replace his term "inference" by "explanatory theory," we have the beginnings of a sensible distinction between two points of view. It is true that I view observation as useful (for my purposes) only insofar as it provides evidence for an explanatory theory, and I therefore have no interest in catalogues of observations, which can easily be constructed on a massive scale. Nor would I regard theory simply as a device for going from observation to observation – say, a device for prediction. Rather, I find observations interesting insofar as they can be used to gain some understanding of the nature of the system. Observations of behavior are of interest to me insofar as they can be used to gain some understanding of the nature of the person carrying out the behavior. The way to gain such understanding is to construct a theory of the nature of this system, using what evidence we can amass (e.g., from behavior) to test and refine it. All of this seems obvious enough.

To return to the sample facts, I am interested in explaining, not merely cataloguing, the fact that "the men expect each other to win" means (roughly) that each expects the other to win, whereas "the men expect each other will win" is not well-formed, with the meaning that each expects that the other will win. These are facts that both Rachlin and I know. I want to know how we come to know them; what can we learn about our internal states from such sample facts? If I am right, an answer at one level is that we have an internal representation of a grammar that generates these facts, among infinitely many others; and an answer at a deeper level is that the language faculty, one subsystem of the mind/brain, is equipped innately with the principle of opacity, or perhaps some deeper principle from which it derives. An explanatory theory is itself a descriptive theory: it postulates properties of inner mechanisms, rightly or wrongly, and can therefore be confronted with evidence of varied sorts. I agree with Rachlin's characterization, if he means to say that I am interested in observations, say of the sample facts, only to the extent that they contribute to this further end.

Rachlin asks why it is "necessary" to develop explanatory theories of this sort, and to propose them to be true of the mind/brain (to "hold a behavioral function hostage in the human body," in his terms). The answer is that it is not necessary. One need not be concerned to understand or explain observations of behavior, just as some person might be interested in collecting insects or rocks with no further concern in mind; or one can conceive of an uncurious engineer who might simply be concerned to predict what some mechanism will do without caring how or why it does it. A child is different from a pigeon or a chimpanzee in that, presented with certain data, it will come to know the sample facts and myriad others like them. Presumably this is because the internal structure of the child is somehow different from that of the pigeon or chimpanzee, surely not merely at the level of sensory mechanisms, since the same results hold if the language input is recoded for a different system. It is not "necessary" to be interested in these properties of the child – that is, in psychology and biology – or to try to discover and understand them. But someone who is interested in these questions will proceed to construct theories of hypothetical inner mechanisms and will find observations "useful" insofar as they contribute to these theories.

Rachlin asks in what respect the environment is "impoverished." Referring to the sample facts, the environment is impoverished in that it is surely false that every person who knows these facts has been provided with specific data or training indicating that the facts are as they are – data blocking the natural inductive step that would interpret the ill-formed sentence, for example. Rather, this knowledge arises from properties of the mind/brain on the basis of much more meager evidence. Our problem is to explain how this comes to happen. One proposal is that the mind/brain is equipped innately with the principle of opacity; therefore, given enough information to determine that *each other* is a reciprocal expression, it applies the innate principle of opacity to determine the sample facts. Like all efforts at explanation, this one is hypothetical. Adopting this hypothesis, we will ask whether other systems of linguistic knowledge reflect the same principle, and whether other sample facts are accounted for on the same assumption. One can investigate the truth of the hypothesis in many other ways. The same is true in all of the cases that Rachlin believes to be unproblematic: walking, whistling melodies, building houses, and so on. We can certainly live our lives without understanding why these acts have the characteristics they do, without trying to understand the nature of behavior or its determinants. But here too, if we hope to understand these questions, we will proceed to construct explanatory theories that postulate inner mechanisms – mechanisms that distinguish an organism that behaves in such-and-such a way from another physical system that does not. Again, all of this seems so elementary that it is difficult to comprehend why it has to be said at all.

Rachlin asks whether we must assume that the laws of gravity are "encoded" somewhere inside a stone. The answer is that we do not, for reasons that are well-known. But we do have to attribute a property to the stone, namely mass. Amazingly, he thinks that since we do not have to assume the laws of gravity to be encoded in the stone, therefore we do not need to attribute specific structure to a child to explain the specific knowledge that the child attains (say, of the sample facts). He must then believe that a pigeon or a chimpanzee will attain this knowledge, indeed any organism will, given comparable experience (perhaps properly recoded for its sensory system), just as any organism will obey the laws of gravity. Note that even if Rachlin does believe this, which I doubt, it would still leave unanswered the question of how it happens, of the inner mechanisms responsible.

Rachlin then discusses what he calls various "difficulties." Since I fail to see in what sense they are "difficulties," I cannot respond, except to remark that his discussion of differences between capacity and knowledge in terms of a "temporal difference" is completely incoherent.

Rachlin asks where is the "filter" that passes only content words (assuming the correctness of a proposed account of "telegraphic speech"; see p. 6). He assumes that if it is "inside the child," then we are led to postulate "another child inside the first child," and he feels that we are led into this "terminological swamp" by "identifying behavior with organs of the body." Note first that it would be absurd to "identify behavior with organs of the body." As for the "terminological swamp," only sheer irrationality would lead one into it. The proposed explanation postulates a certain property of memory, and change in the system of memory. The question of the mechanisms of memory is certainly a fair one, but it is bizarre to suppose that the only approach to it is to postulate a child inside the child.

Rachlin suggests further that there is no more evidence for cognitive rules "wholly inside the speaker" than "for the existence wholly inside the bicycle rider of cognitive rules for bicycle riding." There are two points here: Is there evidence for cognitive rules "wholly inside the speaker?" Is there comparable evidence for the bicycle rider? I discussed the reasons for a positive answer to the first question. With regard to the second, I noted that in principle the answer might come out either way, but there seems to be no reason to suppose the answer to be positive. Rachlin claims that I did not "deal with" the question, but it is not clear what he means. If he means that I did not deal with the first question, he is obviously wrong, though he might argue that my discussion was insufficient or inaccurate. If he means that I did not deal with the second question, he is quite right, but it is no serious part of my concern in that discussion, for reasons that seem obvious enough.

I think that Rachlin's comments are instructive. To a "behaviorist" of this sort, it is simply inconceivable that one might try to understand what makes a human different from a bird or a chimpanzee, or why any of these organisms behave as they do. In his effort to avoid the study of inner mechanisms, he is led to the implicit claim that knowledge of, say, the sample facts (or innumerable others like them) would arise in any organism with an appropriate sensory apparatus simply through the operation of physical laws such as the law of gravity, no matter what its internal structure might be. For example, since vast amounts of English have been spoken into the telephone, it should follow that the telephone exchange knows the sample facts, and that we should be able to elicit behavior demonstrating this by experiment. Assuming that Rachlin rejects such absurdities, his refusal to undertake the study of inner mechanisms (and his further objection to anyone undertaking it) simply amounts to a principled refusal to try to understand the behavior of organisms. I think this is an appropriate epitaph for a certain style of "behaviorism."

Rollin. Rollin comes close to my own views concerning the important questions he raises when he suggests that the innate structures I postulate are both contingent (in that a different organism might have different structures) and "presuppositional" (or, we might say, "constitutive") for our experience. Changes in these innate structures would, as he says, lead to a change in our way of experiencing the world, and might make our "old world" incomprehensible to us.

What I have suggested about human language might, certainly, be false. But for present purposes, it is enough that it might be correct. Let us then tentatively assume that it is, and see what follows. Suppose that UG, which is an innate property of the language faculty, brings us to a state where we know that $f_1, f_2, \ldots$, given data D, where $f_1, f_2, \ldots$ is an infinite array of facts about form and meaning like the sample facts. Then this *knowledge-that* is not warranted by or grounded in experience; if UG were different, we would know that the facts were otherwise, given D. Hence, our knowledge that so-and-so need not be grounded or justified knowledge.

Suppose that a child with UG was placed in a nonhuman speech community characterized by UG′ $\neq$ UG and was presented with the same data D. Then he would again come to know that $f_1, f_2, \ldots$, but his knowledge would not correspond to that of others in the speech community. He would be wrong about their language, but right (by definition) about his own. This would lead to all sorts of personal difficulties for him, but no conceptual ones for us.

UG is not the only faculty of mind. Employing other faculties, Rollin and I might decide that, henceforth, we will use the sentence "the men expect each other will win" to mean that each expects the other will win. We would still know that the facts of our *human language* are otherwise, just as we cannot help knowing the meaning of "chair," even if we decide to use the term to refer to the square root of two (the example is a bit misleading, in that naming does encompass arbitrary acts, as distinct from determination of well-formedness or compositional semantics). Our decision now constructs for us something that departs from human language, as defined by UG, but that might be perfectly usable for communication and translatable to a human language, namely our human language, the language determined by the grammar we have come to know through the exercise of UG. Discussing such a case, Rollin suggests that I "might respond by asserting that any change in the rules would result in something that would not be language," a move he rejects as implausible. But I would not respond that way, since I do not know what the term "language" means, and I doubt that it can be given a sense that is useful in this context. Rather, I would respond by saying that the change results in something that is not a *human language*; that is, that it is not the image of suitable D under UG (which we are assuming to exist as a component of the human mind). (Further qualifications are necessary concerning markedness, but let us ignore these issues.) I don't think that this response is implausible. It amounts to the recognition that UG is only one of the components of the human mind. The difficulties that Rollin cites do not arise. Specifically, systems that are loosely called "language" may differ cross-culturally, because they may depart from "human language" through the intervention of other faculties of mind, or because the "languages" develop under conditions of conflict or diversity of data that depart from the idealization to a homogeneous speech community under which UG will map data to a "human language," in the technical sense of this discussion. Thus knowledge that $f_1, f_2, \ldots$, within a human language, is not "empirically changeable, or refutable," in the specific sense of Rollin's discussion (it is in other senses) – though such knowledge might not correspond to some system that we or others use for one or another reason.

Let us turn now to the question of knowledge of the behavior of objects. First, Rollin is quite right in saying that I gave no reason to suppose that what is true of language is true of other faculties, but I do not agree that it is "difficult to imagine any empirical research that could test the innateness hypothesis [better, some specific hypothesis about innateness, since there is no general "innateness hypothesis"] with regard to our knowledge of physical objects." Suppose, for example, that it is shown that a kitten with no relevant shaping experience refuses to step off a cliff (not a hypothetical example, in this case). Then it would be reasonable to say that the kitten knows that it will fall; it knows certain facts about the world. Suppose the same is true of children, as it may well be. Then it is fair to attribute comparable knowledge to the child, as an innate property, though perhaps one that functions only with triggering experience, at a certain stage of maturation, and so forth. Suppose we can show that a child lacking relevant grounding experience will extrapolate the motion of an object along a straight line or a parabola as it passes behind a screen. Then we can say, quite properly, that the child seems to know (and may in fact know) that the object will emerge at a certain point (if not stopped, etc.). Again, this would be knowledge without grounds: knowledge that results from the application of certain innate principles concerning the nature of objects in visual space. Suppose we can go further, and discover that observing several successive presentations of an object in motion, the child will determine that it has a certain shape, perhaps applying the principle of rigidity suggested by Ullman (1979): the principle that if successive presentations are uniquely interpretable as motion of a rigid object, then they are taken to be successive appearances of this rigid object. If indeed these presentations are stages in the motion of a rigid object of form X, then the child knows that the object in motion has the form X. Suppose further that various other properties of objects (say, those exhibited in perceptual constancies) are known without grounds on the basis of innate principles. Again, empirical research might support these conclusions, and, in a limited way, already does. Assuming results such as these, we could properly conclude that knowledge of the behavior of objects is (in these respects) innate, based on such principles as rigidity, and so forth.

Suppose now that some such story proves true, as it may. Let us then construct a case analogous to that of the child exposed to data D in a speech community that has UG′ rather than UG as a biological property. We are assuming that the child has a system of innate principles P, which lead to knowledge that f_1, f_2, . . ., given experience D; and that the child is placed in a world where things behave differently. The child's expectations would often be mistaken. Thus, the child expects that a physical object passing behind a screen on a parabolic course will emerge at such-and-such a point unless stopped, deflected, and so forth. It does not emerge at the expected point. Therefore the child concludes that it was stopped, deflected, and so forth, or perhaps that it was not a "physical object" in his sense. We might say that in this world the child still knows that physical objects behave in such-and-such a way, but that the things in this world are not physical objects (in the child's conceptual categorization), or that there is for some reason no unobstructed motion (in his sense); or we might say that the cognitive system constructed by the child's mind does not constitute knowledge in this world. But still the child has knowledge of the properties of real objects in the real world, if it satisfies what Rollin calls his "presuppositions for experience." If he is in a different world, he is in trouble, but we are not in conceptual trouble. We can still say that in our real world the child does in fact have knowledge that f_1, f_2, . . . with regard to the behavior of objects, though this knowledge is not grounded in or warranted by experience, but rather results from the application to data of certain innate properties of mind.

Actual examples are not difficult to find. Take **Cromer's** example of a "conserving" child who argues that some of the plasticine was lost or stolen, whatever the presented facts, in the Smedslund experiment that he cites. The principle of conservation involved in these judgments might be innate, though perhaps operational only at a certain stage of maturation. The beliefs that the child is using in drawing these conclusions do constitute knowledge, so far as I can see, whatever their origin. The real world largely conforms to the constructed word of experience in this case, though it does not when mass is converted to energy.

I haven't spoken of the case of mathematics and logic, which raises other questions about the status of the truths of these theories. But for the crucial case of knowledge of the behavior of objects, I think we may discover something like the following: There are certain innate principles P which, under appropriate experience and maturation, lead to a system of rules and representations that provide expectations about the behavior of objects. They lead to the belief that f_1, f_2, . . . , over an infinite range. These beliefs are not grounded in experience or warranted by experience. If they are true of the actual world, then they constitute knowledge about the actual world. Thus, it is correct to say that the child knows that the object will emerge from behind the screen at a certain point (unless deflected, etc.), if it happens to be following a certain trajectory (say, a parabola); that successive presentations are of a rigid object of structure X, that the plasticine was lost or stolen, that he will fall if he steps over a cliff, and so on. If we deny the term "knowledge" in such cases as these, then little will be left of "knowledge about the world." But such knowledge need have no grounds in any sense of this term that will carry the epistemological burden required in empiricist theories.

I have avoided the terms "a priori," "synthetic," and so on. I am not sure that they are appropriate for an account of the various kinds of human knowledge. I agree with Rollin that what I have been saying about humans is, if on the right track, applicable as well to animals, at least *prima facie*. My feeling is that the study of human (and animal) knowledge should be recast in such terms as these. Warrant and justification are not necessary conditions for much of what we call "knowledge" – specifically, factual knowledge – and if the concept is narrowed to exclude these cases, then central areas of what has been called "knowledge" will be excluded. In some respects, traditional analysis of knowledge in terms of warranted true belief may well be appropriate (apart from Gettier problems and the like); namely, instances of knowledge that do not derive from the structure of our fundamental cognitive capacities as they grow; for example, knowledge of scientific fact, which must be acquired through careful experiment and theory construction (in which, I assume, innate factors must also enter crucially, for reasons I will not discuss here). In these cases, we must have adequate grounds for our knowledge claims, or they are worthless. Knowledge comes in many varieties, and for crucial elements of our knowledge, the traditional empiricist paradigm seems to me quite inadequate. How extensive these elements are remains to be discovered. Language seems to be one case, and if the remarks just briefly outlined prove to be somewhere near accurate, then the same is true of what are regarded as more "typical" cases of knowledge.

I recognize that these remarks are sketchy and imprecise, but I see no fundamental reason why they cannot be expanded and made more exact. Furthermore, it seems to me that the little that is known about our beliefs, expectations, and so on, with regard to physical objects suggests that it might prove quite proper to analyze "knowledge" in the terms suggested here, in terms of internalized mental structures (of rules and representations, in some cases), for substantial areas of cognition.

Rosenthal. Rosenthal's thought-provoking comments begin by distinguishing two different applications of the notion "modularity": to the state attained, and to the initial state of the mind. Let's call these "attained" and "initial" modularity, respectively. Suppose we reject initial modularity and accept attained modularity. Then we are assuming that "such differentiation as there may be [in the state attained] will reflect differentiation in the environment" (p. 3 of the target article); that is, that attained modularity reflects environmental modularity. I find this implausible in the cases mentioned, because of the argument from poverty of the stimulus. Thus, if it is true that the environment does not yield the postulated principles of UG (say, locality and opacity) or the rigidity principle and others involved in our knowledge about objects (see response to **Rollin**), then attained modularity reflects initial modularity in these respects. While these remarks do not establish my conclusion that attained modularity in these respects reflects initial modularity, they do at least indicate how one could proceed to verify this conclusion. It is this line of reasoning that underlies the "striking pattern of inference" to which Rosenthal calls attention, namely, from attained to initial modularity.

Rosenthal suggests that while this pattern of argument may be plausible in the case of language, it is "far less credible" in the case of knowledge of music, mathematics, the behavior of objects, social structure, human characteristics, and so on. It seems to me that the plausibility of the argument closely reflects what we know (or plausibly conjecture) about these topics: the more we know, the more plausible the inference is. Take behavior of objects. The work of Marr and his colleagues, cited above, makes very specific proposals about levels of visual processing in terms of a "primal sketch" involving intensity changes, local representation of shape and orientation of surfaces, axes of elongation and symmetry ("stick figures"), determination of structure from motion by virtue of the rigidity hypothesis, and so on. It does not seem likely that these principles of visual processing (assuming they are valid) operate in the analysis of sound input in terms of syllables, phonological features, and so on (though one may find some common properties). If so, we have another case of attained modularity, which it would be only reasonable to ascribe to initial modularity. There is, of course, a traditional view that "higher-level" processes are uniform even if sensory and perceptual systems are modular; perhaps so, but it seems to me a dubious argument from ignorance. As for music, it is not obvious that properties of phrasing, tonality, theme-and-variation, and so on, can be derived from "well-understood aspects of the mechanism underlying the production of sound," and there is work suggesting that common features of musical systems may indeed derive from something innate in the organism (cf. Hindemith 1961; Bernstein 1976; Jackendoff 1977; Jackendoff and Lerdahl, forthcoming).

With regard to the number faculty, we agree that the concept of adding one, indefinitely, distinguishes our number system from abilities of other known species, but Rosenthal adds that understanding this does not "by itself, help us to understand our capacity to deal with [the number] system." I think it does help but does not exhaust the matter. It does not seem to me a "surprising claim" that our mature capacity to deal with the number system is, in its essentials, innate. On the contrary, it is difficult to imagine by what inductive, associative, or other "learning process' this capacity might have derived from experience (though, as I noted, it may be triggered by experience). I've heard reports that aborigines lacking any relevant experience master the number system very easily when they enter a market economy, which would suggest, if true, that the capacities are latent, ready to be put to use. The lack of diversity, which Rosenthal notes, does not seem to me to be a crucial factor; identification of common properties of a variety of diverse systems is not a necessary condition for attribution of innate structure.

I do not quite follow Rosenthal's discussion of the analogy to chemical properties and underlying physical mechanisms. It is true that "the mechanisms that produce effects at higher levels of organization are largely unpredictable on the basis simply of a knowledge of those effects." My point, however, was different: that knowledge of these higher level effects sets empirical conditions to be met by the study of the underlying mechanisms. This seems to me true in both of the cases I mentioned: chemical properties – physical mechanisms, linguistic properties – neural mechanisms. I do not see the basis for Rosenthal's skepticism about comparable investigation of musical and mathematical abilities, nor do I follow the connection he suggests to the issue of psychological reality.

As for the two missile systems, Rosenthal agrees that it is an empirical problem to determine whether the missile is or is not a "cognizing" one in my sense; thus in one case we know it is because "it was so constructed," implying that it is a matter of fact. He then points out, correctly, that I suggest no method for determining whether or not a system is of this type – that is, for settling this matter of fact. But there are no general methods for determining facts of this sort, in the case of missiles, cognitive capacity, atomic theory, or whatever. I would like to recall again the rule of thumb I mentioned in the text: that it is improper to impose demands on the "soft sciences" that cannot be met, even for advanced sciences.

Rosenthal observes that conclusions about initial and attained modularity do not strictly "imply" that much of what we call "learning" is essentially a matter of growth and development. That is correct, but they do seem to me to provide evidence for the latter thesis, if "learning" is understood as the application to arbitrary content domains of fixed principles of induction, association, and so forth. I do, however, agree with Rosenthal's conclusion: "the idea of learning as growth is a further step in the analogy of our cognitive capacities with physiological system." I think it is a step that should be explored, to begin with, by sharpening the notions "learning" and "growth," which could do with much closer analysis.

Is it indeed unintelligible to propose that "what propositional knowledge we can have is, to some degree, a function of our biological endowment," or to imagine that organisms might "diverge from us in respect of what propositional content could enter their mental lives"? Such a thesis might be false, but it does not seem to me "incomprehensible." We can characterize two distinct "cognitive beings," in Rosenthal's sense, which differ in "the range of propositions they could comprehend." Suppose one to incorporate a system of mereology and the other a system of set theory, for example. The notion seems intelligible; the question is whether some such thesis holds of humans. One can imagine several versions of such a thesis. Suppose that our conceptual capacities, including principles of individuation, construction of more complex concepts, abduction, and so on, are the fixed principles P, by virtue of biological endowment. Then we have a class of "humanly intelligible theories" that are attainable in principle by the principles of P. We can then consider a strong or a weak thesis of "unattainability of theories" for an organism characterized by P. Some theory T might be literally not in the range of P, regarded as a function. Or, some theory might be humanly intelligible in this sense, but at so far a remove that within realistic constraints of time, attention, availability of data, and so on (say, within bounds set by the potential existence of the species), it cannot be attained by the principles of P. In either case, some other organism can be imagined for which the inaccessible theories might be readily intelligible.

In fact, this seems to me not only comprehensible, but even, conceivably, subject to investigation (see Chomsky 1980, chapter 6; 1975, chapter 4). It might turn out, for example, that one could give precise content in this manner to Descartes' thesis that we may not "have intelligence enough" to comprehend how free action can be indeterminate, though it would be "absurd to doubt" that it is so (see Chomsky 1980, chapter 1). Such an analysis would depart from Descartes' framework in a number of respects – among them, in recognizing a notion of "human mind" as distinct from "mind."

In short, I do not think that these speculations are ruled out on grounds of unintelligibility, nor that general considerations of the sort suggested in Rosenthal's comments give compelling reason "to seek an alternative to any theory of the acquisition of knowledge that has this consequence." Naturally, one should seek alternatives. My own speculation is that the search for alternatives will be productive in clarifying their inadequacy, but that, of course, remains speculation.

Sampson. Sampson asserts that he has demonstrated that a Popperian thesis combined with an argument of Simon's "predicts" the postulated linguistic universals. Let us distinguish two strands of the argument: evolutionary and ontogenetic. Only the latter is relevant here. The question is whether by attributing the Popperian thesis and Simon's assumptions to the child, as part of its initial state, we can predict that the child will construct only structure-dependent rules, the principles that account for the sample facts, and so on. To be concrete, the question is whether these assumptions will account for the fact that, given such examples as "the men like each other," meaning that each likes the other, the child will unerringly construct a hypothesis that predicts that "the men expect each other to win" will mean that "each expects the other to win," whereas "the men expect each other will win" is not well-formed, with the meaning that "each expects the other will win"; similarly, with innumerable other examples. Nothing in the published literature comes anywhere near substantiating such a claim, which seems quite a remarkable one; I cannot, of course, comment on the unpublished book in which Sampson claims to have provided the demonstration he reports. Note that arguments based on the likelihood that hierarchic structure will develop in evolution, such as the argument of Simon's that Sampson cites, do not seem to have any bearing on the ontogenetic argument.

As for the rapidity of growth of vocabulary items with highly specific properties, Sampson is right in saying that I offered no precise argument, but wrong in saying that the argument is "wholly empty." His task is to show that, on his "Popperian assumptions," the specific properties of vocabulary items, and the uniformity with which they are acquired, can be predicted, given the data available to the child. My point was qualitative, not precise; namely, that observation of the specificity, intricacy, uniformity of acquisition, rate of acquisition, and poverty of the data base suggests the approach he favors is unlikely of success. Rather, an approach such as that proposed by Fodor (1975, and subsequent work) seems to me more plausible, given these qualitative observations. It is clear what Sampson must do to substantiate his thesis. One must certainly remain open-minded; personally, I am not exactly holding my breath.

Schank. Schank purports to speak for "those of us who work in artificial intelligence." His claim, fortunately, is too broad. **Marshall**, in his commentary, is more accurate when he says, probably with Schank in mind, that "much [not all] work in 'artificial intelligence' thus conflates properties of the language faculty (e.g., the structural configurations that determine sentence-internal anaphora) with, for example, one's knowledge of the appropriate way to behave in restaurants. The *a priori* likelihood that the intersection of these sets constitutes a theoretically coherent domain is not great." I think that Marshall is right, and that the work of Schank's group substantiates his judgment. I will not attempt to evaluate that work here; for an analysis concluding – persuasively, I believe – that this work has been virtually without issue, see Dresher and Hornstein 1976; 1977. However, I do want to stress that work in artificial intelligence (AI) is not limited to Schank's approach. For example, Marr and Nishihara (1978), whose work falls strictly within AI, are harshly critical of the tendency to center research on problems "in which human skills seem to rest on a huge base of knowledge and expertise" rather than focusing on components of such problems. Furthermore, they argue that it is a serious error to construct a "mechanism to mimic some small aspect of human performance, for example by writing a language-understanding program . . . applicable only in a highly specialized domain," as in the work to which Schank refers. They argue that "such studies are misguided" because they fail to reveal the basic structure of the problem, and lacking a theory of the computation and its components, they consider only "the mechanisms through which their solutions are implemented." I think that these criticisms are sound and apply directly to the work that Schank recommends – for example, the studies that "simply assert that their programs are their theories and leave it at that." The question goes beyond the scope of our discussion here, but it should be understood that Schank in no sense speaks for "artificial intelligence," as he claims; in my view, its contributions lie elsewhere.

The issue Schank raises can be clarified by a closer look at the example he cites. One of my "sample facts" was that (1) but not (2) is a well-formed question: (1) Which class did the teacher think that his assistant had told to study the lesson? (2) Which class was the lesson harder than the teacher had told that it would be? Schank misunderstood my account of these facts, but that is unimportant. Consider, rather, his own reaction to them. He does not propose an explanation for such facts. Rather, he denies that an explanation should be sought, since "we do not form questions apart from our desire to know something." Such facts as these are not worth considering (or perhaps are not facts) because people are not "silly enough" to produce such sentences as (1) and (2). It is therefore of no interest to him that such sample facts about our knowledge provide evidence bearing on the structure of comparatives, on general principles of locality, and so on, in English and other languages. His attitude is like that of someone who objects to physics on the grounds that in normal life one doesn't find balls rolling down smooth inclined planes, let alone more exotic facts.

Schank might have argued, more reasonably, that an explanation for the sample facts will not contribute to his specific goals. Perhaps, though I doubt it, for reasons implicit in **Marshall**'s commentary, cited above. But he goes much further. He states that a theory can only be "relevant" if it is related to "computer simulations of the communicative process," that there do not exist mechanisms that account for the sample facts, and that therefore no such mechanisms have been developed as part of the state attained. This seems a rather perverse attitude. Facts do not disappear because we can think of no way of explaining them, nor do theories become "irrelevant" if we can think of no way of using them for some project in which we happen to be engaged.

Schank's statement that "grammars exist in the minds of linguists only" might be rephrased in a more rational form: namely, he might have argued that if we had process models in his sense, they would explain such facts as my sample facts without recourse to the idea that grammars are mentally represented. As I have frequently pointed out, this might prove correct. But Shank does not attempt to argue in this way. Rather, he denies the existence of the facts, and thus denies the existence of principles that explain them or mechanisms that satisfy these principles. This is simply a form of obscurantism.

My paper began with the statement that "I will not attempt a systematic presentation of . . . a model here, "but will rather keep to some conceptual problems. Schank reads this as a refusal to "construct specific models" at all, though he knows perfectly well that in much work not reviewed here quite specific models have been presented and studied (though not models for ordering hamburgers). He states that I "dismiss [my] lack of evidence," though he knows that in these technical studies a great deal of evidence is presented and analyzed. This is, again, a curious kind of comment.

Schank objects to my statement that knowledge of language is not a task-oriented device, on grounds that "people know their language so that they can communicate." This is an obvious equivocation. I distinguished language from the "cognizing missile" in that the latter, but not the former, is designed to carry out a specific task (hitting the moon). In this sense, language is not task-oriented, whatever the relation between language and communication may be. Schank's statement, just quoted, is incoherent as it stands, though one can imagine what he has in mind. Attempts to provide some sense to the notion that communication is somehow the essential property of language have not been very successful in my view (see Chomsky 1975, chapter 2), but the question is plainly irrelevant to the point I was making.

Schank asserts categorically that "it is impossible to produce a model of language alone." Language does not exist, he asserts, "apart from beliefs, goals, points of view and world knowledge." This denial of a proposal concerning modularity might prove correct. But mere assertion does not constitute an argument, and one will search in vain, here or elsewhere, for something that goes beyond mere assertion. In fact, Schank's refusal to concede even the existence of obvious facts and his rejection of theoretical work that does not seem to contribute to his specific projects as "irrelevant" makes clear where such dogmatism leads.

Schank claims that I "strongly disavow" computational and psychological experimentation. How one could derive that conclusion from my paper, or anything else I have written, I have no idea. It is true that I believe that Schank's approach has achieved nothing, and will not, because of its strange built-in limitations, but that does not amount to a "strong disavowal" of computational and psychological experimentation.

Schank argues finally that the cognitive sciences "will be in for difficult times" if they study rules and representations "without regard for how people actually think and speak, and without regard for the natural processes that comprise their ability to do so." Study of the transition from the initial to the attained state of knowledge is study of a "natural process," and study of these states and their properties should contribute to sensible investigation of how people actually think and speak. One would also hope that studies of how people think and speak would yield results bearing on properties of the initial and attained state, though this seems to me most unlikely along the lines that Schank proposes, with his narrow focus on programs applicable only in some highly restricted domain, his failure to consider "the theory of the computation" or the components of complex problems, and his lack of concern for many other questions about the nature of the systems that interact to produce the phenomena that we observe in real-life situations.

Searle. Searle begins by stating that his characterization of my work was not an objection but rather part of an objection – one that failed, I argued. It is difficult to see the significance of the distinction. His comments in part recapitulate the argument to which I have already responded, but with some modifications. Let us consider now his new formulation.

Let us first be clear about some things that are not at issue. Searle is not objecting to my usage of the term "rule" to refer to elements of the kinds I postulate as components of grammars or UG: "Move A," opacity, and so forth. Thus, he argues that we would have evidence that the elements I postulate are rules that the person follows if the person could become aware of the rules and agree that he follows them. Second, he is not insisting that rules must be accessible to introspection (though, as we shall see, there remains some equivocation here). Rather, if they are not, there must be some other evidence that the postulated rules are rules we follow – rules that play a "causal role" in behavior, in his usage. And finally, he does not deny that there might be innate rules of the type I have discussed, but he insists that there must be evidence supporting any such hypothesis. With all of this, I agree.

Where then do we disagree? Let us begin by comparing three kinds of fact: (1) when a person is dropped, he falls; (2) the person understands the sample facts in the manner indicated; (3) the person has acquired the knowledge that the sample facts are as they are on the basis of a certain exposure to data. How do we proceed to explain these facts?

In each case we proceed by attributing to the person a certain structure. In case (1), mass; in case (2), an attained state AS; in case (3), an initial state IS. What is the nature of AS and IS? We can attempt to characterize these structures at various levels: e.g., in terms of neural elements, or in terms of general properties of such elements, I have made certain proposals, at the second level, in terms of rules, representations, and principles of computation: AS contains the rules of a grammar G, and computations using these rules yield the facts; IS contains the principles of a universal grammar UG, and AS is determined by fixing the parameters of UG through experience (inter alia). Returning to the sample facts, if G contains the relevant rules of English and the mind/brain uses (follows) these rules in computing representations, then the person will know that the sample facts about reciprocals, and so on, are as they are. And if UG contains the principle of opacity, the person will acquire this knowledge on the basis of evidence sufficient to establish that *each other* is a reciprocal expression, rather than the name of a tree.

Searle argues that this is not enough: "there must be some independent reason for supposing that the rules are functioning causally" for the person, or else the rules are "mere hypotheses that correctly describe his behavior." Searle states that I have provided no "independent" evidence that the rules are more than "mere hypotheses" describing behavior.

There are a number of problems in Searle's discussion. First, the rules are not hypotheses describing behavior. Rather, our hypothesis (mere or not) is that AS contains a certain system of rules and that IS contains another system, and that these rules function in the manner described in the course of behavior, with what Searle calls a "causal" role. It is quite important to be clear about this distinction. It is standard practice to use the term "grammar" ambiguously, referring to the linguist's theory or the system of rules attributed to the mind in this theory (similarly, "universal grammar"). But one must be careful not to be misled by this practice.

Keeping the distinction clearly in mind, let us return to Searle's criticism. When I say that AS contains the rule "Move A" and the principle of opacity, among others, and that these rules are used to form representations that enter into our judgments and other behavior in the ways described, I am formulating a hypothesis about AS, about its modes of computation and representation. But the rule "Move A" and the principle of opacity (etc.) are not hypotheses describing behavior. What Searle should have said, then, is that I have provided no evidence for my hypothesis that AS has the structure I postulate: namely, that AS contains the rules I propose and that these rules are followed in the manner postulated in behavior (e.g., judgment). Thus my hypothesis remains "mere hypothesis." But when his objection is correctly formulated, eliminating the confusion just noted, we see at once that it is groundless. Though of course the hypothesis remains "mere hypothesis" (i.e., empirical hypothesis), I have certainly offered evidence to support it, however one may evaluate the strength of this evidence (an issue that Searle does not raise in this connection). The evidence bearing on the hypothesis attributing rules of grammar to the mind is that sample facts are explained on the assumption that the postulated rules are part of the structure of AS and are used in computations eventuating in such behavior as judgments about form and meaning. The evidence with regard to UG is that properties of states attained are explained on the assumption that the principles are as postulated in IS: the behavior of reciprocals, of indirect questions, of disjoint reference of pronouns, of purposive infinitivals, and so on. One might plausibly argue that the evidence is insufficient or not compelling, or that it is not opacity but some other principle that is involved, and so on, but it is certainly false to say that no evidence has been offered for the hypothesis that AS and IS are as postulated. Searle's notion of "independent" evidence has not been given sense. The evidence that has been put forth is certainly relevant to the truth of the hypothesis that AS and IS are as postulated, and that their rules are used in the course of behavior, in the manner proposed.

A second problem in Searle's account is that there is little sense to the statement that the rules of language, whether the ones I postulate or the ones that Searle postulates, "*cause* the very behavior that they describe and explain" (my emphasis), even if they enter into behavior. Rules that we follow do not "cause" our behavior. But let us put this question aside, accepting Searle's odd usage of the term "cause," and keep to the first problem.

To Searle, none of the evidence that I have provided is evidence that the person is following the postulated rules. Something else is required in principle, some "independent" evidence over and above the explanatory power of the hypotheses. What else? Suppose that the rules could miraculously be made accessible to consciousness, as in the case of the rules that Searle has in mind, which are part of what Moravcsik calls a "shallow theory" of the mind (and which, of course,do not bear on the kinds of empirical problems that I have been discussing, as I take it Searle would agree). Then Searle would agree that we have evidence that the rules are being followed; in his paper, which I discussed in my target article, he went further, but now he states, more reasonably, that such introspective judgment would simply provide inconclusive evidence for the hypothesis that the postulated rules are part of the state attained. But why is this particular kind of evidence of such significance? Why would the speaker's obviously fallible and uncertain intuitions and judgments about the rules he allegedly follows provide evidence that the rules are being followed, whereas the kinds of evidence I discuss (namely, explanation of the sample facts on the basis of the hypothesis that the rules are being followed) in principle is not evidence at all? To this question Searle still offers no response, perhaps because of the confusion with regard to the status of the rules: not hypotheses describing behavior, but rather attributed to AS as part of its structure in hypotheses that purport to explain behavior on the assumption that the rules attributed to AS are followed in behavior ("cause" behavior). Once this confusion is overcome, it seems clear that evidence has been provided (whatever its weight) for the hypothesis that the rules constitute part of the structure of AS and are followed in behavior; and further, evidence has been provided for the hypothesis that the principles of UG constitute part of the structure of IS and play a "causal role" in determining AS (note that in the latter case, the notion "causal role" is appropriate, since questions of will and choice do not appear to enter – AS is determined, it is postulated, by IS, given experience). Searle has provided no sound argument against these hypotheses and no support for his claim that no evidence has been provided for them.

Note that if Searle's fallacious argument had any force, it would apply as well to other theories of mental computation. Consider again the theory of visual processing proposed by Marr and his colleagues (see my response to **Rosenthal**). This theory postulates certain computational rules (e.g., those that provide analysis into "stick figures") that are used in identifying objects, and so on. The rules are not accessible to consciousness. Empirical evidence has been provided that the structure of the visual system is as postulated at the appropriate level of description; that is, that these rules enter into computations of certain representations in the course of identification of objects, and so forth. By Searle's standards, no "independent" evidence has been provided for the internal structure postulated, with its rules, representations, and computations. While there are differences between the two cases, in the present context the analytic principles proposed in this theory of vision have the same status as the rules of grammar attributed to AS in the hypotheses that Searle has misconstrued as rules.

Searle proceeds to argue that "the heart does not follow rules, nor does the liver; rather, there are certain general principles that describe their operations." This is a fact of type (1), above. In investigating the heart and the liver, we try to determine the structural properties of these organs that account for the fact that they conform to the general principles that describe their operation, attributing these properties to the organ in question. Again, we can do this at various levels: in terms of specific physical mechanisms, or more abstractly in terms of properties of these mechanisms. At the second level we can ask a question analogous to the one raised about the two missile systems I discussed, and we will presumably conclude that it would be wrong, in this case, to attribute to the heart and liver a computational system of rules and representations of the sort attributed to the cognizing missile, the visual system, or the language faculty. All of this seems relatively straightforward in principle, once we are careful to distinguish hypotheses about the system from elements attributed to the system: in some cases, rules that are followed.

Searle then proposes what he calls "an alternative speculation to the ones that Chomsky makes." To keep things clear, note that this is an "alternative speculation" to my hypotheses concerning UG, not concerning grammar. Let us suppose, with Searle, that "the structure of possible human language is constrained by the structure of the human brain," by virtue of "quite specific neurophysiological structures in the brain." So far there is no disagreement. I also have a proposal as to the properties of these neurophysiological structures: they have the properties of UG. What is proposed, at the level of abstract characterization, is that the specific neurophysiological mechanisms that we both assume to exist realize the principles of UG, and that a grammar results (in part) by fixing the parameters of UG, this grammar then playing what Searle calls a "causal" role in behavior. But Searle asserts that any apparent success of this proposal concerning the structure of the human brain is "an illusion created by the fact that actual physical structures in the brain were doing their work." By parity of argument, it is an "illusion" to suppose that Marr's principles correctly characterize visual processing at the appropriate level of abstraction, because they hold only by virtue of the fact that the brain is doing its work. And more generally, whenever we succeed in developing a theory that attributes to the mind/brain certain properties, the success of this theory is an "illusion" because it does what it aims to do: account for phenomena on the basis of hypotheses attributing a certain kind of structure to the brain, which enters into behavior in a certain way. Note that, in the specific case in question, the structure that my hypothesis attributes to IS is (in part) a system of rules and principles, which becomes a grammar when parameters are fixed, and so on. This is a particular proposal about IS – about the function that maps experience into state attained. The rules and principles attributed to UG thus play a "causal role," according to this theory, in two respects. First, they play a part in causing the transition from IS to AS, and then, constituting a part of AS, they "cause" behavior (in Searle's sense).

In short, the rules of UG (assuming this theory to be correct, and Searle offers no argument that it is not) are not "artifacts produced by the fact that a common neurophysiological structure constrains actual rules" of grammar. Rather, the rules and principles of UG form part of a characterization of this common neurophysiological structure at the mental level of characterization, and they also form part of the characterization of the state attained, at the same level. Note that Searle does not object to describing systems such as the brain at these two levels: in terms of mechanisms, in terms of general properties of such mechanisms (rule systems, in some cases). Turning now to state attained, rather than initial state, he agrees that in his "alternative" there would be "a mental level where we describe how actual rules and representations function causally" and a neurophysiological level in which these rules and representations are "realized." Adopting still his usage of the term "causally," we may say that the rules and principles of UG (in IS) and of the grammar (in AS) function causally, in the manner already indicated. Where then is the disagreement? We can disentangle it from Searle's statement that "Chomsky's claim is that there is another level of rules *beyond all possible introspection* but not neurophysiological either" (my emphasis); that is, a level of rules that is neither mental nor physiological in the sense just described. True, the rules of grammar and of UG that I postulate are not "neurophysiological," but rather neurophysiologically realized, in the sense that both he and I agree to be legitimate. Why then are these rules not at Searle's "mental level"? Precisely because they are "beyond all possible introspection"; that is the only relevant property that they lack. Once again, as in his original article, Searle has been trapped by his completely unwarranted assumption that rules can be attributed at the "mental level of characterization" only if they are open to introspection. While Searle now explicitly denies that he is making this assumption, as soon as we actually eliminate it, as we must, his error appears clearly. My hypothesis, to which he refers, is that the rules in question are at his "mental level," even though they are "beyond all possible introspection," and the evidence for this hypothesis is the kind of evidence I have reviewed, which counts as evidence that the rules have "causal efficacy" (in his sense).

We are left with certain theories of grammar and UG that are offered as an account of AS and IS. These theories offer hypotheses as to the rules, representations, and computations that enter into behavior. The hypotheses concerning AS and IS may be right or wrong, but Searle has offered no argument whatsoever bearing on this question, nor any alternative account to be considered.

Sober. The points Sober raises can be clarified, I think, if we are careful once again to distinguish between (i) the linguist's hypothesis that such-and-such a rule is part of the mentally-represented grammar, and (ii) the rule so attributed. Thus, let R be a rule and H(R) the hypothesis that this rule has what Sober calls "some appropriate causal role"; H(R) attributes R to the mind/brain and asserts that computations eventuating in behavior use R.

Sober begins by stating that "a psychological hypothesis will be entitled to [the label 'psychological reality'] if it is the best explanation available," in my view. Thus H(R) is entitled to this label if it is part of the best theory dealing with the speaker's judgments, and so forth. But he then shifts his ground and says that "rules have psychological reality," in accordance with "a fairly standard construal of the idea" of psychological reality, only if they "play some appropriate causal role." But this "standard construal" refers not to the psychological reality of H(R), but of R, a different matter. Let us distinguish the two notions PR(theory) and PR(entity); respectively, psychological reality of a theory and its hypotheses, such as H(R), and psychological reality of entities such as R attributed by the theory to the mind/brain. Sober's comparison of my view with the "standard construal" thus involves a crucial equivocation between PR(theory) and PR(entity).

Turning now to the physical analogy that Sober discusses, note that it is misleading in one respect: hypotheses such as H(R) are not analogous to laws of nature, but rather to hypotheses about the structure of particular systems such as the sun or the human visual system. Let us therefore recast Sober's argument in these more appropriate terms, which does not affect its thrust. Suppose that the theory T(sun) states that the sun contains Helium and the theory T(vision) states that the human visual system contains edge-detectors. Then T(sun) and T(vision) are analogous to H(R), and Helium and edge-detectors are analogous to R.

Sober begins by comparing attribution of physical reality to theories such as T(sun) and T(vision) with attribution of psychological reality to rules (note again that I have reformulated his argument, replacing "laws of nature" by theories of specific systems, as is appropriate in this context). But this is an equivocation, for the reasons just noted. Let us then reformulate the argument to eliminate the equivocation. Let us accept Sober's statement (now properly reformulated) that if T(sun) and T(vision) have physical reality, and some theories T′(sun) and T′(vision) are logically equivalent to them, then T′(sun) and T′(vision) also have physical reality. Analogously, if H(R) has PR(theory) and some hypothesis H′(R) is logically equivalent to it, then H′(R) has PR(theory). So far there is no problem.

Turning next to PR(entity), Sober argues that if R has PR(entity), and *Y* is "logically guaranteed to be input-output equivalent" with R, then it does not follow that *Y* has PR(entity). The analogue would be a situation in which Helium has the physical analogue of PR(entity) with respect to the sun, and some entity *Y* is "logically guaranteed" to have the same empirical effects as Helium in the sun (similarly, edge-detectors in the visual system). But, Sober's reformulated argument asserts, it does follow that *Y* has physical reality, so physical and psychological reality are different. But now that the equivocation is removed, it is difficult to make any sense of the argument. To say that two entities are "logically guaranteed" to have the same empirical effects ("to be "input-output equivalent") is simply to say that the theories postulating them are logically equivalent and hence have the same "reality" at the level of PR(theory). But the theory of English has the same status as the theory of the sun or of vision in this regard, and the same is true of the elements they postulate. There is no distinction of intensionality. Thus Sober's objection has no force.

There are some subtleties that should be kept in mind. Thus, consider two theories of representation of meaning in human language, one that holds that grammars generate representations using one notation (say, familiar quantifier-variable notation) and another that holds that grammars generate representations using a different notation (say, variable-free notation). These notational systems might be equivalent in the sense that anything expressible in one is expressible in the other: inferences match, and so on. But nevertheless the two theories may be empirically different. In fact, this seems to be true in the case in question; cf. Chomsky (1975; 1977; 1980).

Similar questions arise when we consider more carefully Sober's reference to rules that are "logically guaranteed to be input-output equivalent" but yet do not, in his view, necessarily have the same status at the level of PR(entity). Above, I assumed that he meant that the theories postulating these rules were logically equivalent, but one might give a different interpretation to his contention. Suppose that linguistic theory LT assigns a grammar G to a certain language, while linguistic theory LT′ assigns grammar G′ to this language. Suppose that G and G′ are strongly equivalent in the sense that they not only generate the same set of strings but also assign to them the same structures (a notion that can be made precise).

Suppose further that G and G′ differ by only one rule, R in G and R′ in G′. Suppose further that from inspection of LT and LT′ we can deduce the strong equivalence of G and G′. Perhaps this is what is meant by saying that R and R′ are "logically guaranteed to be input-output equivalent." If so, does it then follow that there is no empirical difference between G and G′, R and R′? No, it does not. For example, investigation of some other language might reveal that there is empirical evidence for a certain general property in linguistic theory that permits R but not R′. Or, R and R′ might yield derivations that differ in some property (say, length), and psycholinguistic experiment might support one but not the other consequence. In principle, there are many possible kinds of evidence that might bear on the choice between, respectively, R-R′, G-G′, and LT-LT′. Thus, while Sober's conclusion (now reformulated) that PR(entity) for R does not imply PR(entity) for R′ is correct, this has nothing to do with intensionality; and furthermore, the same considerations apply to the analogue of PR(entity) with regard to Helium, edge-detectors, and so on.

Sober's second objection is that, among the hypotheses that are psychological in their subject matter and true, only some are "psychologically real" in the sense that they postulate mentally-represented rules that are followed in computations eventuating in behavior. Other such hypotheses may not attribute mechanisms, or may be true by virtue of properties of the vocal organs, for example. The distinctions are worth noting, and I have no objection to the use of the term "psychologically real" in the specific sense that Sober suggests, though my own inclination, as noted in the target article, is to avoid the term altogether as seriously misleading, just as the term "physically real" is rarely used in the natural sciences. See, in this connection, **Harman**'s comments and my response.

Consider, finally, the alternatives that Sober suggests to my "psychological interpretation" of linguistic hypotheses. Sober suggests that these hypotheses might be taken to describe "social regularities" or "abstract mathematical structures." But then we surely want to know why there are these social regularities and not others, or why we consider these abstract mathematical structures and not others. Surely the facts might be otherwise. The principle of opacity, for example, need not be true of English or other languages. For some aspects of language – say, the irregular past tense of *go* – it may be correct to attribute our shared knowledge to something like "social regularity," but, for the reasons I discussed, this seems highly unlikely in the case of the "sample facts" and such principles as opacity. I can see no reasonable alternative to the assumption that the opacity hypothesis and many others like it are true (if they are) because the mechanisms are as postulated rather than of some other sort. Even in the case of "shallow rules" in **Moravcsik**'s sense, such as the rule giving the past tense of *go*, we are led to postulate mechanisms, though the matter is relatively uninteresting in this case. When we turn to more significant properties of language, the same move is appropriate, but with far more interesting results, if we hope to understand how "social regularities" are observed or how the relevant cognitive systems arise, or if we are sufficiently curious as to ask why certain abstract mathematical structures are relevant to the study of language while others, no less plausible on a priori grounds, are not.

Stich. Stich raises two important questions: (1) about knowledge, and (2) about universals. Let me take the second first, since it seems more readily addressed.

First, I do accept the answer that Stich felt to be an "unlikely interpretation" of my views: namely, that all evidence (from linguistic intuitions, psychological experiment, brain damage, cognitive simulation, etc.) is potentially relevant to determining what the internally represented grammar is. Now to the difficulty that, Stich suggests, "quickly emerges elsewhere."

Stich alleges that I argue that if we have evidence of the sort he outlines for some principle (say, locality), then I go on to infer that "the principle that is radically underdetermined by the evidence available to the learner must be a *universal* feature of all languages." He comments that this inference does not follow. I agree, and would not argue for necessity, only plausibility. In fact, "conditional universals" have often been proposed. But let us take the hypothetical case Stich proposes at the end of his commentary. Given the two alternatives he suggests, anyone would certainly choose the assumption that the category A principles are universal in the absence of evidence to the contrary, in favor of the pair of conditionals. The reasons for such choices deserve consideration, but not in the present context. They do not bear specifically on psychology or linguistics.

The first question seems to me a more difficult one. Stich and others have argued that "unproblematic cases of knowledge" have certain properties not shared by "knowledge of the rules of grammar." One difference is accessibility to consciousness, and if this were the only difference, we could simply say that "cognizing" in my sense is just unconscious or tacit knowledge: something that we would call knowledge if it were conscious. But Stich suggests another distinction: namely, the unproblematic cases "form a highly integrated inferential system" whereas material internally represented in some other way involves principles "inferentially insulated" from factual belief. But here problems arise. Take our shared knowledge of the sample facts: e.g., that "the men expected each other will win" is not well-formed, with the meaning that each expected the other will win. This seems to me a relatively unproblematic case of propositional knowledge – knowledge that so-and-so. But this case forms part of a system containing inferentially insulated principles, according to Stich's account. Or suppose that our knowledge that an object on a parabolic course passing behind a screen will emerge at such-and-such a point is based on an innate principle *P*. For present purposes it is enough that this might be true, that there is nothing incoherent in assuming it to be true. This case of *knowledge-that* also seems unproblematic, indeed rather typical of much of the discussion in the literature. But if matters are as just suggested, then both of these unproblematic cases form part of a highly integrated system (though perhaps not strictly an "inferential" system) including principles (opacity, *P*) that are unconscious, innate, and perhaps inferentially insulated (see response to **Rollin**). The integrated systems may not have the properties that Stich requires. Furthermore, the elements of this system, even the unproblematic cases, might very well lack what are generally taken to be crucial features of knowledge: specifically, grounding and warrant. Thus it does not seem to me clear that the allegedly unproblematic cases have what are often regarded as typical properties of knowledge.

As for the modular approach to systems of knowledge that Stich suggests, it is, as he says, quite congenial to my view, though I suspect that many of the core beliefs that constitute what might be called our "common-sense understanding" of things and events around us are parts of integrated systems with crucial elements that are inaccessible to consciousness, possibly innate, and perhaps inferentially insulated or constitutive.

Van Gulick. Van Gulick's comments are quite apt, but they somewhat mistake my intentions. In introducing the notion "cognize," I did not hope to sidestep the problems of analysis of knowledge, but rather to focus attention on them in preference to other questions that seem to me less interesting – e.g., whether accessibility to consciousness is a necessary feature of what someone would call "knowledge." I do not think I was begging any questions in saying that if a person could become conscious of the rules of his grammar, we would not hesitate to say that he knew these rules, given the way they interact to produce cases that no one hesitates to call "knowledge" (e.g., the sample facts). But this leaves open the more interesting question of the nature of the structures and processes that enter into what we call "knowledge" when there is no issue of accessibility, and what we would call "knowledge" when this issue is set aside.

Thus I had intended to identify a certain domain of problems in introducing the concept "cognize." A person cognizes that a string of expressions has certain properties, that an object passing behind a screen will emerge at a certain point, and so on; and the person also cognizes the principles and rules that enter into determining these items, whatever their origin, in the case of cognitive systems of rules and representations. (I do not suggest, however, that "the language faculty cognizes that the rules . . . are thus-and-such.") But I agree with Van Gulick that the interesting questions arise right here: the problem is to analyze the kinds of content and processes that enter into "cognizing." Here there is no substitute for the detailed study of specific cognitive systems: language, object identification, and so on. Thus Van Gulick is quite right to say that "the shift from 'know' to 'cognize' leaves intact all those problems about how to specify the content of what is cognized, which can only be resolved by appeal to detailed theories about the internal processes involved."

I think that Van Gulick's characterization of "information possession" and "the content of information" may be too narrow. An organism may possess information about some event (say, the hour that Napoleon was born) without being structured "to interact successfully with the world along the lines of its ends or interests" by virtue of possession of this information. One may have no relevant ends or interests and no way of interacting with the world related to this information (apart from answering questions on a quiz show, in which case the notion trivializes). Furthermore, two people may possess information that is "identical in content" though they would use it quite differently; say, a tall and short person knowing that a desired object is at a certain height from the floor (which is not to deny Van Gulick's specific point about the earthworm and the person). Van Gulick's reference to causal theories of knowledge is to the point and is discussed briefly in Chomsky (1980). He is right in thinking that I find much of this work close to my own views, though I think it tends to underestimate the importance of integrated mental structures and sometimes to misconceive the nature of innate elements.

As for the discussion of "selective" and "instructive" processes and levels of description, Van Gulick is again right. Note that Jerne (1967) makes essentially the same point when he distinguishes "between learning and selection in terms of levels of analysis" (cf. p. 14 of my paper). This of course leaves open the question of which higher level processes should properly be called "learning," even at the more abstract level of analysis – a question that can only be resolved by further conceptual analysis and empirical discovery. The directions Van Gulick outlines may well contribute to this end.

References

Adinolfi, M. (1978) The immune system and the brain. *Developmental Medicine and Child Neurology* 20:509–16. [JCM]

Andor, J. (1978) Case grammar, deep semantic relations and cognition. In: W. U. Dressler and W. Meid, (eds.) Proceedings of the Twelfth International Congress of Linguists, pp. 163–66. Innsbruck: Innsbrucker Beiträge zur Sprachwissenschaft, [JA]

Aslin, R. N., and Pisoni, D. B. (1980) Some developmental processes in speech perception. In: G. H. Yeni-Komshian; J. F. Kavanagh; and C. A. Ferguson (eds.) *Child phonology: perception and production.* New York: Academic Press. [GG]

Bard, J. B. L. (1977) A unity underlying the different zebra striping patterns. *Journal of Zoology, London* 183:527–39. [JCM]

Beilin, H. (1975) *Studies in the cognitive basis of language development.* New York: Academic Press. [RFC]

Bellugi, U. (1967) *The acquisition of the system of negation in children's speech.* Unpublished doctoral dissertation. Harvard University. [RFC]

(1971) Simplification in children's language. In: R. Huxley and E. Ingram (eds.) *Language acquisition: models and methods*, pp. 95–119. London and New York: Academic Press. [RFC]

Bernstein, L. (1976) *The unanswered question.* Cambridge: Harvard Univ. Press. [NC]

Bever, T. G. (1970) The cognitive basis for linguistic structures. In: J. R. Hayes (ed.) *Cognition and the development of language*, pp. 279–362. New York: John Wiley & Sons. [RFC]

Bever, T. G.; Lackner, J. R.; and Kirk, R. (1969) The underlying structures of sentences are the primary units of immediate speech processing. *Perception and Psychophysics* 5:225–34. [GH]

Blakemore, C. (1973) Developmental factors in the formation of feature extracting neurons. In: F. O. Schmitt and F. G. Worden (eds.) *The neurosciences: third study program.* Cambridge: MIT Press [NC]

Blank, M.; Gessner, M.; and Esposito, A. (1978) Language without communication: a case study (Mimeo). Dept. of Psychiatry, Rutgers Medical Sch. Piscataway, N.J. [NC]

Bower, G. H.; Black, J. B.; and Turner, T. J. (1979) Scripts in text comprehension and memory. *Cognitive Psychology* 11:177–220. [RCS]

Bradley, D.; Garrett, M. F.; and Zurif, E. (1979) Syntactic deficits in Broca's Aphasia. In: D. Caplan (ed.) *Biological studies of mental processes.* Cambridge: MIT Press. [JM]

Bresnan, J. (1978) A realistic transformational grammar. In: M. Halle; J. Bresnan and I. Miller (eds.) *Linguistic theory and psychological reality.* Cambridge: MIT Press. [JM]

Bruner, J. S.; Goodnow, J. J.; and Austin, G. A. (1956) *A study of thinking.* New York: John Wiley & Sons. [RFC]

Bryant, P. (1974) *Perception and understanding in young children: an experimental approach.* New York: Basic Books. [NC]

Catlin, J. (1978) Discussion of the chapters by Stolzenberg and Chomsky. In: G. A. Miller and E. Lenneberg (eds.) *Psychology and biology of language and thought*, pp. 271–80. New York: Academic Press. [RFC]

Chomsky, N. (1957) *Syntactic structures.* The Hague: Mouton. [NC, RC]

(1962) Explanatory models in linguistics. In: E. Nagel; P. Suppes; and A. Tarski, (eds.) *Logic, methodology, and philosophy of science.* Stanford: Stanford Univ. Press. [RJM]

(1965) *Aspects of the theory of syntax.* Cambridge: MIT Press. [NC, JDM, RJM]

(1967) The formal nature of language. In: E. H. Lenneberg (ed.) *Biological foundations of language.* New York: Wiley. [NC, RFC]

(1968) *Language and mind.* New York: Harcourt Brace & World. [GS]

(1969) Some empirical assumptions in modern philosophy of language. In: S. Morgenbesser; P. Suppes; and M. White (eds.) *Philosophy, Science and Method* (Essays in honor of Ernest Nagel) New York: St. Martin's Press.

(1972) *Language and mind.* New York: Harcourt Brace Jovanovich. [NC]

(1972a) *Studies on semantics in generative grammar.* The Hague. Mouton. [NC]

(1973) *Problems of knowledge and freedom.* London: Fontana. [GS]

(1975a) *Reflections on language.* New York: Pantheon. [NC, GH, GL, JDM]

(1975b) Knowledge of language. In: K. Gunderson (ed.) *Language, mind and knowledge.* Minneapolis. Univ. of Minnesota Press. [NC]

(1976a) *Reflections on language.* London: Temple Smith. [GS]

(1976) On the nature of language. In: Harnad, S. R.; Steklis, H. D.; and Lancaster, J. (eds.) Origins and evolution of language and speech, p. 280. *Annals of the New York Academy of Sciences.* [NC]

(1977) *Essays on form and interpretation.* New York: North Holland Elsevier. [NC]

(1978) A theory of core grammar. *Glot* 1:7–26. [JCM]

(1980) *Rules and representations.* New York: Columbia Univ. Press. [NC]

(Forthcoming) Human language and other semiotic systems. *Semiotica.* [NC]

and Halle, Morris (1968) The sound pattern of English. New York: Harper and Row. [JDM]

and Lasnik, H. (1977) Filters and control. *Linguistic Inquiry* 7:425–504. [JCM]

Clark, E. V. (1971) On the acquisition of the meaning of *before* and *after*. *Journal of Verbal Learning and Verbal Behaviour* 10:266–75. [RFC]

(1973a) How children describe time and order. In: C. A. Ferguson and D. I. Slobin (eds.) *Studies of child language development*, pp. 585–606. New York: Holt, Rinehart, and Winston. [RFC]

(1973b) Non-linguistic strategies and the acquisition of word meaning. *Cognition* 2:161–82. [RFC]

Coltheart, M.; Patterson, K.; and Marshall, J. C., (eds.) (1980) *Deep dyslexia*. London: Routledge and Kegan Paul. [JCM]

Cooper, D. (1975) *Knowledge of language*. New York: Prism Press, London and Humanities Press. [SPS]

Cromer, R. F. (1968) The development of temporal reference during the acquisition of language. Unpublished doctoral dissertation, Harvard University. [RFC]

(1974) The development of language and cognition: the cognition hypothesis. In: B. Foss (ed.) *New perspectives in child development*, pp. 184–252. Harmondsworth, Middlesex: Penguin Books. [RFC]

(1976a) The cognitive hypothesis of language acquisition and its implications for child language deficiency. In: D. M. Morehead, and A. E. Morehead (eds) *Normal and deficient child language*, pp. 283–333. Baltimore, Maryland: University Park Press. [RFC]

(1976b) Developmental strategies for language. In: V. Hamilton, and M. D. Vernon (eds.) *The development of cognitive processes*, pp. 305–58. London and New York: Academic Press. [RFC]

(in press) Reconceptualizing language acquisition and cognitive development. In: R. L. Schiefelbusch, and D. Bricker (eds.) *Early language intervention*. Baltimore: University Park Press. [RFC]

Curtiss, S. (1977) *Genie: a psycholinguistic study of a modern-day "wild child."* New York: Academic Press. [NC]

Danto, A. C., and Morgenbesser, S. (1957) Character and free will. *Journal of Philosophy* 54:502. [NC]

Davis, L., and Gardner, H. (1976) Strategies of mastering a visual communication system in aphasia. In: Harnad, S. R.; Steklis, H. D.; and Lancaster, J. (eds.) Origins and evolution of language and speech. *Annals of the New York Academy of Sciences:* 280. [NC]

Denenberg, V. H.; Garbanati, J.; Sherman, G.; Yutzey, D. A.; and Kaplan, R. (1978) Infantile stimulation induces brain lateralization in rats. *Science* 201:1150–51. [NC]

Dennett, D. C. (1975) Why the law of effect will not go away. *Journal of the Theory of Social Behavior* 5:169-87. [NC]

(1978) *Brainstorms*. Montgomery, Vt.: Bradford Books. [SPS]

Dennis, M. (1978) Language acquisition in a single hemisphere: semantic organization. In: D. Caplan (ed.) *Biological studies of mental processes*. Cambridge: MIT Press. [NC]

(1980) Capacity and strategy for syntactic comprehension after left or right hemidecortication. *Brain and Language* 10. [JCM]

and Whitaker, H. (1976) Language, acquisition following hemidecortication: linguistic superiority of the left over right hemisphere. *Brain and Language* 3:404–33. [NC]

Dewart, M. H. (1975) A psychological investigation of sentence comprehension by children. Unpublished doctoral dissertation. University College, London. [RFC]

Donaldson, M. (1978) *Children's minds*. Glasgow: Fontana/Collins. [RFC]

and Balfour, G. (1968) Less is more: a study of language comprehension in children. *British Journal of Psychology* 59:461–72. [RFC]

and McGarrigle, J. (1974) Some clues to the nature of semantic development. *Journal of Child Language* 1:185–94. [RFC]

Donnellan, K. S. (1977) Review of Gunderson. *Language*. 53(3). [NC]

Dresher, B. E., and Hornstein, N. (1976) On some supposed contributions of artificial intelligence to the scientific study of language. *Cognition* 4:321–398. [NC]

(1977) Reply to Schank and Wilensky. *Cognition* 5:147–49. [NC]

Ferreiro, E. (1971) *Les relations temporelles dans le langage de l'enfant*. Genève: Librairie Droz. [RFC]

and Sinclair H. (1971) Temporal relations in language. *International Journal of Psychology* 6: 39–47. [RFC]

Fillmore, C. J. (1975) An alternative to checklist theories of meaning. In: C. Cogen et al. (eds.) Proceedings of the First Annual Meeting of the Berkeley Linguistics Society, Berkeley, pp. 123–31. [JA]

(1977) The case for case reopened. In: P. Cole and J. M. Sadock (eds.) Syntax and Semantics Vol. 8., Grammatical Relations, pp. 59–81. New York: Academic Press. [JA]

Fodor, J. A. (1975) *Language of thought*. New York: Thomas Crowell. [NC, HR]

Fodor, J. D. (1978) Parsing strategies and constraints on transformations. *Linguistic Inquiry* 9:427–73. [JCM]

Frankfurt, Harry, (1977) Descartes on the creation of the eternal truths. *The Philosophical Review*, LXXXVI:36–57. [DMR]

Galaburda, A. M.; Sanides, F.; and Geschwind, N. (1978) Human Brain: Cytoarchitectonic left-right asymmetries in the temporal speech region. *Archives of Neurology* 35:812–17. [JCM]

Gardner, H.; Zurif, E.; Berry, T.; and Baker, E. (1976) Visual communication in aphasia. *Neuropsychologia* 14:275–92. [NC]

Ghiselin, M. T. (1974) A radical solution to the species problem. *Systematic Zoology* 23:536–44. [MTG]

(1978) The economy of the body. *American Economic Review* 68:233–37.

(1980 In press) Natural kinds and literary accomplishments. *Michigan Quarterly Review*. [MTG]

Glass, A. V.; Gazzaniga, M. S.; and Premack, D. (1973) Artificial language training in global aphasics. *Neuropsychologia* 11:95–103. [NC, RFC]

Goosens, W. K. (1978) Reduction by molecular genetics. *Philosophy of Science* 45:73–95. [JCM]

Gottlieb, G. (1976a) The roles of experience in the development of behavior and the nervous system. In.: G. Gottlieb (ed.) *Development of neural and behavioral specificity*. New York: Academic Press. [GG]

(1976b) Conceptions of prenatal development: behavioral embryology. *Psychological Review* 83:215–34. [GG]

(in press) Development of species identification in ducklings: VI. Specific embryonic experience required to maintain species-typical perception in Peking ducklings. *Journal of Comparative and Physiological Psychology*. [GG]

Graesser, A. C.; Gordon, S. E.; and Sawyer, J. D. (1979) Recognition memory for typical and atypical actions in scripted activities: tests of a script pointer and tag hypothesis. *Journal of Verbal Learning and Verbal Behavior* 18:319–32. [RCS]

Graves, C.; Katz, J. J.; Nishiyama, Y.; Soames, S.; Stecker, R.; and Tovey, P. (1973) Tacit knowledge. *Journal of Philosophy* 70. [SPS]

Grice, H. P. (1957) Logic and Conversation. Unpublished William James lectures, Harvard Univ. [JRS]

Harman, G. (1973) *Thought*. Princeton: Princeton Univ. Press. [GH]

Harnad, S. R.; Steklis, H. D.; and Lancaster, J. (eds.) (1976) Origins and evolution of language and speech. *Annals of the New York Academy of Sciences:* 280. [NC]

Hill, J. H., and Most, R. B. (1978) Review of Harnard, Steklis, and Lancaster (1976) Language 54:651–2. [NC]

Hindemith, P. (1961) *A composer's world*. New York: Anchor. [NC]

Hollander, B. (1920) *In search of the soul*. London: Kegan Paul. [JCM]

Hubel, D. H. (1978) Vision and the brain. *Bulletin of the American Academy of Arts and Sciences*. 31:28. [NC]

Hughes, J. (1972) Language and communication: acquisition of a non-vocal "language" by previously languageless children. Unpublished Bachelor of Technology thesis, Brunel University. [RFC]

(1974/75) Acquisition of a non-vocal "language" by aphasic children. *Cognition* 3:41–55. [RFC]

Hull, D. L. (1975) Central subjects and historical narratives. *History and Theory* 14:253–74. [MTG]

(1976) Are species really individuals? *Systematic Zoology* 25:174–91. [MTG]

Huttenlocher, J., and Strauss, S. (1968) Comprehension and a statement's relation to the situation it describes. *Journal of Verbal Learning And Verbal Behaviour* 7:300–4. [RFC]

Huttenlocher, J.; Eisenberg, K.; and Strauss, S. (1968) Comprehension: relation between perceived actor and logical subject. *Journal of Verbal Learning and Verbal Behaviour* 7:527–30. [RFC]

Jackendoff, R. (1977) Review-article: Bernstein, The unanswered questions. *Language* 53:883–894. [NC]

(1979) How to keep ninety from rising. *Linguistic Inquiry*. 10:172–177. [NC]

and Lerdahl, F. (forthcoming) *A generative theory of tonal music*. Cambridge: MIT Press. [NC]

Jacob, F. (1977) Evolution and tinkering. *Science* 196:1161–64. [NC]

(1978) Darwinism reconsidered. *Le Monde* Sept. 6–8, 1977; translated in *Atlas*. [NC]

Jahoda, G. (1979) On the nature of difficulties in spatio-perceptual tasks. *British Journal of Psychology* 70:351–63. [AM]

Jaisson, P. (1975) L'impregnation dans l'ontogenese des comportements de soins aux cocons chez la jeune formi rousse (*Formica polyctena* Forst.) *Behaviour* 52:1–37. [GG]

Jenkins, L. (1978) Language and genetics. *Theoretical Linguistics* 5:77–82. [JCM]

Jerne, N. K. (1967) Antibodies and learning: selection versus instruction. In: G. C. Quarton; T. Melnechuk; and F. O. Schmitt (eds.) *The neurosciences: a study program*. New York: Rockefeller Univ. Press. [NC]

Karmiloff-Smith, A. (1977) The child's construction of a system of plurifunctional markers. In: M. Bullowa (Chair) *Language development*. Symposium presented at the biennial conference of the International Society for the Study of Behavioural Development, Pavia, Italy. [RFC]

(1978) The interplay between syntax, semantics, and phonology in language acquisition processes. In: R. N. Campbell, and P. T. Smith (eds.) *Recent advances in the psychology of language: language development and mother-child interaction*, pp. 1–23. New York: Plenum Press. [RFC]

(1979) *A functional approach to child language: a study of determiners and reference*. Cambridge: Cambridge Univ. Press. [RFC]

Kasher, A. (1977) Foundations of philosophical pragmatics. In: R. E. Butts and J. Hintikka (eds.) *Basic problems in methodology and linguistics: logic, methodology and philosophy of science*, V. Dordrecht: Reidel. [NC]

Katz, J. (1972) *Semantic theory*. New York: Harper & Row. [NC]

Keil, F. C. (1979) *Semantic and conceptual development*. Cambridge: Harvard Univ. Press. [NC]

Kenny, Anthony, translator and editor. (1970) *Descartes: philosophical letters*. Oxford: Clarendon Press. [DMR]

Kintsch, W. (1977) Memory and cognition. New York: John Wiley. [JA]

and van Dijk, T. A. (1978) Toward a model of text comprehension and production. *Psychological review* 85:363–94. [JA]

Kitcher, P. (1978) The nativist's dilemma. *Philosophical Quarterly*. [NC]

Koehler, O. (1956) The ability of birds to "count." In: J. R. Newman, (ed.) *The world of mathematics*. New York: Simon & Schuster. [NC]

Kosslyn, S. (forthcoming) *Image and mind*. Cambridge: Harvard Univ. Press. [NC]

Koster, J. (1978) *Locality principles in syntax*. Dordrecht: Foris. [NC]

Lakoff, G. (1977) Linguistic gestalts. *Proceedings of the Chicago Linguistics Society*, 1050 E. 59 St., Chicago. [GL]

and Johnson, M. (1980) *Metaphors we live by*. Chicago: Univ. of Chicago Press. [GL]

Lehnert, W. G. (1977) *The process of question answering*. Hillsdale, N.J.: Lawrence Erlbaum Associates. [RCS]

Lesky, E. (1970) Structure and function in Gall. *Bulletin of the History of Medicine* 44:297–314. [JCM]

Lewes, G. H. (1871) *The history of philosophy from Thales to Comte*. London: Longmans. [JCM]

Lightfoot, D. W. (1979) *Principles of diachronic syntax*. New York and Cambridge: Cambridge Univ. Press. [NC]

Limber, J. (1977) Language in child and chimp? *American Psychologist* 32:280–94. [NC]

Lorenz, K. (1965) *The evolution and modification of behavior*. Chicago: Univ. of Chicago Press. [RVG]

Lycan, W. (in preparation) Functionalism and psychological laws. [SPS]

Macnamara, J. (1972) Cognitive basis of language learning in infants. *Psychological Review* 79:1–13. [RFC]

Maratsos, M. P. (1973) The effects of stress on the understanding of pronominal co-reference in children. *Journal of Psycholinguistic Research* 2:1–8. [RFC]

Marcus, M. (1978) *A theory of syntactic recognition for natural language*. Unpublished Ph.D. Thesis, Massachusetts Institute of Technology. [PTWH]

Marr, D. (1976) Early processing of visual information. *Philosophical Transactions of the Royal Society*, B, 275:483–524. [JCM]

and Nishihara, H. K. (1978) Visual information processing: artificial intelligence and the sensorium of sight. *Technology Review* 81:2–23. [NC]

Matthews, R. (1979) The plausibility of rationalism. To be presented to the annual meeting of the Society for Philosophy and Psychology, March, 1980. [RJM]

Mehler, J. and Bertendini, J. (1979) Infants' perception of speech and other acoustic stimuli. In: J. Morton and J. Marshall (eds.) *Psycholinguistic Series II*. Cambridge: MIT Press. [NC]

Minsky, M. (1977) Frame-system theory. In: P. N. Johnson-Laird and P. C. Wason (eds.) Thinking, pp. 355–76. Cambridge: Cambridge Univ. Press. [JA]

Moravcsik, J. (1975) Aitia as generative factor in Aristotle's philosophy. *Dialogue*. [NC]

(1977) "How do words get their meaning?" Yehoshua Bar-Hillel lecture, Jerusalem, forthcoming. [NC]

(1979) Understanding. *Dialectica*. [NC]

Nagel, T. (1969) Linguistics and epistemology. In: S. Hook (ed.) *Language and Philosophy*. New York: New York Univ. Press. [SPS]

Nelson, K., and Gruendel, J. (1978) From person episode to social script: two dimensions in the development of event knowledge. Paper presented at the Biennial Meeting of the Society for Research in Child Development, San Francisco. [RCS]

Newport, E.; Gleitman, H.; and Gleitman, L. (1977) Mother, I'd rather do it myself: some effects and non-effects of maternal speech style. In: C. E. Snow and C. A. Ferguson (eds.) *Talking to children: language input and acquisition*. Cambridge: Cambridge Univ. Press. [NC]

Ojemann, G., and Mateer, C. (1979) Human language cortex: localization of memory, syntax, and sequential motor-phoneme identification systems. *Science* 205:1401–3. [JCM]

Osherson, D. N. (1976) *Logical abilities in children*. New York: Wiley. [NC]

Pinker, S. (1979) Formal models of language learning. *Cognition* 7:217–83. [PTWH, RJM]

Premack, D. & Woodruff, G. (1978a) Chimpanzee problem-solving: a test for comprehension. *Science* 202:532–35. [NC]

(1978b) Does the Chimpanzee have a theory of mind? *The Behavioral and Brain Sciences* 1:515–26. [NC]

Putnam, H. (1962) It ain't necessarily so. *Journal of Philosophy* 59:658–71. [GH]

Quine, W. V. O. (1960). *Word and object*. New York: John Wiley & Sons. [JMM]

Quine, W. V. O. (1975) Mind and verbal dispositions. In: S. Guttenplan (ed.) *Mind and language*. London: Oxford. [NC]

Reed, E. S. (1979) The role of symmetry in Ghiselin's "radical solution to the species problem." *Systematic Zoology* 28:71–78. [MTG]

Rizzi, L. (1978) Violations of the *Wh* island constraint in Italian and the subjacency condition. In: C. Dubisson; D. Lightfoot; and Y. C. Morin (eds.) *Montreal Working Papers in Linguistics* 2:155–90. [NC]

Rollin, B. (1971) Hume's blue patch and the mind's creativity. *Journal of the History of Ideas* XXXII:119ff. [BER]

(1976) *Natural and conventional meaning: an examination of the distinction*. The Hague: Mouton. [BER]

(1978) Thomas Reid and the semiotics of perception. *The Monist* 61:257ff. [BER]

Sampson, G. R. (1978) Linguistic universals as evidence for empiricism. *Journal of Linguistics* 14:183–206. [GS]

(1979) A non-nativist account of language universals. *Linguistics and Philosophy* 3:99–104. [GS]

(1980) *Making sense*. Oxford and New York: Oxford Univ. Press. [GS]

Sapir, E. (1933) The psychological reality of the phonene. Reprinted in English translation in: D. G. Mandelbaum (ed.) *Selected writings of Edward Sapir*. Berkeley and Los Angeles: Univ. of California Press, 1949. [NC]

Savage-Rumbaugh, E. S.; Rumbaugh, D. M.; and Boysen, S. (1978) Symbolic communication between two chimpanzees. *Science* 201:641–42. [NC]

Schlesinger, I. M. (1977) Production and comprehension of utterances. Hillsdale, N.J.: L. Erlbaum [JA]

(1979) Cognitive structures and semantic deep structures. *Journal of Linguistics* 15:307–24. [JA]

Schopenhauer, A. (1974) The fourfold root of the principle of sufficient reason. La Salle, Illinois: Open Court. [NC]

Schwartz, R. (1969) On knowing a grammar. In: S. Hook (ed.) *Language and philosophy*: New York: New York Univ. Press. [SPS]

Searle, J. R. (1969) *Speech acts*. Cambridge and New York: Cambridge Univ. Press. [JRS]

(1976) The rules of the language game. Review article on *Reflections on Language*. *Times Literary Supplement*. Sept. 10. [NC, JRS]

Selfridge, M. (1979) A process model of language acquisition. Ph.D. Thesis, Department of Computer Science, Yale University, New Haven, CT. [RCS]

Serpell, R. (1979) How specific are perceptual skills? *British Journal of Psychology* 70:365–80. [AM]

Sheldon, A. (1974) The role of parallel function in the acquisition of relative clauses in English. *Journal of Verbal Learning and Verbal Behaviour* 13:272–81. [RFC]

Simon, H. A. (1962) The architecture of complexity. *Proceedings of the American Philosophical Society* 106:467–82; reprinted (1969) in *The sciences of the artificial*, Cambridge, Mass.: MIT Press. [GS]

Slobin, D. I. (1978) Universal and particular in the acquisition of language. Paper presented at the workshop-conference, "Language acquisition: state of the art," University of Pennsylvania. May 19–22. [RFC]

(1979) The role of language in language acquisition. Paper presented at the Eastern Psychological Association, Philadelphia, April 21. [RFC]

Smedslund, J. (1961) The acquisition of conservation of substance and weight in children: III. Extinction of conservation of weight acquired "normally" and by means of empirical controls on a balance. *Scandinavian Journal of Psychology* 2:85–87. [RFC]

Smith, N. (1979) Syntax for psychologists. In: J. Morton, and J. C. Marshall (eds.) *Psycholinguistics Series*, Vol. 2. London: Elek. [JCM]

Snow, C. E., and Ferguson, C. A. (eds.) (1977) *Talking to children: language input and acquisition*. Cambridge: Cambridge Univ. Press. [RFC]

Staddon, J. E. R., and Simmelhag, V. L. (1971) The "superstitution" experiment: a reexamination of its implications for the principles of adaptive behavior. *Psychological Review* 78:16–43. [HR]

Stich, S. (1971) What every speaker knows. *Philosophical Review* 80:476–96. [SPS]

(1978a) Empiricism, innateness and linguistic universals. *Philosophical Studies* 33:273–86. [SPS]

(1978b) Beliefs and subdoxastic states. *Philosophy of Science* 45:499–518. [SPS]

(1979) Between Chomskian rationalism and Popperian empiricism. *British Journal for the Philosophy of Science* 30. [SPS]

Strohner, H., and Nelson, K. E. (1974) The young child's development of sentence comprehension: influence of event probability, nonverbal context, syntactic form, and strategies. *Child Development* 45:567–76. [RFC]

Taub, E. (1976) Motor behavior following deafferentation in the developmentally and motorically mature monkey. In: R. M. Herrman; S. Grillner; P. S. G. Stein; and D. G. Stuart (eds.) *Neural control of locomotion*. New York: Plenum. [NC]

Terrace, H. (forthcoming) Is problem solving language? *Journal of the Experimental Analysis of Behavior*. [NC]

Twaddell, W. F. (1935) On defining the phoneme. *Language Monograph* No. 16. [NC]

Ullman, S. (1978) *The interpretation of visual motion*. Cambridge: MIT Press. [NC]

van Dijk, T. A. (1977) Context and Cognition: Knowledge frames and speech act comprehension. *Journal of Pragmatics* 1:211–32. [JA]

Velletri-Glass, A.; Gazzaniga, M.; and Premack, D. (1973) Artificial language training in global aphasics. *Neuropsychologia* 11:95–104. [NC, RFC]

Walter, J. G. (1805) *Etwas über Herrn Dr. Gall's Hirnschädellehre*. Berlin: Scherer. [JCM]

Wexler, K. (1978) A principle theory for language acquisition, *Social Science Research Reports, vol. 20*, Univ. of California, Irvine. [NC]

Wiens, J. A. (1970) Effects of early experience on substrate pattern selection in *Rana aurora* tadpoles. *Copeia* No. 3, 543–48. [GG]

Wilcox, S., and Palermo, D. S. (1975) "In," "on," and "under" revisited. *Cognition* 3:245–54. [RFC]

Wilkes, K. V. (1980) Brain lesions. *Philosophical Quarterly*, forthcoming. [AM]

Williams, M. (1977) *Groundless belief*. New Haven: Yale Univ. Press. [NC]

Winograd, T. (1972) Understanding natural language. *Cognitive Psychology* 2:1–191. [SPS]

(1976) Towards a procedural understanding of semantics. *Revue Internationale de Philosophie* 30:260–303. [GH]

Young, R. M. (1970) *Mind, brain and adaptation in the nineteenth century*. Oxford: Clarendon Press. [JCM]

Zangwill, O. L. (1978). Aphasia and the concept of brain centers. In: G. A. Miller, and E. Lenneberg (eds.) *Psychology and biology of language and thought*. New York: Academic Press. [JM]

Zeki, S. M. (1978) Functional specialization in the visual cortex of the rhesus monkey. *Nature* 274:423–28. [JCM]