to Part II, where Putnam shows that there is more to the "general intelligence" thesis than has emerged heretofore. The second issue, which Chomsky has defined here as "the assumption about the built-in conditions limiting the possible output," takes a new orientation in the next chapter, where a consistent theory of induction, developed by Carl Hempel and Nelson Goodman in terms of a priori *constraints on "projectible predicates," is summarized by Chomsky and Fodor. Explicitly or implicitly, a sort of challenge has been seen in the preceding chapters; in fact, the problem of innate (or at any rate* a priori*) constraints on possible cognitive output has appeared recurrently in this debate. Fodor's presentation in Chapter 6, Chomsky's remarks in the preceding chapters, Bischof's account of inborn working hypotheses, and Sperber's advocacy of innate cognitive dispositions all bear on this issue. In the following chapter Chomsky and Fodor expound in some detail the historical origins of a theory of induction to which they both adhere and the reasons why this philosophical issue has often been so controversial, with metaphysical, ideological, and even political overtones. A whole section of Chomsky's book* Reflections on Language *is devoted to a thorough analysis of the inductivist theories of knowledge.* In what follows, the ideological concomitants of inductivism are examined by Chomsky in response to a challenge made during the discussion by the anthropologist Thomas de Zengotita.*

* N. Chomsky, *Reflections on Language* (New York: Pantheon, 1975).

CHAPTER TWELVE

The Inductivist Fallacy

The discussion reported in this chapter, which has been abridged for the reasons I have explained in the Preface, is the natural sequel to Chapter 6 and the basis of many of the arguments that will keep rebounding between Putnam, Chomsky, and Fodor in Part II.

As a first approximation, I will point out the three major problems that emerge from this discussion. In order of presentation, they are as follows: (1) the nature (and the plausible origins) of the lawful regularities projected a priori by the subject onto sets of relevant empirical data; (2) the failure of "Locke's program" and its impact on modern psychology; and (3) the scientific and extrascientific (religious, ideological, political) rationales for rejecting innatist hypotheses. Since the discursive unfolding of these topics is preceded by only a brief summary—a recapitulation rather than a full presentation—the general reader may benefit from a few preliminary specifications, both historical and circumstantial. The easiest way of access is presumably to be found in the critique of induction, originally framed by Hume and then elaborated by modern philosophers of science such as Karl Popper, Carl G. Hempel, Herbert Feigl, and Nelson Goodman.

Induction is, since Aristotle, the philosophical legitimization of the otherwise innocent procedure through which expectations on future events are dictated by the results of past experience. From the empirically ascertained fact that "All swans observed up to this moment are white," the "reasonable" inference can be made that "All swans (observed or as yet

unobserved) are white." Such an inference, as Chomsky and Fodor keep reminding us, is nondemonstrative; that is, it is held valid only until it is refuted by the next counterexample (the discovery of a black swan). This procedure is totally unproblematic in everyday life and, as Chomsky points out in the discussion, in the experimental sciences. What is instead problematic, and to a high degree at that, is to justify induction purely on logical grounds. Hume was the first to give up such hopes, and by reducing inductive inference merely to a "custom or habit," he provoked, in Bertrand Russell's scathing terms, "the bankruptcy of eighteenth-century reasonableness."* Hume's critique of induction did not impede the practice of induction which, logicians notwithstanding, goes successfully on today as it always has, but rather the quest for rules of induction. In a classic paper published in 1945, Carl Hempel puts the problem back where it belongs: "Generally speaking, such rules would enable us to infer, from a given set of data, that hypothesis or generalization which accounts best for all the particular data in the given set. But this construal of the problem involves a misconception: while the process of invention by which scientific discoveries are made is as a rule psychologically guided and stimulated by antecedent knowledge of specific facts, its results are not logically determined by them; the way in which scientific hypotheses or theories are discovered cannot be mirrored in a set of general rules of inductive inference." This "misconception" (or this fallacy, to use a more current linguistic terminology), as Hempel specifies, leads to a number of crucial, though unwarranted, assumptions: "An adequate rule of induction would have to provide . . . mechanically applicable criteria determining unambiguously, and without any reliance on the inventiveness or additional scientific knowledge of its user, all those new abstract concepts which need to be created for the formulation of the theory that will account for the given evidence. Clearly, this requirement cannot be satisfied by any set of rules, however ingeniously devised; there can be no general rules of induction in the above sense; the demand for them rests on a confusion of logical and psychological issues."†

Therefore, the proper place for an "inductive logic" is, so to

* B. Russell, *History of Western Philosophy* (London: Allen and Unwin, 1961), p. 645.

† Carl G. Hempel, "Science and the Logic of Confirmation," *Mind* 54:1–26, 97–121, 1945; reprinted in C. G. Hempel, *Aspects of Scientific Explanation* (New York: Free Press, Collier-Macmillan, 1965), pp. 5, 6.

speak, sandwiched between the psychology of scientific creativity and the establishment of general "rules of acceptance" for (sufficiently) confirmed hypotheses. These rules, "roughly speaking, would state how well a given hypothesis has to be confirmed by the accepted observation reports to be scientifically acceptable itself." Whether these rules can be construed on "pragmatic" or on "purely logical" grounds is considered by Hempel "an open question."

Returning now to our debate, it can be said that much of what is being discussed in this chapter, as well as in Chapter 6 and in Part II, precisely concerns the psychology of scientific inventiveness. The processes implicit in Fodor's "language of thought" and Piaget's "psychogenesis of knowledge" are just those processes that Hempel wants to differentiate from inductive logic proper once and for all. Since the issue has often been confused and misconceived, it is not surprising that an entire section of this debate is devoted to the inductivist fallacy. It will be seen that fundamental disagreements subsist between Chomsky and Fodor on one side and Papert and Bateson on the other concerning such a neat partition between psychology, inductive logic, and pragmatic rules of acceptance.

The statement of the "paradox," which opens the discussion, represents a particular aspect of the inductivist fallacy: it concerns the a priori definition of what is "relevant evidence" with respect to a given hypothesis, implying also the a priori availability to the subject of criteria for selecting that hypothesis as opposed to the (infinite) others that are equally compatible with the data. Hempel's formulation is as follows: "In the discussion of scientific method, the concept of relevant evidence plays an important part. And while certain inductivist accounts of scientific procedure seem to assume that relevant evidence, or relevant data, can be collected in the context of an enquiry prior to the formulation of any hypothesis, it should be clear upon brief reflection that relevance is a relative concept; experiential data can be said to be relevant or irrelevant only with respect to a given hypothesis; and it is the hypothesis which determines what kind of data or evidence are relevant for it. Indeed, an empirical finding is relevant for a hypothesis if and only if it constitutes either favorable or unfavorable evidence for it; in other words, if it either confirms or disconfirms the hypothesis. Thus, a precise definition of relevance presupposes an analysis of confirmation and disconfirmation."*

* Ibid. (Emphasis added.)

In the course of the discussion, this evident fact is constantly referred to as a "tautology." In Part II, Putnam contests the legitimacy of such a label, to which Chomsky replies that "tautology" is not to be taken here in its strict sense of "circular definition," but rather in the loose colloquial meaning of "evident truth" (as Hempel says, "it should be clear upon brief reflection"). The tautology in question is, to put it as simply as possible, that no knowledge is possible unless concepts and hypotheses are already "in the mind" before *anything is observed at all. Granting this, it appears equally straightforward to postulate that the* source *of concepts and hypotheses is innate. Chomsky and Fodor assume this further step to be so self-evident that it is not even worth a discussion. They also state that the reason why so many thinkers, including Nelson Goodman and Willard Quine, have avoided this inevitable conclusion is to be found in the realm of ideology rather than in that of pure philosophy. The last part of this chapter, therefore, deals with the ideological shortcomings of innatist and environmentalist hypotheses.*

The acceptance of the "tautology," according to Fodor, cannot alone display the full power of an alternative program in psychology and linguistics unless it is reinforced, on independent grounds, by the acknowledgment that Locke's program has failed. In Fodor's words, "The notion that most human concepts are decomposable into a small stock of simple concepts—let's say, a truth function of sensory primitives—has been exploded by two centuries of philosophical and psychological investigation. In my opinion, the failure of the empiricist program of reduction is probably the most important result of those two hundred years in the area of cognition."

Locke's program was founded on a negative heuristic rejecting all notion of "innate principles in the mind" and on a positive heuristic laying the foundations of all human knowledge only on "sensation" and "reflection." To quote him: "All those sublime thoughts, which tower above the clouds, and reach as high as heaven itself, take their rise and footing here: in all that great extent wherein the mind wanders, in those remote speculations it may seem to be elevated with, it stirs not one jot beyond those ideas *which* sense *or* reflection *have offered for its contemplation."*[*]

Locke can be considered as the founder of the "order-from-

[*] John Locke, *An Essay Concerning Human Understanding* (New York: Dutton, 1971), vol. 1, book II, chap. 1, sec. 24, p. 89.

noise" program (see the Introduction) in the domain of psychology. The present chapter constitutes, therefore, a further example of how alternative research programs, once they have set forth their basic presuppositions, confront both their negative and their positive heuristics. However, it would be premature to formulate a conclusive judgment on these matters before reading Part II, where several crucial arguments of heuristic importance are developed. In a sense, the present discussion is still only a general introduction.

Statement of the Paradox

Noam Chomsky and Jerry Fodor

Chomsky In the history of modern philosophy there is a vast literature dealing with a few very simple points about the impossibility of induction, like the whole debate about the Goodman paradox.[1] Once you understand the paradox, it is obvious that *you have to have a set of prejudices in advance for induction to take place;* but that doesn't change the fact that there is a vast literature in which people have tried to show that is not true.

Fodor Let me state briefly how Hempel described the paradox about thirty years ago, since it is a very easy way of seeing it.[2] He described it in terms of induction, but if you think about it, it is exactly the same for learning. Hempel says: suppose you have a set of observations on the values of two variables across some range of circumstances. These observations can be represented as a set of points in a two-dimensional Cartesian coordinate system. Once plotted, the points can be fit by a curve that represents an induction about the hypothetical relationship between the two variables. The problem of induction, in this situation, is the problem of deciding which curve to choose. This choice is underdetermined by the data. For any finite set of data points, there will be an infinite set of curves, all of which fit the data equally well. Imagine, for example, a set of points arrayed in a perfectly straight line. In this case, one would surely want to choose a linear curve to represent the relationship. But the data do not force this choice. For in addition to the linear solution, there is an infinite set of sinusoidal curves, each of which fits the points

as well as the straight line. These sine waves vary freely in amplitude but have a fixed period, chosen so that every one travels through each of the observation points. Given this set of alternatives, we are obviously in need of some principle that will select the straight line and block the sine waves.

What does it mean to say that we have to block them? It means that *before* we look at the data we have to have the possible curves ordered in terms of preference, so that given just these data we choose the straight line, given slightly anomalous data we choose the closest approximation to a straight line, and so on. You can call that simplicity, or an a priori ordering of the functions, or nativism. This leads to the same point as before: *you can't carry out an induction, it is a logical impossibility to make a nondemonstrative inference without having an a priori ordering of hypotheses.* This general point about nativism is so self-evident that it is superfluous to discuss it; the only question is, how specific are the innate constraints?

Chomsky The specific question concerning predicates was a variant of the one pointed out by Nelson Goodman. His particular interpretation of it seemed very paradoxical to many people and gave rise to a huge debate. What it comes down to essentially is this: Suppose you want to carry out the most trivial sort of induction from "I have seen so many emeralds in a sequence that are green" to "All emeralds are green," a most elementary case of induction. He points out that there could be a predicate, which he calls "grue," which has for us the following meaning (but which to a person who speaks that language just means "grue"): "looked at before this precise instant, whatever today's date is, and green" or "looked at after this instant, whatever today's date is, and blue." The predicate "grue" has been verified 100 percent so far: every emerald looked at is green, and a person who speaks the language in which "grue" is the simplest predicate will simply assume that the next emerald looked at is "grue," that is, to us, green. Therefore, depending on which predicate you pick, you can predict that the next emerald is either green or blue in our language, and in the other language either "grue" or "bleen." You can escape this particular example by imposing some restriction that tells you to choose "green" rather than "grue" (one can think of millions of such restrictions), but then you have a kind of algorithm for immediately creating the next counterexample. The net effect of the infinite series of counterexamples you can develop by this algorithm is

that *you must in some sense have the whole set of predicates from the beginning.* This is an elementary fact, but that doesn't change the fact that there is a vast literature, which is of very little interest (or none at all) to scientists, but which has an enormous interest for those concerned with the nature of induction.

I think that many other theories exist, but they are not theories about the question we are discussing. For example, let's assume some developmental theory of human learning which says that when a child is 3 years old he can do this class of things, and when he is 5 years old he can do some other class of things; I think that this is a perfectly substantive, fascinating, and important theory, but it is not a theory about the topic we are discussing.

I think Fodor was right when he said that the whole question of nativism is beside the point, in that the general thesis is so obviously true that it is even not worth discussing. The question that we ought to ask is, what are the innate factors that make things happen the way they do? One of these innate factors may be that there exists, for some reason that we would like very much to understand, a maturational process that brings it about that when a child has reached age 3, something in his brain has happened that enables him to do certain things. Perhaps it is even true, for reasons built into the genes, that having reached that stage, he will only get to the next stage if certain complex phenomena appear in his environment. This theory is possible, but it has nothing to do with nativism because it is just going to add to the mass of material that we are going to attribute to the genes, since that is the only place where there is going to be an explanation. In this sense, I think that the whole issue of innatism is really beside the point, and the only reason anybody discusses it is, to my view, because of the three or four centuries of illusion that has been maintained about the issue.*

Discussion

De Zengotita Could one say that if nativism is beside the point, that depends in the final analysis on the evidence of this tautology?

* Editor's note: Further clarifications on the inductivist fallacy are to be found in Part II.

Chomsky What is important is not just to see that something is a "tautology," but also *to see its import.* The import of the "tautology" is that there isn't any point in looking for a general theory of learning. What one should do is to look at the complexities of the system of development and the relationship with the environment, the complexity of the environment, and so on. I think that what Papert was talking about is very important (see Chapter 11): I think he is right when he says that just to present evidence isn't enough; you can sit a child in front of a television set, for example, and run well-formed sentences in front of him, and I'm sure he is not going to learn a thing. So that means that simply having the evidence presented to you is not enough for learning. Suppose that I tried to teach something to Premack's chimpanzee, Sarah; I'm sure that I could give her all the evidence—an apple, a triangle—and she wouldn't learn anything because I am not doing whatever is necessary to get the system to function. I don't know how to do this; he does. We must discover what is necessary to get the system to function; then, if we are interested in going a step further, we will ask what there is in that organism (obviously genetically determined, because there is nothing else) that brings it about that in presenting the evidence as I did, I didn't do what was necessary to make the system function.

The controversy about nativism is not worth pursuing because as soon as we understand anything, we know where to make the next move. We will then make contact with the biologists, and that is especially important because they must want to know what it is that they are supposed to explain. Consequently, what we can do is to state some conditions concerning what they will have to explain.

Bateson From the fact of the tautology, as I understand it, you should know the next step: for instance, the chimpanzee has learned to put the eyeglasses on the green; the next step is to know all the multitude of things that the chimpanzee learned from the fact of putting the glasses on the green, and this is now no longer innate but is part of the context of the next learning, and we don't know how these things fit together.

Chomsky Right, but suppose now we went ahead and found out what was involved in his making the next step, and then we ask, what is the origin of that thing X that is involved in it? You answer, trivially, that it is genetic determination, and you keep doing it at every point where you get an answer, because there aren't any other possibilities.

Premack I simply want to say that in my opinion, the real issue has to do with the elements—not with their origin, but with the nature of the elements which, when properly combined, account for acts of intelligence of all different kinds: judgments of synonymy, the distinction between synthetic and analytical sentences, the ability to recognize representations of oneself, and so forth. Of what psychological material are these acts composed? When do they emerge? What are the experiential factors that affect them? To look for their origin is a waste of time.

Chomsky I agree with Premack to a certain extent. The substantive scientific question for most of us, at least for me, is to answer precisely the question he raised. As I understand it, his question is, what are the universals of language? What are the necessary properties of language that hold, not only by accident, by virtue of the fact that the existing languages are what they are? That is a hard question to answer, but it is possible to proceed to answer it. At that point a move is made which I don't think has any alternatives to it, and which I hardly even regard as an inference. If I assert that certain properties are the necessary properties of language, it follows at once, if you accept what we have been calling "the tautology," and its consequences, that these properties are rooted in the genes. Unless you are a mystic, you can't believe otherwise. Then the next question arises: are we going to try to give an explanation for that? This depends on the state of biology; I would love to have an explanation, and I suppose that biologists would just as much. They would like to know what their investigation of neuronal networks is supposed to be doing. To make biology capable of taking the next step, it is necessary to describe these properties of the organism, and here I think that there is not and cannot be any controversy, any debate, between the biologists, the linguists, and the developmental psychologists once they understand the nature of what they are doing. I don't at all deny the importance of what they are doing; I simply think that it has often been totally misdescribed, and once their work is described properly it can then contribute to the joint enterprise just like all other research.

Bateson As I understand it, we are trying to account for a given component of the description of an organism, whether

it is a behavioral component or the color of the skin. Let's say, for example, that my skin is of a certain color. Now, is this trait determined genetically or not? And how is the question going to be handled? We all know that the color of that skin will change if it is exposed to the sun. We now have another descriptive statement about the organism: that this change happens. And this descriptive statement is to be related to the genome or to the environment. We observe the fact that *Homo sapiens* apparently has the genetic capacity to become brown in the sun. We now ask the next question: can *Homo sapiens learn* to get brown in the sun? This is conceivable. One might practice tanning in the sun. Suppose that you get an answer to the metaquestion; what we are dealing with is the problem of logical levels. It is not a question of nativism versus nonnativism; it is a question of *what is the logical level where contribution of the genotype is located?* The genotype could furnish the capacity to learn something or the capacity to learn about learning something. Whatever the level, a component of the phenomenon will always lead us as far as the genotype.

Chomsky What Bateson says is entirely true, but it is really irrelevant to what I just said. Premack, I, and others, I hope, agree that the first step is to try to find out *what is essential to a given organism*. Our first guess may be wrong: we may look at everybody around the table and think that what is essential to this organism is to have white skin. Then someone could object and point out that if a person stays out in the sun for two or three hours, he won't have white skin any more. Then we say that white skin was not essential, and so our first idea was wrong. What is essential to the organism is that under one condition it has a white skin and under another condition it has a red skin. We will consider this to be essential in the organism, and then we will see if it is right or wrong—wrong in this case for other obvious reasons. We then do more experiments, and finally we find something that we tentatively think is essential to the organism. All of Bateson's questions had to do with the first step, but once this first step is made we no longer ask the question of origin because there are no alternatives, unless we are mystics; the only possibility is genetics, and that is why no one bothers to ask these other questions. It is possible that we are wrong, that we didn't do enough experiments, but that simply means that we are doing science and not mathematics. It is always

possible to commit an error; we know that perfectly well. Let's stop talking about it. It is part of the nature of research that our assumptions may be proved false, but nevertheless, the logic of the inquiry is clear. Once we have satisfied ourselves to a degree X that a certain characteristic is essential to the organism, we no longer ask about its origin, because we know the general answer to that question. That's why nobody asks: Do people learn to have a heart? It could be true, but nobody asks the question because they are satisfied that this is essential to the organism.

Papert It is relevant to recall my example of the genome of the perceptron (see Chapter 3). This determines in the most direct sense that the decision-making process be limited to linear combinations of local properties. But what about its ability to count? Do we expect to find descriptions of number in the design plans for the machine, or in its genetic code if it were a living creature? Surely not. At any rate, you'll find nothing there that directly resembles number. And investigators (in the hypothetical situation of an empirical science of living perceptrons) could get into some very sterile disputes and fruitless research if they thought they had to find a representation of number or a mechanism for generating hypotheses about number. In fact, the perceptron's ability in numerosity is an emergent one, emergent from quite different properties. It really is an amazing fact, and it required a great mathematician like Euler to discover that numbers could be computed by making local observations on blobs without having any idea of whether two nonadjacent observations concerned the same blob. The underlying fact is that the algebraic sum of all the curvatures along the circumference of a blob, whatever its shape, must be 2π. So if one finds 6π worth of curvature in a world made up of blobs (that is, surfaces of arbitrary shape but with no holes), we can be sure that there are three objects. This is remarkably counterintuitive, but nevertheless true.

Chomsky What you have to do is to say that there is a certain property, whatever property it might be, that this device can be demonstrated to have. Having done that, we can now go back to the central issue, which is very clear in this case. You are not dealing with the question that we have been discussing. We are discussing the question of how we go from observing what an organism does to discovering its essential properties and then ultimately making an inference about

its genes. You are considering a totally different question, a case where in effect you know the genes; that's given in advance. You know the initial structure of the device. Given this knowledge, you can then through deep mathematical theorems say something about . . .

Papert That's not the point. Suppose we don't know anything about the genes and can only observe the organism's behavior. Someone can then add up the genes and do the experiment with numbers and say: it must have some more genes, go and look for them, because it must have a gene for number. He has based his contention that it must have a gene for number on (1) the observation that the machine can recognize numbers and (2) the fact that he cannot conceive of a process (nor could anyone, before Euler) by which this ability could derive from other, already known genetic properties. So he postulates extra genes and sends the biologist down the garden path looking for nonexistent things.

Chomsky That is relevant, but your analysis now fails for another reason. It just illustrates the truism that I emphasized before, namely, that nondemonstrative arguments may turn out to be wrong. It is certainly true that a nondemonstrative argument may turn out to be wrong; that is in the nature of nondemonstrative reasoning, by definition. If we conclude by scientific inference that this organism must have the following 83 genes (because that is the only way we have of explaining the fact that it does so and so) somebody else may come along, since the argument is nondemonstrative, and say: This is all well and good, but I can show exactly how the same result can be obtained with only 32 of those 83 genes. We can never be sure that this is not true because we are engaged in nondemonstrative inference . . . I don't understand why these trivialities are part of the discussion.

Bateson Because it is in the nature of that tautology.

Papert Let's discuss it anyway, since you admit that most of the world hasn't accepted your tautology.

Chomsky Isn't it true that nondemonstrative inference may lead to incorrect conclusions?

Papert Of course . . . I would like to ask Fodor a question. He says that you can't find a predicate unless it is already there. In this case, was the predicate there? Was it there or not?

Fodor I wouldn't have noticed it, unless I were as clever a mathematician as Euler.

Chomsky That doesn't change the fact that it was there.

Fodor Given a hypothesis about the innate structure of the organism, it is clear that there will be quantities of interesting questions about what a device with this structure could do; and these are fundamentally mathematical questions. If you have a mathematical proof, for example, that a certain device couldn't do a certain thing, and elsewhere an experimental proof that a certain organism *can* do that thing, that is a very important result, because we can now limit the possible models for the organism. Everybody has understood this, at least since Locke—including Locke. He had a rather powerful conception of what the innate structure of an organism was: it was a combinatorial algebra of a fundamentally Boolean kind plus a sensorium. We now have a whole mass of results, which are not formal but are nevertheless highly persuasive and which suggest that an organism with *that* sort of innate structure could not acquire such workaday concepts as *buy and sell, automobile, typewriter,* and so on. But we *do* have those concepts, which suggests that our innate endowment can't be what Locke supposed it was. That is what scientific investigation in this area is like. You make hypotheses about what the expressive capacities of the device are; you ask whether a device with these expressive capacities could do such and such a thing; you compare that device with an organism by experimental procedures. That is what scientific method is like in this area.

Papert I have a comment to make about Fodor's presentation and about his book, *The Language of Thought,* which spells out his position in more detail.[1] Consider the set of predicates that I know at this moment; now suppose you see me learn what seems to be a new one. Fodor wants to deny its newness; he wants to say that it was already there, in a certain sense. But there are two senses, and I think that Fodor shifts between them. One sense says literally that the predicate was already there; the other sense simply allows the new predicate to be defined in terms of old ones. But in the second case, we are satisfied with saying that a predicate cannot come out of nothing.

Fodor The argument doesn't say that; the claim about indefinability is made on *independent grounds,* which lead back to the failure of Locke's project.

Chomsky Fodor is not denying that the set of available predicates may have interconnections that allow some of them to

be defined in terms of the others. That's a matter of discovering the structure of the set of predicates.

Papert A real developmental psychologist, or somebody who really wants to make a machine that functions well, must recognize the tautology that you can't start from nothing. But it makes all the difference in the world whether we start with a small number of elements or whether we have to assume some very complicated things like the specified subject condition and the existence of number. That is really the issue between you and Piaget: not whether something has to be there from the beginning, but rather *how much* and what kind of something.

Chomsky Exactly, but as I said before, that is a noble enterprise and I would like to see some results from it, but it is not going to bear on the general issue of nativism.

Papert That is still a mystery to me, like the question about why everybody doesn't accept Chomsky's tautology. I would like to ask Fodor why he wrote a book that was so difficult that I had to spend many hours deciphering it, if all he wanted to say was the tautology that you can't build from nothing?

Fodor I wrote it for three reasons. The first is that despite this triviality, at least the triviality that Papert mentioned, it seems to me that developmental literature includes doctrines that are taken very seriously (including notably Piaget's views) which fly in the face of this tautology. Piaget's system, as far as I can see, requires a passage between stages that couldn't occur: the development of structurally richer logics out of poorer ones. The second reason is that the auxiliary hypothesis that is required to construct a serious argument (as opposed to just a tautology) is the failure of Locke's program. I consider that the notion that most human concepts are decomposable into a small stock of simple concepts—let's say, a truth function of sensory primitives—has been exploded by two centuries of philosophical and psychological investigation. In my opinion, the failure of the empiricist program of reduction is probably *the most important result of those two hundred years in the area of cognition*. The third point is that the argument cited by Papert is not the one I gave. The argument I gave was this: models of learning have invariably been *models of hypothesis formation and confirmation*. The implications of this are much stronger, as I keep saying, than the *potential* availability of a stock of predicates. It presupposes the actual *exploitation* of the defining predicate as part

of the learning procedure, as I tried to demonstrate earlier (see Chapter 6). So the reason that I pursued my line of argument, despite the fact that I find it embarrassingly trivial, in the way Papert puts it, is that (1) I didn't put it the way he puts it; I presented something much stronger, and (2) it seems to me to have ramifications of a very great importance, so great, as a matter of fact, that when you put these three reasons together, I am inclined to think that the argument has to be wrong, that *a nativism pushed to that point becomes unsupportable, that something important must have been left aside*. What I think it shows is really not so much an a priori argument for nativism as that *there must be some notion of learning that is so incredibly different from the one we have imagined* that we don't even know what it would be like as things now stand. This was, by the way, what I said at the beginning of my talk.

Bateson May I point out to Fodor a problem that has intrigued me all during this debate? We imagine an initial state S_0 which is essentially a stock of predicates, propositions, injunctions, and so forth. The thing that I don't know from the way people have been talking is whether that stock is imagined to be essentially a tautologically coherent body so that the premises agree with each other to make a structure of some kind, or whether it contains incoherences, inconsistencies, perhaps contradictions, and so on.

Fodor What I do know is this: on the one hand, the elementary states have to be the sort of things that can be true or false, because they are going to form the premises of inferences. On the other hand, they have to have the capacity to generate an infinite set of hypotheses, because there is an infinite number of things one can learn. That, plus a general view that Kant was somehow right, is about all I know and all I even suspect about the general structure of this device.

I think, moreover, that methodologically speaking, to put things as ambitiously as Kant did—"Let's get a general picture of this device straight off"—is probably hopeless. I think the best procedure is exactly the reverse strategy: *let's find a small domain that has as little rapport as possible with the rest and try to study it as a sort of protomodel of what is going on*. After all, this device is quite probably the most complicated system in the universe, and it seems to me that anybody who asks himself what the general structure of this

system is like is just doomed to fail. It is possible that certain aspects of this system can be isolated, that is, that they are specific to, say, language. Perhaps the examination of very restricted properties of that form of behavior will lead us to ask what the set of axioms must look like for *that* case, and assuming that we have a model for that case, perhaps we can go on to other things. The reason that I think it is so important for Chomsky to be right about the innate specificity of language, or at least about the innate specificity of *some* interesting psychological subdomain, is that if there is nothing that can be studied in a truly independent way, then I doubt that we are going to find anything out; the problem will become too difficult.

Chomsky Just one brief remark: Why has the illusion of the existence of other possibilities been harbored so long, and why is it still harbored? I don't know the correct answer, but I want to point out one possible answer that has to be obvious, at least to anybody who knows any mathematics. A mathematician can have some very obvious assumptions, but nevertheless the consequences of those assumptions can be very hard to discover; that is what mathematics is all about. I think that in a sense we have a situation which is not unlike that, even though we are not dealing with mathematics. There are assumptions which everybody will agree to when they look at them. But along the way, if inference leads to some result they don't like, they back out, which I think is unfortunate. Sometimes consequences of assumptions that we ought to accept are not easy to accept (there is a psychological compulsion to withdraw), but that doesn't mean that we shouldn't accept them.

De Zengotita Can you describe what you would consider to be a psychological compulsion?

Chomsky I think there is a long tradition, which certainly goes back to scholasticism and then becomes intermingled with empiricist currents, that would have us believe that the human mind is empty, a *tabula rasa*. Nobody will agree explicitly with that any more, but some people find it hard to stray very far from that assumption. Secondly—and this is pure speculation on my part, I have no evidence whatsoever—the belief that the human mind is empty provides a justification for all sorts of authoritarian systems. If the human mind is empty, any method of molding minds is legitimate, and you see this worked out in extreme forms, as with Skinner, for example.

This can end up in a kind of design for fascism originating from the assumption that after all, the human mind is empty . . . so that we, the good architects, will fix it up so that the environment is right and everybody will live happily ever after. Intellectuals are often prone to this sort of thing, because they see themselves as the ones who are going to do the molding of minds.

Again speculating, I will mention a very curious inversion of ancient religious beliefs: it is a very ancient religious doctrine that the human soul is out of bounds; you cannot investigate the human soul, that's the domain of the priest. Science may deal with other things, but not the human soul. Why people hold that view, I don't know, but some certainly hold it very strongly. In my view a very curious thing happened around the seventeenth or eighteenth century, namely, that under the pretext of trying to overcome that belief, people found a new way of formulating it, which was to say: we are not going to study the human soul by the methods of the natural sciences, we are going to study it by purely a priori methods, that is, we are just going to insist that the human mind is empty, or that it has this or that set of properties, and then we are going to investigate these properties. This is the case with associationist psychology, for example, which doesn't have even the appearance of a science. A scientific approach will say: here is what the mind achieves; here is what I will have to postulate to explain that. Associationist psychologists took the opposite approach. They say: here is what the mind is, and I am going to investigate the consequences of that assumption *ad infinitum*, without determining whether whatever I find actually resembles what the mind achieves. You can pursue a study like that forever; you can always find more consequences of those inadequate assumptions, and nothing stops you from going on indefinitely. What is interesting is that this was presented as if it were a scientific approach, but of course, it is precisely the antithesis of science. It is a purely dogmatic approach that starts with a priori, unchallengeable assumptions about the nature of the mind and then goes on to draw out consequences from these assumptions, never paying any attention to the question of whether the assumptions happen to be the right ones. In my view, this is a kind of inversion—and I don't know of any other historical examples—of traditional religious dogma disguised in the name of science. People no longer say that the soul cannot be studied; rather they say

implicitly that it cannot be studied by the methods of natural science. I think that there must be very powerful reasons why people find it extremely difficult to accept the conclusions that follow from very simple observations. There are some very striking examples. Take, for instance, the "grue" paradox that I mentioned; the person who devised it and developed it in a classic book was Nelson Goodman, who is perhaps the most extreme environmentalist who ever lived. He seems to think that everything is learned; the way I read what he says is that any attribution of innate structure to the mind is false—there cannot be such an innate structure. If I interpret his views correctly, he believes that if you were to visit a primitive tribe and to present one group with photographs of people and another group with reproductions of Kandinsky abstractions, then the training period to get them to recognize that the photographs are photographs of people and the Kandinsky abstractions are representations of people would be approximately the same, because there is no built-in system of representation that determines that photographs have some special relation to the things photographed. He has argued things of that sort. Now, it is striking from the point of view of the sociology of science that the very person who gave fundamental arguments that demolish this view should nevertheless uphold it in a certain sense. Another important example is Quine, who is probably the leading philosopher in America, if not in the world. He has taken views on this subject at opposite extremes almost in successive paragraphs. He says at some points that there cannot be anything beyond conditioning, though he agrees with Fodor's basic point that you have to have a quality space fixed for conditioning to take place. He has sometimes said that all that is learned is conditioned, and at other times he has said that it is inconceivable that what you know is the result of conditioning. I think he just contradicts himself. I think one ought to explain all of this by the indirect arguments I suggested; I don't see any other rational explanation.

Papert It is certain that there are powerful irrational motivations for both sides of the nativist controversy. Intellectuals have a vested interest in believing in the existence of natively superior brain power, so they are subject to powerful forces opposing the idea that their minds are constructed. If we think we are intelligent, we prefer to think that we are intelligent by nature; we prefer to believe that the intelligence

is a property of our essential selves. Thus, elitism and associated authoritarian and fascist positions are drawn to support nativism. On the other hand, as Chomsky says, fascists have an interest in believing that the masses are pliable, and not at all determined by their genes; thus the political aspects of the controversy are no less confused, and no more explanatory, than the scientific arguments. Moreover, Chomsky's theory is only plausible if one uses a very particular criterion of rigor which is highly demanding for the justification of all statements except the assertion of innateness.

Chomsky I agree with what Papert just said, but I would like to point out that there is a fundamental asymmetry. The question was asked, *Why, in the face of overwhelming evidence to the contrary, do people still maintain a certain position?* And I suggested some answers. Papert, on the other hand, is asking why, given overwhelming evidence *in favor* of their position, people maintain a certain position. And there may even be irrational answers to this question.

Papert But what is "overwhelming evidence" for what is already in the eye of the observer? It is perfectly symmetrical as I see it.

Chomsky Then you have to tell me where the argument from the "tautology" to the conclusion breaks down.

Papert From the tautology to which conclusion? From the tautology to the conclusion that there must be *something* innate, that there must be *some* structure that determines, nobody would deny. But the existence of those *particular* things depends at best on whether or not one accepts what I call the Chomsky principle 17 . . .

Chomsky The argument that *specific* things have to be attributed to the mind is of course nondemonstrative, and therefore it would be totally unfair for me to ask you where the argument breaks down. Of course, we all know where it breaks down; it is not a deductive argument. But I will ask you another question: in the face of the fact that this is the only theory in existence so far to explain a certain category of facts, why do you persist in saying that it can't be right?

Papert Because, as inconceivable as it might seem to you, it doesn't seem to me to be the only theory that exists.

Chomsky It is not "certainly" true; these are all hypotheses. I will repeat again that they are conjectures, scientific hypotheses, and if you want to know my feeling, I'm sure that they

are false. I cannot believe that any detailed hypothesis that I can propose today, or that anybody else can propose, is likely to be true.

The debate proper concludes on this note of modesty. The rest of the book is composed of papers written on the debate and after the debate. At the time of our meeting, Chomsky's book Reflections on Language *was in press. It is my feeling that the last part of the present exchange between Papert and Chomsky on the "ideology" of innatism and environmentalism becomes much clearer in the light of two passages from* Reflections on Language, *where the problem is analyzed with unprecedented clarity and exhaustiveness.*

> *The doctrine that the human mind is initially unstructured and plastic and that human nature is entirely a social product has often been associated with progressive and even revolutionary social thinking, while speculations with regard to human instinct have often had a conservative and pessimistic cast. One can easily see why reformers and revolutionaries should become radical environmentalists, and there is no doubt that concepts of immutable human nature can be and have been employed to erect barriers against social change and to defend established privilege.*
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> *But a deeper look will show that the concept of the "empty organism," plastic and unstructured, apart from being false, also serves naturally as the support for the most reactionary social doctrines. If people are, in fact, malleable and plastic beings with no essential psychological nature, then why should they not be controlled and coerced by those who claim authority, special knowledge, and a unique insight into what is best for those less enlightened? Empiricist doctrine can easily be molded into an ideology for the vanguard party that claims authority to lead the masses to a society that will be governed by the "red bureaucracy" of which Bakunin warned. And just as easily for the liberal technocrats or corporate managers who monopolize "vital decision-making" in the institutions of state capitalist democracy, beating the people with the people's stick, in Bakunin's trenchant phrase . . .*
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> *It is reasonable to suppose that just as intrinsic structures of mind underlie the development of cognitive structures, so a "species character" provides the framework for the growth of moral consciousness, cultural achievement, and even participation in a free and just community. It is, to be sure, a great intellectual leap from observations on the basis for cognitive development to particular conclusions on the laws of our nature and the conditions for their fulfillment; say, to the conclusion that human needs and capacities will find their fullest expression in a society of free and creative producers, working in a system of free association in which "social bonds" will replace "all fetters in human society." There is an important intellectual tradition that stakes out some interesting claims in this regard. While this tradition draws from the empiricist commitment to progress and enlightenment, I think it finds still deeper roots in rationalist efforts to establish a theory of human freedom. To investigate, deepen, and if possible substantiate the ideas developed in this tradition by the methods of science is a fundamental task for libertarian social theory. Whether further investigation will reveal problems that can be addressed or mysteries that will confound us, only the future can tell . . .**

* N. Chomsky, *Reflections on Language* (New York: Pantheon, 1975), pp. 132–134.