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The Psychology of Language and Thought

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Noam Chomsky interviewed by Robert W. Rieber

QUESTION: What role does cognition play in the acquisition and development of language? Do linguistic factors influence general cognitive development?

CHOMSKY: I would like to re-phrase the first question and ask what role other aspects of cognition play in the acquisition of language since, as put, it is not a question I can answer. I would want to regard language as one aspect of cognition and its development as one aspect of the development of cognition. It seems to me that what we can say in general is this:

There are a number of cognitive systems which seem to have quite distinct and specific properties. These systems provide the basis for certain cognitive capacities -- for simplicity of exposition, I will ignore the distinction and speak a bit misleadingly about cognitive capacities. The language faculty is one of these cognitive systems. There are others. For example, our capacity to organize visual space, or to deal with abstract properties of the number system, or to comprehend and appreciate certain kinds of musical creation, or our ability to make sense of the social structures in which we play a role, which undoubtedly reflects conceptual structures that have developed in the mind, and any number of other mental capacities. As far as I can see, to the extent that we understand anything about these capacities, they appear to have quite specific and unique properties. That is, I don't see any obvious relationship between, for example, the basic properties of the structure of language as represented in the mind on the one hand and the properties of our capacity, say, to recognize faces or understand some situation in which we play a role, or appreciate music and so on. These seem to be quite different and unique in their characteristics. Furthermore, every one of these mental capacities appears to be highly articulated as well as specifically structured. Now it's perfectly reasonable to ask how the development of one of these various systems relates to the development of others. Similarly, in the study of, say, the physical growth of the body, it makes perfect sense to ask how the development of one system relates to the development of others. Let's say, how the development of the circulatory system relates to the development of the visual system.

But in the study of the physical body, nobody would raise a question analogous to the one you posed in quite this form. That is, we would not ask what role physical organs and their function play in the development of the visual system. Undoubtedly, there are relations between, say, the visual and circulatory systems, but the way we approach the problem of growth and development in the physical body is rather different. That is, one asks -- quite properly -- what are the specific properties and characteristics of the various systems that emerge -- how do these various organs or systems interact with one another, what is the biological basis -- the genetic coding, ultimately -- that determines the specific pattern of growth, function and interaction of these highly articulated systems: for instance, the circulatory system, the visual system, the liver, and so on. And that seems to provide a reasonable analogy, as a point of departure at least, for the study of cognitive development and cognitive structure, including the growth of the language faculty as a special case.

QUESTION: It might help if you could define how you use the term "cognition" as opposed to the term "language."

CHOMSKY: Well, I wouldn't use the term "cognition" as opposed to the term "language." Rather, cognition is an overall term that includes every system of belief, knowledge, understanding, interpretation, perception, and so on. Language is just one of many systems that interact to form our whole complex of cognitive structures. So it's not a matter of language as compared with cognition any more than one could study, say, our knowledge of the structure of visual space as compared with cognition. Furthermore, I don't believe that one can think of "cognition" as a unitary phenomenon.

QUESTION: Cognition is a way of knowing and language is a medium whereby we know?

CHOMSKY: Not as I am using the terms, the term "cognition" as far as I understand it simply refers to any aspect of our belief, knowledge, or understanding. Now among the various cognitive systems and cognitive structures, one of them happens to be the system of language. We know language more or less as we have a system of beliefs and understanding about, say, the nature of the visual world.

QUESTION: So it's a separate system, is it not?

CHOMSKY: It's one of the many systems entering into an array of interconnected cognitive structures. Perhaps the analogy to physical organs is the best way to explain the way I see it. Let's just ask, how do we study the structure of the body? We begin by a process of idealization, in effect. We say there are -- we assume there are -- various systems that interact to constitute our physical body. For example, the visual system and the circulatory system and so on. Now this is, of course, an idealization; the systems are not physically separable. The circulatory system interacts with the visual system physically.

QUESTION: But the CNS and the ANS are separable....

CHOMSKY: Only under a certain idealization, which is assumed to be an appropriate one. Well, you can study the structure of each of these systems and the mode of their interaction. Everyone assumes that this is a proper way to study anything as complicated as the human body: by isolating for investigation particular systems that have their own specific structure and a specific mode of development, recognizing of course that they are not isolated from one another -- that the mode of their interaction is just as much genetically determined as are their specific characteristics. So, using the term organ, in a slightly extended sense, to include something like, say, the circulatory system -- not the usual sense -- we might regard the body as a system of physical organs, each with its specific properties and peculiarities and with a mode of interaction, all genetically determined in basic outline, but modified in various ways in the course of growth.

Now, I think that there is every reason to suppose that the same kind of "modular" approach is appropriate for the study of the mind -- which I understand to be the study, at an appropriate level of abstraction, of properties of the brain -- and in particular for the general system of cognitive structures, which does not exhaust the mind, but is the part we're talking about. That is to say, I'd like to think of the system of cognitive structures as, in effect, a system of "mental organs," each of which is quite specific, highly articulated, developing in a particular manner that is intrinsically determined -- if the biologists are right, genetically coded -- with, of course, complex interactions that are also very largely predetermined. It seems to me that, insofar as we understand anything about cognition -- about some aspects of cognition -- we discover very specific mental structures developing in the course of growth and maturation in quite their own way. And language is simply one of these structures.

I'm sure if we were to study, to take another distinctly human characteristic, our capacity to deal with properties of the number system -- it's unique to humans, as far as we know, a specific capacity of the human mind -- one might, for example, try to explore the properties of that system in the mature person. We might then ask how that system develops through childhood, what kind of stimulation from the environment is necessary for it to develop to its mature state, and so on. In doing so we would have studied the growth of a particular mental organ to its mature state, and if we could pursue this enterprise successfully, we could, at least on an abstract level, characterize the principles that determine the structure of this mental organ, principles that must be themselves genetically coded in some fashion. (The language system can be and, in fact, is being studied in essentially this way. Similarly, we could study the other mental organs that I mentioned before or others.) In this way we could develop what seems to me a reasonable version of a "faculty psychology."

QUESTION: When you talk about this language structure system, are you referring to all language, nonverbal language, and language as a developmental process?

CHOMSKY: Here we have to be a little careful. The term "language" is used in quite different ways, and only confusion can arise from failure to distinguish them. In the first place, the term is used to refer to human languages, that is, a specific biological characteristic of humans. There is a human language faculty which allows us to develop the kind of knowledge that you and I share that makes it possible for us to conduct this conversation. And that capacity is simply part of the species-specific biological endowment. Putting aside possible individual variation, we may think of this faculty as a common and as far as we know uniquely human possession. In terminology that is now fairly standard, we may refer to a characterization of central properties of this faculty as "universal grammar," a system that we may regard as analogous to basic properties of the human visual system. That is one use of the term "language." Each human language is one of the various specific systems that can emerge within that set of initial constraints.

The term "language" is often used in quite a different way, referring not to some specific biologically determined system, but rather to any mode of communication or mode of expression, in some very general sense. So, for example, when one talks about the language of gesture or the language of the bees, or the language of ape calls, or when one asks whether music is a language or mathematics is a language and so on, in any of those questions and discussions, some notion of "language" is presupposed which is very different from the former sense.

QUESTION: I was really thinking of something else. I was thinking of the notion that some people believe, namely that oral language, verbal language in the child is a development of something that happens prior to the emergence of spoken language -- nonverbal activities such as pointing, etc., cognitive activity -- pre-language rites as it were.

How is the acquisition and development of language influenced by interpersonal and intrapersonal verbal and nonverbal behavior?

CHOMSKY: It depends on what aspect of language one is talking about.

QUESTION: Say, the first word, for instance.

CHOMSKY: Let's take the first word and assume that it's a name. Suppose the child's first word is some name for its mother, or something like that. In the act of reference, obviously other cognitive capacities come into play. That is, before a child can refer to some object in its external environment, it has to have isolated and identified objects in its environment. It has to have recognized that there are people, that there are things, and that they have

certain properties -- constancies and persistence and so on. Unless all of this organization has already taken place, there is nothing to refer to. Therefore, the act of reference can't take place. I don't think there is any special reason to believe that any of those competences are learned. I assume that the capacities that enable us to isolate and identify physical objects in the outside world and understand their properties -- capacities which we might also think of as forming some mental organ -- are just as much genetically determined in their specific characteristics as is the language faculty. But there is no doubt that in, for example, using a word to refer to an object, that kind of organization is presupposed, however it is developed. That's almost tautological. So in that respect, of course, other cognitive capacities enter crucially into any use of language, including the earliest use. However, that doesn't tell us very much. To take a physical analogy, we might also say that unless the circulatory system is functioning, the visual system is inoperative. It's perfectly correct, but it doesn't tell us anything about the structure of the visual system.

The kind of question that ought to be raised in connection with the growth of language is just the kind of question that we raise in connection with the growth of some other system, say, the visual system. What are the structural and functional properties that emerge as this system grows and matures? What are the principles that govern this growth and that are realized in the systems that develop? To what extent are these principles invariant and biologically determined? To what extent do the properties of the system that develops simply mirror accidental contingencies of experience? To what extent do they reflect other independently developing capacities, and so on. I think that as far as we know the growth and emergence of the language faculty is highly specific. By the time the child has the most rudimentary knowledge of language, say at three years old, a normal child -- and in fact any child, apart from really serious pathology -- is using principles that as far as we know have no close analogue in other mental faculties. After all, what are the basic properties of language, the most rudimentary and elementary properties of language, which emerge quite early -- certainly a four-year-old has already developed them very extensively.

The most elementary property of language that one can think of, I guess, is that it involves a discrete infinity; that is, there is an infinite range of possible constructions -- there is no longest sentence. This is not a continuous system, that is, it does not involve variation along some continuous dimension, as say the bee language does in principle; but rather there is a discrete infinity of possible expressions, each with its form and its meaning. That property of language manifests itself at an extremely early point. Prior to this point one might want to say that there is no language in the sense of "human language." Prior to that point, it would make sense to say that we have something analogous to the incipient motions of fluttering of wings of a bird before its capacity to fly has matured, perhaps. But at the point at which the system of a discrete infinity of utterances manifests itself, and that's very early, we can say that we have at least the rudiments of human language emerging. As for the principles that organize and characterize that discrete infinity of utterances with their forms and meaning, obviously this system must be represented in a finite mind -- ultimately, neurally represented in a finite brain -- which means that there must be some finite system of rules which operate in some fashion to characterize the unbounded range of possible expressions, each with its fixed form and meaning. And knowledge of language means nothing more than internal representation, ultimately neural representation of that system.

Perhaps the next most elementary property of language is that these rules basically operate on phrases; that is, they don't operate on a string of words, a sequence of words, but on words organized into larger units. Then, as we go on to further properties of language, we discover ways in which the rules operate on phrases and on hierarchic structures of phrases in order to form more complex expressions by recursive embedding

and other principles. As far as I can see, these are the most elementary properties of human language. But even these elementary properties, so far as we know, have no significant analogues in other systems.

There are, of course, quite different views of the matter. Piaget and his colleagues, if I understand them, take the position that the emerging structures of language necessarily reflect sensorimotor constructions. I have never understood exactly what they mean by this claim. If they are saying, for example, that a child cannot use words to refer without having something to refer to, that is, without a prior organization of the world into objects of possible reference, then one cannot object, obviously. But they seem to be claiming something more, perhaps that the principles that govern the structure and functioning of the language faculty are in fact principles that arise in the course of the development of the child's sensorimotor constructions. If that is the claim, then it seems to me a very curious one, which cannot be maintained on the basis of any current knowledge of the nature of these systems. Perhaps some sense can be made of this claim, but I'm not aware of any formulation of it that has any credibility at all, and I constantly wonder why it is put forth with such dogmatic certainty. It seems to have little prior plausibility, and to my knowledge lacks any empirical support.

QUESTION: Your metaphor of birds just reminded me that Leonardo da Vinci wanted to study the structure of the bird in order to discover the functional dynamics of flying. In the study of the structure of the bird was the key to what flight was, and it seems that this approach is pretty much; the same in general principle as Leonardo's approach, i.e. from the study of structure comes the knowledge of function.

CHOMSKY: That's extremely natural. I can't imagine any other approach. How else could one proceed?

QUESTION: Well, some people feel that to study the other way around perhaps is better. To study function in order to find out what structure is. And, of course, that's what you were attacking when you set out to destroy the house that Skinner built.

CHOMSKY: Well, not really. My criticism of Skinner was not that he was trying to study structure on the basis of function, but rather that, in the Skinnerian system, there are simply no principles. His "theory of language" was almost vacuous. I don't mean to say that his principles of partial reinforcement, for example, are vacuous; they are not. How interesting they are, one might argue, but at least they have content. However, in the work that he's done on so-called higher mental processes -- for example, language -- there are simply no discernible principles at all. When you explore the proposals that he puts forth, they dissolve into metaphor and vacuity. One can see very easily why this should be the case: it's because Skinner departs radically from the framework of the natural sciences in several important ways; specifically, by taking it as a *a priori* principle that you're not allowed to develop abstract theories. As he puts it, you're not allowed to develop theories of internal representation or mental structure, to postulate mental structures, which in this domain simply means you're not allowed to have theories of a non-trivial character. Naturally, anyone who insists on this doctrine -- merely a form of mysticism -- is never going to get anywhere. And, investigating the system as it develops, you find, not unexpectedly, that it simply has no principles that one can put to the test. My criticism has nothing to do with the relationship of structure and function. Skinner put forth no account of either, as far as I can see, but merely developed a terminology which he prefers to traditional "mentalist" terminology, apparently because of highly misleading connotations that vaguely suggest experimental procedures.

QUESTION: What we've been talking about so far has been the verbal signal system. Lets

go on to the nonverbal system. How much of the nonverbal system is helping the verbal system grow in the beginning stages, and once it's gotten formulated, how do they reciprocally influence one another? That is to say, are the verbal and nonverbal signal systems interrelated?

CHOMSKY: Let me stress again that I don't have any doctrine on this matter; the facts are whatever they turn out to be. A second point I ought to stress is that I don't think there is really any serious evidence about this; all we can do for the moment is speculate, beyond certain fairly obvious remarks. There are certain obvious interconnections between the verbal and gestural systems. In fact it's enough to watch somebody talking to notice that -- as I'm talking now -- I'm gesturing all over the place -- anybody who's observing these gestures would notice that they relate in all sorts of ways to the form and content of my utterance. For example, I stress something by a gesture, but even the phrasing -- the intonation structure of the utterance -- corresponds in quite obvious ways to things going on in the gestural system. They're in tandem, and some common source is obviously controlling them both; they're just too well correlated for anything else to be the case.

Nevertheless, the system of gestures is very different in its underlying principles from the system of language. The system of gestures, in fact, seems to have very much the properties of what might be called "verbal gestures," for example, stress or pitch. If you consider the system of intonation in language -- stress and pitch basically -- you can immediately separate out two different components. On the one hand, there is a continuous component; that is, the loudness, the pitch peaks in my utterances can vary in principle over a continuous range, in whatever sense it makes to talk about continuous dimensions in the physical world. The more agitated I become, the more I want to pointedly emphasize something, the greater the stress and the higher the pitch will be at the end, again over a continuous range. So there is a continuous system which looks as though it has very much the properties of nonverbal gesture. If someone were observing me carefully, he might notice that my arms move more when the intonational peaks in my utterances are higher. There might be such a correlation. On the other hand, there is another element in the stress and pitch system that is radically different in character. There are significant respects in which the whole intonational contour of an utterance -- its stress patterns and pitch patterns -- is closely related to the discrete hierarchical phrase structure, and internal word structure for that matter, that reflects the rules of English grammar. In the actual performance of language, these two systems interact. So, for example, the abstract phrase structure of the utterance that I'm now producing determines one of a discrete set of possible abstract pitch and stress patterns. But then some other kind of system interacts and spreads that over a continuous range.

I'm now talking on just the verbal side, and even here we find, I think, quite different systems; one a system which is really as much a part of the discrete grammar of English as is, say, segmental phonology, words, structure, or syntactic phrase structure. Similarly there is a gestural system that shows up in speech as well. For example, it expresses itself in the range of intonation or stress contours that somehow are constructed on the scaffolding that derives from the rules of grammar.

QUESTION: Do you believe there is a grammar of gesture?

CHOMSKY: That's a very different question. I've been talking not about sign language, but about the gestural system that is associated with spoken language. Sign language undoubtedly has a grammar as does spoken language, and in the actual use of sign language, we surely will find the same kind of interaction of a discrete grammatical system and a gestural system that we find in spoken language.

Presumably there is a system, a set of principles, that determines the nature of the gestural system and the way in which it interacts with the language system, but whether those principles should be called a grammar is another question. I would think that it is a dubious metaphor, because it leads one to expect commonality of structure, and that is very much an open question. In fact, it seems to me that there isn't likely to be much structure in common. Even at the most rudimentary level, the systems appear to diverge radically. A system of principles that determines the nature of some continuous system is going to be very different from a system of principles that determines the nature of some discrete system. And as we proceed, I think we will find more and more divergencies. To a certain extent, at least, the gestural system is like a speedometer; perhaps the degree of my commitment to what I am saying is reflected in the extent to which my arm moves while I'm saying it. This is almost like a recording device. There is undoubtedly much more to continuous gesture than that, but there is at least that. Now that's a property that doesn't appear at all in the discrete system of recursive rules that determine the formal structure of language, and that determine what I called the basic scaffolding on which the stress and intonational contours are constructed. It may be, incidentally, that sign language does make use of such properties.

QUESTION: Speaking of stress and rhythm, do you feel that the study of stress contours, etc. has any possibility of getting us closer to the biological basis of the structure of language?

CHOMSKY: I would think that the study of any aspect of language has a possibility of getting us to the biological structure.

QUESTION: Some may offer better bets than others.

CHOMSKY: I think they're just going to lead us to different aspects of the biological structure. For example, the study of abstract syntax or abstract phonology leads to certain aspects of the biological structure of language, that is, to crucial and intrinsic elements of cognition. The study of stress and intonational contours -- as I mentioned, we have to separate the components of those, one of them being very much like abstract syntax and phonology, but the other one, a continuous system which has at least some of the properties of a recording device -- that may tell us something about other aspects of the biological basis for human language, for example, about rhythm and symmetry and properties of serial behavior, the sort of thing that Lashley talked about years ago, all undoubtedly other aspects of our biological nature.

But I would still want to resist what is a very common assumption, and I think one that is totally wrong, namely that the study of the abstract structure of language can't tell us anything about what is sometimes called "psychological reality" or biological nature. On the contrary, it is precisely telling us about psychological reality in the only meaningful sense of that word, and also about our biological nature, namely -- ultimately -- the set of genetically determined principles that provide the basis for the growth and development of these specific capacities.

QUESTION: Why do you think that mistake has been made?

CHOMSKY: I think the mistake has a curious history, and maybe the easiest way to explain would be to talk a little bit about the history. Maybe the first use of the phrase "psychological reality" is in Edward Sapir's paper in, I think, 1933 on the psychological reality of the phoneme, which has become a sort of *locus classicus* for this discussion. (Sapir, 1933). Sapir, in this paper, tried to show that the reactions of his informants, in American Indian languages, provide evidence that the phonemic analyses that he was

proposing for these languages were psychologically real.

QUESTION: Meaning what?

CHOMSKY: What did he mean by that? That's the interesting question. Let's reconstruct what Sapir was doing -- or intimated that he was doing. He was investigating the data of a language -- the phonetic data of the language -- and he proposed a rather abstract phonological structure that he claimed underlies the range of phonetic phenomena that he studied. The empirical justification for the postulated abstract phonological structure was simply that if you assumed it, then you could explain many of the phonetic facts, you could show that the phonetic facts were not just a random array of disorder, but that in fact they reflected some simple principles; there were interesting abstract principles from which a range of phenomena follow. Notice that Sapir did not take that to be an argument for psychological reality. That is, he did not conclude from the fact that he was able to construct an abstract theory of, say, Southern Paiute phonology on the basis of which a variety of facts could be elegantly explained -- he did not take that as a demonstration of psychological reality for the underlying phonological theory. Rather, he clearly felt that, in order to demonstrate psychological reality, he needed some other kind of evidence; for example, evidence that under some conditions his American Indian informant seemed to be hearing something that was not physically present, and other behavior of that sort. Implicit in Sapir's approach was the assumption that there are two kinds of evidence in this field. There is the kind of evidence provided by the phonetic data themselves -- these provide evidence for the correctness of the phonological analysis. And there's another kind of evidence, namely behavioral evidence of some different sort, which is evidence for the psychological reality of that phonological analysis.

As the discussion of psychological reality has proceeded since that time, this assumption has been held constant. I don't want to run through the whole history; but coming right up to the present, the same distinction is quite common. If you look at the latest issue of a journal with an article on psychological reality, you will find almost invariably that the question raised is: what is the evidence for the psychological reality of some linguistic construction? A linguist proposes some principle or structure for English, say, such-and-such a phonological system or condition on syntactic rules, or whatever. Then someone comes along and says, "all right, that's very interesting; but what's the evidence for the psychological reality of the systems and principles that you've postulated?" The evidence is supposed to come from an experiment in which a subject is pushing buttons or something like that. Now again the presupposition is that the data available to us fall into two categories. There are the data that come from experiments and bear on psychological reality; and there are the data provided by, let's say, informant judgments or language use itself which don't bear on psychological reality, but on something else. But this distinction is senseless.

QUESTION: Some people, I think, have raised the question of "psychological reality" on the basis that literature sometimes refers to something as having a psychological reality that was generated by the mind of the writer of the article.

CHOMSKY: That's right.

QUESTION: And only by the mind of the writer of the article.

CHOMSKY: True enough, but that kind of criticism is quite independent of the senseless distinction I have been discussing.

One can do a bad job of constructing theories on the basis of evidence derived from

button-pushing, informant judgment, electrodes in the brain, or whatever. What I would like to suggest is the following, going back to Sapir. He was looking at the phonetic data from a certain American Indian language and was able to show that, if he assumed a certain abstract phonological structure with rules of various kinds, he could account for properties of these data. He could explain some of the facts of the language. That investigation in itself was an investigation of psychological reality in the only meaningful sense of the term. That is, he was showing that if we take his phonological theory to be a theory about the mind -- that is, if we adapt the standard "realist" assumptions of the natural sciences -- then we conclude that in proposing this phonological theory he was saying something about the mental organization of the speakers of the language, namely that their knowledge and use of their language involved certain types of mental representations and not others -- ultimately, certain physical structures and processes and not others differently characterized. That is, he was making a claim about psychological reality, and he had evidence for it. The evidence was that his hypothesis would explain some facts. And that is the only sense in which there ever is evidence to support a truth-claim about reality -- physical or psychological. In fact, the so-called "psychological evidence," the behavioral evidence that Sapir adduced, was arguably weaker than the so-called "linguistic evidence" adduced with regard to the correctness of the postulated abstract theory. But he would not have written the article the other way around, that is, first noting his informant's reactions (the "psychological evidence"), then postulating a phonological theory to account for these reactions, and then appealing to the explanatory power of this phonological theory as evidence for its "psychological reality," that is, its truth.

The same is true if we move to the present. Suppose that a linguist today proposes some abstract principle of grammar, or some constraint on the operation of rules, and suppose he argues for that principle on the basis of a demonstration, which let us assume to be a very convincing demonstration so that we don't run into the question of accuracy -- we'll just look at the logic of the situation; suppose he can give a very convincing demonstration that by assuming that abstract principle, let's say governing the manner of application and the nature of rules, he can explain some very strange phenomena about our explicit and manifest knowledge. The linguist has thereby provided evidence for the psychological reality of that abstract principle in the only sense in which one can provide evidence for the "reality" of a theoretical construction, i.e., for its truth. The objection that you cite, namely, how do you know it's not just the invention of a theorist, can be answered only in one way; by considering how well the theory explains the evidence and how significant the evidence is. To persist with this objection in the face of a convincing explanation of interesting facts, that is, to ask for some other kind of justification, would be simply perverse. To see that, we can transfer the whole discussion over to the physical sciences. Suppose, for example, someone....

QUESTION: I think I understand what you mean, but I would like your reaction to this, because I think you're simply using the word differently. People do say that. I would interpret their use of the word "psychological reality" to mean that it's only real if enough people engage in it, and one person's engaging in it might simply be idiosyncratic, and therefore may be psychologically real to that individual, but not generalizable as a psychologically real principle.

CHOMSKY: I don't believe that this is the way the issue is perceived, but let's take a look at this question: the difference between what is idiosyncratic and what is common to some group. Fine. How do we investigate that. Well, let's keep within the range of what is called, in what seems to me a rather misleading locution, "linguistic evidence." So, let's suppose that I'm investigating the speech of some speaker -- let's say, myself -- and I find that there is a strange array of acceptable and unacceptable utterances. Suppose I'm considering interrogative expressions. I find that some are well-formed (for example, "who do you think

won the game") while others are not (for example, "who did you ask what game won," meaning: "who is the person x such that you asked what game x won"). Suppose now I find that I can explain the array of possible and impossible questions by assuming some abstract principles that constrains the grammar. Then somebody comes along and says, how do you know that's not idiosyncratic. We know how to find out: I look at the next person and see whether he has a comparable array of possible and unacceptable interrogative expressions and a comparable system. Suppose I find that I can explain that person's array of acceptable and unacceptable utterances by the same principle, and so on. Suppose I go and find that the same principle also enters into explanations for other phenomena in this language or other languages. All of this is what is called "linguistic evidence." Let's now assume the usage you suggest. Then the first investigation of one speaker provides evidence for the psychological reality of that abstract principle for that speaker -- that is, evidence supporting the theory incorporating this principle, or in other words, evidence supporting the hypothesis that the theory and the principle are true, for this speaker. The question you raise is whether the result generalizes; notice that it is not a question about psychological reality, rather it is a question about the generality of a certain conclusion about psychological reality.

QUESTION: Most people who have been objecting to the use of that term have been objecting to that meaning.

CHOMSKY: No, that's not correct. I'm sure that's not correct. The argument is not that the results do not generalize. The people who have been raising questions about "the psychological reality of linguistic constructions" would have said that the evidence provided for the first speaker doesn't support a claim of psychological reality for that speaker, and would not matter how extensive and compelling that evidence is; it is somehow "the wrong kind of evidence." The so-called "linguistic evidence" can, in principle, only establish that the principle in question suffices to provide explanations, but somehow does not bear on this mysterious quality of "psychological reality." A demonstration of psychological reality requires evidence about reaction time or something of that sort. That is, it requires what is called "psychological evidence."

QUESTION: But surely psychological evidence would be observing behavior that's common to enough people to make it psychological evidence.

CHOMSKY: I don't see that. We can perfectly well have so-called "psychological evidence" about a particular person. There are two quite different issues here. The first is whether we have a correct theory for the individual in question; the second is whether the correct theory for the individual in question happens to be similar in interesting respects to the correct theory for some other individual. These are different questions.

QUESTION: Individual differences as opposed to generalized differences.

CHOMSKY: Fine. But the whole discussion of psychological reality takes place on a different dimension. It has nothing to do with individual differences and shared group properties. Let me make it concrete. Suppose the subjacency principle to account for a certain informant's judgments about what is and what is not a properly-formed question, as in the examples I just mentioned. Without going into details, this principle holds that mental computations have to be "local" in a well-defined sense, and it does in fact provide an explanation from the phenomena. I haven't shown anything about "psychological reality" for this person; I've only mentioned [it], along with much else, within a certain theory of grammar. Now the standard response would be that which explains what he does. To show "psychological reality," one would have to do an experiment involving reaction time, etc. Suppose I then proceed to show that for the next person I study the same principle of

subjacency accounts for what that person is doing, and for the next person. Suppose the result extends to other phenomena and other languages. The response would still be: you haven't yet given any evidence for "psychological reality;" you've only shown that you have a simple and elegant theory that accounts for a lot of facts -- and who says that nature is simple? In contrast, even the weakest evidence concerning reaction time, etc., is held to bear on "psychological reality." The evidence falls into two different logical categories: some evidence is labeled "for explanatory theories;" other evidence is labeled "for psychological reality." That is the tacit assumption that is pervasive in the literature all the way back to Sapir.

Again I think that one can see what is wrong in the whole debate by transferring it over to the physical sciences, and trying to imagine a comparable situation. Imagine that some astrophysicists have developed a theory about what is happening in the interior of the sun on the basis of observations of light emitted from the solar periphery. Suppose they analyze the light that is emitted and they develop some kind of complicated theory about fusion, and so on, and then suppose someone comes along and says, "Well, that's very interesting, but how do you know you've established "physical reality?" What's your evidence that the structures, entities, processes and principles that you have postulated have the property of physical reality?" What could the scientists respond? They could only say, "We've already given you evidence that justifies our claim concerning physical reality, namely, it is that if we assume these entities, etc., we can explain the properties of the light emitted from the solar periphery." And then suppose the interlocutor says, "Well, that's all very interesting. I agree that you have a simple explanatory theory, but how do you know that what you have assumed is real? Perhaps the light emissions result from the mischievous acts of a Cartesian demon. The physicists could only respond, "we told you what we think is real and why. We'll be glad to search for more evidence, but since your objection does not rest on the inadequacy of evidence that won't help. Furthermore, you have not presented any alternative explanatory theory for consideration." We have an impasse.

In fact such discussions don't take place in the physical sciences. The reason is that certain canons of rationality are assumed, one of them being that a claim to have demonstrated "physical reality" is nothing more than a claim to have developed an intelligible, powerful explanatory theory dealing with some range of significant phenomena. The phenomena that are being explained are what provide the evidence for the correctness, the truth, the "physical reality" if you like, of the constructions of the theory. If we were to adopt these canons of rationality in the human sciences, we would see at once that the whole discussion of "psychological reality" is just off the wall. To the extent that Sapir or anyone has convincing "linguistic evidence" for a theory that postulates some abstract structure or process, to exactly that extent he has provided evidence for the truth of that theory, that is, for the "psychological reality" of its constructs, in the only meaningful sense of the term.

QUESTION: So what you're saying, if I understand you correctly, is that the arguments about psychological reality boil down to one person simply saying that your truth ain't my truth.

CHOMSKY: What it boils down to, I think, is that quite irrational attitudes often prevail within the human sciences. For example, the assumption I have already mentioned that evidence comes labeled in one of two categories. Some come with the label, "I bear on psychological reality" -- namely, studies of reaction time, etc. Other evidence comes with the label, "I only bear on the correctness of theories" -- for example, evidence about the distribution of phonemes, about well-formedness of sentences, etc. It is not a matter of "my truth versus your truth;" rather of rationality versus irrationality. Recall that the issue is not

the quality of the evidence or its relevance to selecting among theories or the depth or explanatory force of the theories. The most insignificant result about reaction times is supposed to bear on "psychological reality" in a way in which even the strongest and most varied "linguistic evidence" in principle cannot. It's as if someone came to the physicist and said, "your evidence about the sun only has to do with light being emitted from the solar periphery, and I don't call that evidence about 'reality.'" For me, evidence about 'reality' is limited to experiments in a laboratory placed inside the sun where you actually observe hydrogen becoming helium, and so on." That's obviously absurd.

What I think is remarkable about our disciplines, right up to the present, is that the basic approach of the natural sciences is so commonly rejected. I believe, frankly, that this is one reason why so much of psychology never gets anywhere: it refuses to accept the canons of rationality that have been standard in the natural sciences for centuries. The *a priori* objection to theoretical constructions that go beyond some arbitrary level of complexity and abstractness is one such example. One might read the whole curious history of behaviorism as a series of variations on this theme. And the debate about psychological reality is another case in point. If someone were to claim, let's say, that he had evidence for the psychological reality of the subadjacency principle, that he could use it to explain such-and-such facts about the form and interpretation of linguistic expressions, the response would not be: "your evidence isn't strong enough." That would be a rational response. Somebody could say, that's interesting, but I don't think the evidence is very strong, and the theory seems rather shallow. That's a rational response, perhaps even the correct response. But that's not the response that you hear. The response is....

QUESTION: When they say it's not strong enough, did they mean that they did agree in principle with your basic method?

CHOMSKY: No, that's not true.

QUESTION: Well perhaps they didn't agree with the way you got there.

CHOMSKY: No, I don't think that's quite it either. What happens, I think, is that experiments involving memory or reaction time, for example, are regarded as providing evidence for "psychological reality," whereas evidence of the so-called "linguistic" type would be regarded as, in principle, providing no evidence at all about psychological reality. So it's not that the linguistic evidence is not too compelling. Rather, it's that it's evidence of the wrong type, and therefore no matter how much more of that sort of evidence you accumulate, the same kind of critique would be given. Now that's just irrational, as soon as one begins to analyze it, the whole long debate makes no sense from the outset.

QUESTION: I would like to get your reaction. to something specific. People have accused you of neglecting the importance of the environment in your notion of the structure of language and the theory of language, and as I recall you have repeatedly denied this.

CHOMSKY: Let me begin by saying something that I hope is uncontroversial. Namely, there is something characteristic of the human species -- there is some species-specific property, some part of the human biological endowment that contributes to the growth of language in the mind. That is, language doesn't grow in a rock or in a bird under comparable conditions of stimulation. That's obvious, I hope. So therefore, there is something about the human mind that plays a role in determining that knowledge of language develops in that mind. A second point that is equally obvious is that the way in which language grows in the mind is going to be affected by the nature of the outside environment; that is, if we are growing up in the United States we'll learn to speak English and if we're growing up in some parts of East Africa, we'll learn to speak Swahili. That's

again obvious. So what's clear is that there is some biological capacity which differentiates us from rocks and birds and apes and so on; It plainly isn't just a sensory capacity, because we can easily translate language into some other sensory modality accessible to birds or apes and the same observation will hold. So there is some mental characteristic, if you like, something about our nature which reflects itself in the structure and growth of a particular mental organ and that constitutes the intrinsic, innate contribution to the growth of language. And there are also environmental factors, which have both a triggering effect and a shaping effect on the growth of this intrinsically determined "mental organ."

It is, incidentally, important to distinguish the triggering and the shaping effect. Certain conditions may be required for a given system to function and develop, even though they do not shape its development; other conditions may determine how the system functions and develops. Consider for example the development of the mammalian visual system. It has been reported that mother-neonate contact is a prerequisite for the development of normal depth perception in sheep, for example. Suppose that this is the case. Then we would conclude that some kind of social interaction has a triggering effect on the growth and functioning of a biologically determined system, but not (at least, not necessarily) that it shapes this growth and function. In contrast, the distribution of horizontal and vertical lines in the visual field appears to shape the growth of the mammalian visual system. It may not be easy to separate out the strands, but the conceptual distinction is important. Plainly, neither mother-neonate contact with its presumed triggering effect or distribution of lines in the visual field with its apparent shaping effect is going to determine that the visual system will be that of a cat and not a rabbit or a bee. But the triggering conditions must be fulfilled for the system to develop or function in a certain way and the shaping conditions will play a role in specifying and articulating that growth and function. Similarly, in the case of language, it may be that certain types of social interaction play a triggering role and there is no doubt that environmental factors play a shaping role.

So there is an intrinsic, genetically determined factor in language growth; the term "universal grammar," as I've already mentioned, is often used for the theory that attempts to characterize one fundamental component of this aspect of the genotype. And there are environmental factors of several sorts that trigger and shape language growth, as the biologically-given capacity grows and matures in the early years of life. The problem is, then, to tease out these distinct contributions. That they both exist is beyond question, at least among rational people. The problem is to separate and identify them (and furthermore, to distinguish triggering and shaping factors, among the environmental factors).

Now turning to your question, it is quite possible that in my own efforts to separate these factors I've tended to slight the environmental factors, and it is, in my opinion, even more likely that I've tended to underestimate the innate endowment, because of an inadequate and superficial understanding of universal grammar. But that is a question of fact -- an interesting and very important question of fact. To show that I have not given enough weight to the environment, one would have to demonstrate that in the particular proposals I've made, where I've tried to deal with certain phenomena in terms of principles of universal grammar, in fact these phenomena should be explained, let's say as a reflection of some environmental factor.

To be concrete, consider again the example we've already discussed briefly, namely, the rule of question-formation in English. To pick standard examples, we know that the interrogative expressions "who do you think will win the game" or "what do you believe that John told Mary that Bill saw" are properly formed in a way in which "who do you think that will win the game" or "who did you ask what game will win" or "who do you believe the claim that John saw" are not. I've tried to explain such facts as these on the basis of

principles of universal grammar, say the subadjacency principle which I've already mentioned. Now someone else might come along and say, no, these are just idiosyncratic properties reflecting environmental factors. You tried to say the "bad" sentences and your mother slapped you on the wrist. Or something like that. That's how you came to make the distinction. Well, there's a factual question here, obviously.

QUESTION: Maybe you just never heard them.

CHOMSKY: Well, the fact that you never heard the sentences you know to be improperly formed doesn't help, because it is also most unlikely that you have heard the ones you know to be properly formed, or anything resembling them. You say many things you've never heard, all the time. For example, it is unlikely that you or I have even heard anybody say, "who did Mary tell Sam that Tom was likely to see." We've never heard that before, and quite possibly never heard an instance of that category sequence before, but we know that that's a well-formed sentence. So the fact that I didn't hear the improper sentence explains nothing, because among the things that I never heard, some of them I recognize as well-formed sentences and give an interpretation to, and others I recognize as not well-formed sentences though often I know perfectly well what meaning they would have, were they properly formed.

All of this takes us back to the most elementary property of language, its discrete infinity, from which we see at once that only a trivial sub-part has ever been heard, and that sub-part we cannot possibly remember. That is, no one can recall whether or not he has heard a particular sentence or sentence type, with trivial exceptions. In order to show that these phenomena reflect something about the environment, one would have to show something about the specific training or something of that sort. Evidence would have to be produced to show that these phenomena are a reflection of the environment. If some such explanation could be produced, if, for example, some account can be produced of the phenomena concerning the rule of question-formation on the basis of environmental factors, I'd certainly want to look at it. What we find, however, is something totally different. Namely, people argue that environmental factors are critical but without offering any account of the facts in question in terms of such alleged factors. And as long as they don't produce any moderately plausible account in terms of presumed environmental factors, all I can say is that they're not holding my attention. It is not very interesting if somebody claims that something is the result of the environment or an act of God or electrical storms in the vicinity, or whatever, if they don't provide some explanatory scheme that can at least be investigated.

QUESTION: What I would like to know is what specifically, would you use to show how the environment does play a role in the acquisition of language.

CHOMSKY: It's easy enough to find a concrete example. The fact that I call this thing a table instead of a sulxan, which I'd say if I'd learned Hebrew, plainly reflects the fact that I grew up in the United States and not in Israel.

QUESTION: Yes, but what about within a particular language itself?

CHOMSKY: Well, there are things which are certainly a reflection of environment. The example I just mentioned, for one, or the fact that the detailed phonetics of my speech happens to be very much like, I'm sure, a small group of people who were around me in my childhood. Mostly my peers rather than my parents. That fact undoubtedly relates to environmental factors in the growth of language.

QUESTION: You might slip in a little Philadelphia accent every once in a while, like I do.

CHOMSKY: All I have to do is listen to myself on a tape recorder to see that it's not so little -- even though I haven't lived there for over twenty-five years. But it seems to be the case that a child will develop the detailed phonetic characteristics of his peers, and that these tend to persist substantially after adolescence. So, for example, the child of immigrant parents will speak like his schoolmates, and will do so to a fantastic degree of fineness of reproduction, far beyond anything required for communicative efficiency or the like. For example, if I had spoken with a slightly different phonetics, nobody would have even noticed it, but the point is there's something about us that makes us mimic to an incredible degree of refinement properties of the phonetic environment in which we live at an early stage of childhood. That's a striking example of the effect of the environment on the development of speech, within a particular language. There are many others, of course, at every level of language structure and use of language.

QUESTION: You know the examples that the anthropologists have used for years about differences between Navaho and English, that Navaho and Hopi have a different structural quality to them that seems to center very much around the verb rather than the noun. Would you consider that to be a function of environment?

CHOMSKY: First I would want to establish the facts. It's only been in the last few years that there have been investigations of Navaho and Hopi, in particular, of a sufficient level of depth for such questions to be seriously raised. In fact there's been a qualitative advance in the nature of linguistic research into Navaho and Hopi, those two cases in particular, because for the first time native Americans for whom these are the native languages have been adequately trained in linguistics, largely by my colleague Ken Hale at MIT, so that they can begin to investigate their languages the way we investigate English. This has led to remarkable advances, I believe, in the level of the research that's being done, so that now perhaps one can begin for the first time to raise the kinds of questions to which people have given all sorts of dubious answers in the past. I'm not convinced that anything of the sort you suggest can as yet be substantiated. True, those languages differ from, say, English in many different respects, and these undoubtedly....

QUESTION: Let's just assume that if you take a Navaho speaker and an American speaker and you translate Navaho into English, but you do it and the American speaker says, "I'm dying," and the Navaho speaker says, "death is taking place with me." The Navahos seem to utter things that exemplify their view of themselves in the world where action is at the center of things rather than nouns.

CHOMSKY: I don't understand what that means. English certainly....

QUESTION: If I say "death is taking place with me" instead of "I am dying," what is the difference between the two statements?

CHOMSKY: Well, if I say "I am dying," dying is not an action anyway. For nobody, neither the Navaho nor us, is dying an action, I would think. And certainly English grammar is crucially based on verb structure and relations of nominal and other categories to verbs and on what have been called "thematic relations" between noun phrases and verbs, and so on. Maybe it will turn out that there is some difference between Navaho and English in this respect, but I'd like to see the evidence before... I'd like to see a coherent question.

QUESTION: Well, the difference between those two utterances: would they primarily be a difference of biology or environment?

CHOMSKY: What differences there may be are obviously environmental. That is, I don't

say the sentence in Navaho, and the Navaho doesn't say the sentence in English, but I assume that there is no relevant distinction in genotype. We obey the same principles of universal grammar.

QUESTION: So there's something in the environment that precipitated this different structure.

CHOMSKY: If there is one. But that's even true at the level of the sounds we produce. The sounds we produce are different, the words are different, their organization is different, and so on. You're raising the question of whether the conceptual structures associated with those utterances are different, and as to that, I simply think that we don't know.

QUESTION: There were two questions there. There was the one you first mentioned, and there was the other, namely, does something in the environment produce the difference that we notice as a difference. Are you saying maybe there really isn't a difference that makes a difference?

CHOMSKY: At the level of conceptual structure? First we have to see if, at the level of conceptual structure, there is a difference. If there is, then it will be because of the environment. What else could it be? I don't think that you and I are genetically different from the Navaho speaker in any relevant respect. So in fact wherever we can find a difference of phonetic or syntactic or conceptual structure, we will naturally assume that it is somehow related to environmental factors.

QUESTION: Wouldn't it be possible that a pure Navaho that was born only out of Navaho stock may be inheriting some kind of structural difference for his language?

CHOMSKY: It's certainly a logical possibility but I don't think anyone takes it very seriously. Of course, it's never been studied in a systematic way, but the evidence we have certainly suggests that, say, if I were to adopt a Navaho child, that child would grow up speaking English as if he were my own child. That is, there is no evidence that I know of for the differentiation of the human species into language types. There are people who argue that: Darlington, for instance, if I remember correctly. But I doubt that anyone takes that very seriously.

QUESTION: It's not a point of view that you would take, or is it?

CHOMSKY: It is conceivable. It wouldn't even terribly surprise me nor would it be particularly interesting as far as I can see. There are other respects in which human beings differ from one another genetically -- height, weight, skin color, hair length, and all sorts of things -- and it's conceivable that they also differ in some marginal respect with regard to the mental organ of language. But if there is such a difference at all, I would assume that it's at such a remote periphery that to investigate it would be completely pointless at the present.

QUESTION: Some people have been disturbed with your use of the word "organ of language." In terms of structure, they feel that it's rather simplistic to say that language is an organ, like the heart or the liver, and that it's a misrepresentation of a very dynamic, complex system.

CHOMSKY: That's a curious argument. Suppose, in fact, that language is, as such critics assume, an extremely complex system -- let's assume for the sake of discussion that the language system is far more complex than, say, the heart or the visual system. We then notice something else: this highly complex system, which we're assuming, say, to be far

beyond other physical systems in complexity, nevertheless develops in an essentially uniform way, across individuals. You and I can converse perfectly well about some topic we've never discussed before, which presumably means that this marvelously intricate system in your brain has developed in more or less the same way that it has developed in my brain. So what we are now considering is the following assumption, or mixture of assumption and fact: (1) that the system of language that develops is very complex, far beyond the physical organs; (2) what is plainly a fact, namely, that it's essentially uniform over a significant range among individuals. Now the conclusion that follows from those assumptions is that the basic properties of the whole system are genetically determined. The structural properties and functions of this system and its interactions with other cognitive structures must be largely intrinsically determined, if in fact systems of remarkable complexity and intricacy develop in an essentially uniform way in an environment that is plainly not articulated and differentiated in anything like sufficient detail to fix these specific properties. That would seem an unavoidable consequence if indeed we assume, with the critics you mention, that the resulting system is one of a very high order of complexity and specific structure. But that is simply to say that we have reached the conclusion that it is quite appropriate to regard the "language faculty" as, in effect, a "mental organ," in the sense that I suggested; that is, to assume that it is genetically determined in considerable and specific detail as one component of the mind, neurally represented in some as yet unknown fashion. There is no other way to account for the high degree of intricate, specific structure and uniformity of growth of the system.

I think it can be a useful corrective for fields like psychology and linguistics to transfer the kinds of questions that they raise over to the domain of the physical sciences, because very often when you do that you see that the questions are badly formulated. I think that this is a case in point. Suppose someone were to come along and say, look, I don't believe that the development of the heart or the circulatory system or the visual system -- I don't believe that any of these things are genetically determined. I think they are learned by the embryo; that is, the embryo tries all sorts of different things and finds that the circulatory system seems to work out best, or perhaps there is some environmental factor that we don't know about yet that reinforces the random experiments of the developing embryo, determining by reinforcement that it develops a heart instead of some other system; that's how the organism develops a heart. And that's why the human embryo grows arms of wings. It's a reflection of the embryological environment. The embryo tries out a lot of possibilities and arms seem to work out better than wings, or something like that. If such a proposal were made, people wouldn't even bother to ridicule it.

Let's take an example from post-natal development; let's take, say, onset of puberty. Suppose someone comes along and says, I think that people learned that, if they don't try to, or try not to reach sexual maturity, then their friends laugh at them and their parents punish them; and if they try to, they get rewarded. It's just a matter of copying other people who have gone through puberty. Again, such suggestions would not even be an object of ridicule. What everybody assumes without even discussing it is that all the things that I've just described are genetically determined.

But let's ask why these suggestions are so ridiculous. That's an interesting question. It's not because we know the answer to the question how pre-natal growth takes place. Nobody knows much about that. Nobody can tell you what in the genes determines the growth of organs or, say, the onset of puberty. Still, it's taken for granted that it is a genetically determined maturational process in all these cases. Why? Well, only because of the high degree of specificity and uniformity of the process or the result of the process -- there's such a qualitative gap between that degree of specificity and uniformity on the one hand and the environmental stimulation on the other that it's inconceivable that these developments are reflecting some property of the environment.

Let's go back now and look at the language case. Notice that on your own assumption the same conclusion holds *a fortiori*, because in fact what is assumed by the critics you cite is that the language system is even more complex than any of the physical organs which are taken to be determined by genetic endowment. And, of course, the development of this immensely complex system is quite uniform among people. So there is a uniform development to an even more complex system, with no apparent possibility, so far as we know, of relating it to environmental factors.

QUESTION: I think that when you talk about the liver and heart, there doesn't seem to be a by-product of the interface between, say, mental and somatic life. You get such things as language, you get a structure and a process which is a by-product of the interface between mental and physical life.

CHOMSKY: But that comes back to my original point. Why should we abandon normal canons of rationality when we turn to the study of the mind? It's certainly true that the study of the mind has to do with different systems than in the conventional study of the body. But the question I'm asking is why should we abandon the approach we take for granted in studying the body when we turn to the study of the mind. What you're saying is that, look, this has to do with the mind, therefore it works differently. But that's not answering the question.

QUESTION: No, I said it has to do with the relationship between the body and the mind.

CHOMSKY: Okay, so why should we abandon normal canons of rationality when we talk about the relationship of the body and mind -- bearing in mind, again, that the study of mind is a study of a very poorly understood physical system, conducted at an appropriate level of abstraction.

QUESTION: I don't think you should.

CHOMSKY: Well, if we don't; then the very same considerations that lead us to take for granted that there is a genetically determined process of maturation in the course of physical organ growth will lead us to assume *a fortiori* that the same is true of mental organ growth. That turns out to be not only a reasonable approach, but also a successful one -- the only successful one, to my knowledge.

QUESTION: But the point I'm trying to make, and I'd like your reaction to, is that obviously the mind can influence the body, and the body can influence the mind. Nobody in his right mind would think that the mind can in its structural development influence the structure of the heart, or the structure of the liver.

CHOMSKY: That's absolutely untrue. Take the study of psycho-somatic medicine.

QUESTION: Well, you're just altering the structure. You are born with the structure of the heart.

CHOMSKY: You're born with the structure of language. I know of no reason to believe that there is any fundamental difference in the respects in which the human embryo has at the earliest stage the potential structure of the heart on the one hand, and the potential structure of language on the other.

QUESTION: But it doesn't unfold in language until the first year of life. You can look at the heart when it comes out. You can see its structure.

CHOMSKY: That's why I gave the example of puberty. There is plenty of post-natal physical development, evidently; in fact there is a lot of neural maturation of the brain that takes place well after birth in humans particularly. Does anybody doubt that the dendritic growth that's going on from ages two to four is genetically determined? Do they think it's a reflection of the environment? In fact, take the study of the maturation that takes place in the visual system after birth. Or take even dramatic cases of genetically determined maturation such as puberty, for instance; or for that matter, death, which takes place long after birth, but is genetically determined. We are determined to be the kind of organism that will die after so many years. Obviously physical growth takes place after birth; nobody thinks it's learned. No one thinks that children are reinforced to grow until age seventeen or thereabouts, and then they're not reinforced any more, so they stop growing. That's absurd. There's no specific moment, say, birth at which qualitatively different things necessarily begin to happen. Many aspects of our physical development take place in a genetically determined fashion well after birth, of course, triggered and shaped in some manner by environmental factors -- as is true of embryological development as well. Onset of puberty, for example, seems to vary with nutritional level over a considerable range, so is conditioned by environmental factors. But does anybody get confused about that and think that we learn to undergo puberty? Of course not. As far as I can see, as far as we have evidence at least....

QUESTION: You learn to cope with it.

CHOMSKY: But my point, to get back, is this. On the very assumption that you proposed -- namely that the language system is far more complex than the obvious physical systems of the body, which may or may not be true -- but if it is true then *a fortiori* you're led to the assumption that this is a case of strongly determined maturation and specific development in a genetically specific direction.

QUESTION: Of what importance is the current research in comparative psycholinguistics (recent attempts to train chimpanzees and/or apes via sign language or any other method)?

CHOMSKY: Investigations that have been carried out so far I think are intriguing. Some of them -- Premack's, for example -- seem quite interesting. They tell us something about chimpanzee intelligence. As far as language is concerned, what this work has so far shown is, I think, about what anybody would have predicted in advance. Namely, as far as we know, even the most rudimentary characteristics of human language are completely beyond the capacities of apes that otherwise share many of the cognitive capacities of humans. At least that's the result of the work so far reported. For example, take the properties that I mentioned before when I was beginning to list the most elementary properties of language, for example, the fact that language involves a discrete infinity of utterances based on recursive rules involving phrases, building more complex phrases by recursive embedding of various structures, and so on. As I mentioned, these are the most superficial and rudimentary properties of human language, and there seems to be nothing even remotely analogous in the systems that are laboriously imposed on apes. That's exactly what we should expect, I think. Why should we expect it? Because, if it turned out, contrary to what has so far been shown, if it turned out that apes really did have something like a capacity for human language, we would be faced with a kind of biological paradox. We would be faced with something analogous to, say, the discovery on a previously unexplored island that there is a species of bird with all the mechanisms for flight that has never thought of flying, until somebody comes along and trains it and says, look, you can fly. That's not impossible, but it's so unlikely that nobody would take the possibility very seriously.

Now, of course, there are capacities that are never realized; for example, take the number capacity. That's a genetically determined capacity, no doubt, but it was never realized in human life until long after human evolution was essentially completed. So that part is not surprising. What would be quite surprising, however, is the following: suppose that an organism has a certain capacity and suppose that circumstances exist in normal life for that capacity to be used. And suppose furthermore that exercise of that capacity would confer enormous selectional advantages. And suppose finally that the capacity is never put to use. That would be a very strange phenomenon. I would be surprised if there were examples of that in natural history or in biological evolution. I think any biologist would be amazed to discover anything of the sort. But that's what people who are working with apes somehow -- a lot of them, not all of them -- seem to believe to be true. And while you can't rule out *a priori*, it seems to me quite a long shot, a very exotic belief, and certainly one for which no evidence has been forthcoming. So I would tend to dismiss it as -- it seems to me... Tom Sebeok once described it as an example of the "pathetic fallacy," the long-standing tendency to invest nature with human properties. I suppose it's another case of that.

It seems to me that this kind of investigation may make perfectly good sense as a technique for learning something about the intellectual capacities of apes, although whether this is the best way of pursuing that question is perhaps open to doubt. One might find much more substantial manifestations of ape intelligence by studying what they do naturally, rather than training them in tasks that are vaguely analogous to the early manifestations of certain human capacities. Just as it would be a questionable research strategy in the study of human intelligence to try to get human children to behave like apes. One might learn something, but it doesn't seem obvious that this is the most reasonable way to approach the problem of investigating the capacities of a particular species. In fact, it's for this reason that it seems to me that Premack's work has been of considerable interest. He's not just trying to make the apes behave as though they're funny-looking people, but rather to investigate their intellectual capacities in a straightforward way. There's nothing wrong with that, in fact, it is a very significant line of research. And it seems to me, to repeat, that in regard to language, what has so far been found and what I anticipate will be found is about what you'd expect, that apes lack the rudiments of anything comparable to human language, at least in any domain in which anything is known about human language and, evidently, the significance of analogies, dubious at best, is essentially nil outside of such domains. Similarly you may get human beings to jump farther and farther, but they're never going to fly.

QUESTION: What are the most important and promising applications of research in the psychology of language and cognition? For example, in therapy, in teaching, etc.

CHOMSKY: My general feeling is that it's practitioners, therapists, teachers and so on who will have to explore these questions. It would be terribly presumptuous of me even to suggest anything. Because I have no experience, I have no particular knowledge about these matters; It would be particularly inappropriate for me to venture off-the-cuff comments or proposals because the questions are not academic but have important human consequences. I have opinions, of course, and sometimes voice them, but they do not derive from any special knowledge that I may have.

QUESTION: Do you feel that the field of language and cognition is, as some believe, in a state of transition searching for a new theory or paradigm? If so, what kind of theory do you believe will emerge or is at present emerging?

CHOMSKY: Well, I'm looking for a new theory too, and I always have been. In fact, I don't

see how anybody can ever do anything different. You mention paradigms. I think when Tom Kuhn was discussing paradigms, he had in mind major scientific revolutions. You know, the Galilean revolution or Einstein or something of that sort. But it seems to me to cheapen, to demean the whole concept to apply it to....

QUESTION: Do you mean to say that you do not think that you have not been involved in major scientific revolution in psychology.

CHOMSKY: Well, to compare it to the revolutions in the natural sciences is quite improper. The kind of work I've been associated with has earlier antecedents, and builds very definitely and explicitly on them. There are differences in point of view but, quite honestly, I don't think that I've suggested anything in the human sciences beyond what I've been stressing here over and over again, namely, let's apply the canons of rationality that are taken for granted in the natural sciences. And when we do, some things will be fairly obvious. Beyond that, I've tried to discover the properties of a particular cognitive system.

QUESTION: If you haven't really revolutionized the ideas, perhaps you've revolutionized the interests.

CHOMSKY: My own feeling is that anything I've done in the study of language or in other fields is hardly more than the application of normal standards of rationality, which have been taken for granted in the natural sciences for centuries, to phenomena in these fields. When you do, some things are immediately obvious. For example, it's immediately obvious that language involves a discrete infinity of constructions, that grammar involves iterative rules of several types. That is where the serious work begins, and I do think that many quite interesting ideas have been developed and explored in the past 30 years or so by pursuing these questions, that is, in the work on generative grammar. But it seems almost transparent that the general approach is a natural one, although it would have been difficult to pursue it without the stimulus of the developments in the theory of formal systems in the past century.

I feel the same way about our discussion of cognitive structures as "mental organs" that is, about a modular rather than uniform theory of the mind, and also about the great significance of innate determinants of mental growth. Again, all of this seems transparent, as soon as you face the questions without prejudice. Or take the questions we discussed concerning "psychological reality." Again, what seems to be a fundamental error undermining the whole debate over this issue is clear enough as soon as we drop certain prejudices. In fact, quite generally, as we're able to peel away certain layers of traditional dogmatism, it seems to me almost obvious what the general mode of proceeding ought to be. I wouldn't regard that as a "paradigm shift." Nor do I think that a lot of the currently fashionable talk about repeated paradigm shifts makes any sense. It's striking in the social sciences.... I've read articles by linguists and psychologists who talk about paradigm shifts that come every two years or so. In physics, they come once in two centuries. This is just nonsense. Of course, we ought to be looking for new theories all the time. The existing theories in these domains are hopelessly inadequate, and therefore we try to improve on them, or construct them on a new basis. If I were to accept now what I myself had proposed twenty years ago, I'd quit the field. That would be enough to show that it's not a worthwhile field to be in.

QUESTION: Twenty years ago you proposed something that had a fundamental impact on the development of both linguistics and psychology. You started a movement which, perhaps might have happened without you, I don't know, but it's hard to believe that behaviorism was going to go out so rapidly as it did without the impact that you had on it. I know it's hard to look at yourself in historical perspective, but it seems to me that you did

have a rather major impact on the shift from a very strongly behavioristically oriented profession to a profession that is very much different at present.

CHOMSKY: I think that behaviorism in any of its variants had essentially run its course. Its accomplishments have to be absorbed in the psychology of the future, but the stranglehold on thought that it imposed had to be broken, and twenty years ago -- to take the moment in time that you mentioned -- this was happening from several points of view. More fundamentally, I feel that it is necessary to disentangle psychology from its antecedents in empiricist learning theory and to approach its problems afresh. If you ask what psychology should be doing, what new theory it should be looking for, my feeling is -- repeating once again -- what it ought to be doing is trying to study the human mind and its growth and its manifestations much as we study any complex problem in the natural sciences. We should try to isolate the specific sub-systems that enter into a very complex interaction in the comprehensive abstract system that we call the mind, and also to find the physical basis for these specific systems, if we can. We should be looking for the principles that govern the structure and functioning of those systems, as well as their interactions, and we should also try to unearth and make explicit the innate properties that determine their growth. That is where the significant theories are going to arise, I would guess. It may be that someone will come up with a radically new way of thinking about these questions, but it is not obvious that one is required, at least with regard to the questions we have been discussing today.

There are many questions that we haven't discussed at all -- for example, questions about the causation of behavior, the exercise of will, choice, and so on. About these questions, I have nothing to say and I know of nothing substantive to repeat that others have put forth. I've tried to make a distinction elsewhere between "problems" and "mysteries" -- the former involving questions that give rise to intelligible and perhaps promising research programs and the latter lying beyond our cognitive grasp, perhaps for contingent historical reasons or perhaps for deeper reasons: we are, after all, biologically given organisms with our particular intellectual scope and limits, not "universal creatures," capable of comprehending anything. The fact that we can construct intelligible scientific theories in some domains presumably results from intrinsic capacities that may very well limit, in principle, the scope of our understanding. Such speculations aside, we have been discussing here what I would like to call "problems", in this sense, but there are other questions that still, and perhaps for us forever, fall into the domain of mysteries, questions of the causation and choice of action among them.

But keeping to questions relating to the structure of cognitive systems and the determinants of their growth, I think there are quite a lot of open questions and some reasonable programs of research designed to study them, in quite a few domains. The particular domain into which I put most of my energies, the structure of language, seems to me to have been a very exciting one just in the last seven or eight years. I don't pretend to speak for any consensus in the field here, in fact, I'm in a very small minority in the field in this respect, but I believe it's been possible in the past few years to develop a theory of languages with a degree of deductive structure that provides a kind of unification and explanatory power going well beyond anything that would have been imagined even a decade ago. Again, I don't think many linguists agree with me about this -- but that's the way it looks to me. Let me stress again, so there is no confusion about it, that with regard to what I just said, I suppose I'm in a very small minority in the field today. But then, that has always been the case. With regard to me, it doesn't seem very different now from what it was ten or twenty years ago. But my own views are not what they were then, and I hope they will not be the same ten years from now. Any person who hopes to be part of an active growing field will take that for granted.

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