

**THE  
LANGUAGE  
OF  
THOUGHT**  
JERRY A. FODOR



The Language & Thought Series

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# THE LANGUAGE OF THOUGHT

JERRY A. FODOR

*Massachusetts Institute of Technology*

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# **THE LANGUAGE OF THOUGHT**

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## The Language & Thought Series

THE LANGUAGE AND THOUGHT SERIES UNDER THE GENERAL EDITORSHIP OF  
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*The wind is changing*

MARY POPPINS

Much of what appears here emerged from discussions that I have had at odd times (and in, for that matter, odd places) with my wife, Janet Dean Fodor. This book is dedicated, with love and gratitude, to her.

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## PREFACE

There used to be a discipline called speculative psychology. It wasn't quite philosophy because it was concerned with empirical theory construction. It wasn't quite psychology because it wasn't an experimental science. But it used the methods of both philosophy and psychology because it was dedicated to the notion that scientific theories should be both conceptually disciplined and empirically constrained. What speculative psychologists did was this: They thought about such data as were available about mental processes, and they thought about such first-order psychological theories as had been proposed to account for the data. They then tried to elucidate the general conception of the mind that was implicit in the data and the theories. Speculative psychology was, by and large, quite a good thing: William James and John Dewey were speculative psychologists and so, in certain of his moods, was Clark Hull. But it's commonly said that there aren't any speculative psychologists any more.

Insofar as it's true that there aren't, the fact is easy to explain. For one thing, speculative psychology exhibited an inherent hybrid instability. The distinction between first-order theories and higher-order theories is largely heuristic in any but a formalized science, so speculative psychology tended to merge with straight psychology. The elucidation of general concepts is a typical philosophical concern, so speculative psychology tended to merge with the philosophy of mind. In consequence, speculative psychologists had trouble deciding what department they were in and were an embarrassment to deans.

There were, moreover, fashionable epistemological theories—theories about the proper conduct of science—which suggested that no respectable inquiry *could* be partly conceptual and partly empirical in the way that speculative psychology was supposed to be. According to such theories, matters of fact are distinct in principle from relations of ideas, and their elucidation ought thus to be distinct in scientific practice. Philosophers who accepted this epistemology could accuse speculative psychologists of psy-



chologizing, and psychologists who accepted it could accuse them of philosophizing. Since, according to the epistemologists, psychologizing and philosophizing are mutually incompatible activities, these accusations were received with grave concern.

There was, in short, a period when speculative psychology was viewed as a methodological anomaly and an administrative nuisance. Yet the speculative tradition never quite died out either in psychology or in the philosophy of mind. Empirical psychologists continued to design their experiments and interpret their data in light of some conception, however shadowy, of what the mind is like. (Such conceptions tended to become explicit in the course of methodological disputes, of which psychologists have plenty.) Similarly, though there are some philosophers who claim to practice pure analysis, there are lots of other philosophers who don't. For the latter, a general consonance with the facts about mental states is an acknowledged condition upon theories of the logic of mental state ascriptions. And, even analytical philosophers are sometimes to be found reading the empirical literature and laying down the law on what the data mean. Indeed, it is often the avowedly atheoretical psychologist who turns out to have the most grandiose philosophical pretensions (see, e.g., Skinner in *Beyond Freedom and Dignity*, 1971), just as it is the methodological apriorists in philosophy who often have the strongest views on how the data must be read (see, e.g., Malcom in *Dreaming*, 1962).

This book, in any event, is unabashedly an essay in speculative psychology. More specifically, it is an attempt to say how the mind works insofar as answers to that question emerge from recent empirical studies of language and cognition. The attempt seems to me to be worth making for two reasons: first, because the question of how the mind works is profoundly interesting, and the best psychology we have is ipso facto the best answer that is currently available. Second, the best psychology we have is still research in progress, and I am interested in the advancement of that research.

The last ten years or so have witnessed a proliferation of psychological research activities predicated on the view that many mental processes are computational processes, hence that much of 'higher cognitive behavior' is rule governed. Techniques for the analysis of rule governed behaviors are now familiar to scientists in a variety of disciplines: linguistics, simulation psychology, cognitive psychology, psycholinguistics, etc. There can be no argument but that the employment of these techniques has revolutionized both the practice and the theory of the behavioral sciences. But while it is easy to see that things have changed, it is less easy to say where they have gotten to. My impression is that many practitioners feel increasingly unclear about the general character of the theoretical framework that they are working in and quite uncertain what should happen next. An attempt at consolidation would therefore seem to be in order.

That, I take it, is one of the things that speculative psychology is for.



One seeks to provide enough insight into the drift of current research to aid in guiding future inquiry. This is, of course, quite a different matter from merely *summarizing* the research. One wants to say: 'If our psychology is, in general, right then the nature of the mind must be, roughly, this . . .' and then fill in the blank. Given the speculative elucidation, the experimentalist can work the other way around: 'If the nature of the mind is roughly . . . , then our psychology ought henceforth to look like this: . . .', where *this* blank is filled by new first-order theories. We ascend, in science, by tugging one another's bootstraps.

Speculative psychology, so conceived, is fraught with fallibility. For one thing, since it seeks, fundamentally, to extrapolate from the available scientific theories, it is in jeopardy of those theories proving to be false. It may, after all, turn out that the whole information-processing approach to psychology is somehow a bad idea. If it is, then such theories of the mind as it suggests are hardly likely to be true. This sort of thing has happened before in psychology. It now seems reasonably clear that the whole learning-theoretics approach to the explanation of behavior was a bad idea, and that the theory of the mind that it proposed was ludicrous. There's nothing one can do about this except to get on with the job and find out. Making explicit the account of the mind that it commits us to may be the best way of showing that our psychology has gone wrong if, in fact, it has.

Second, there is surely more than one way to read the morals of the current psychological research. I shall sketch a theory about mental processes in this book, and I shall argue that that theory *is* implied by most of what sensible linguists and cognitive psychologists accept these days. But I don't suppose that every sensible linguist or cognitive psychologist will agree with me. Indeed, what I mainly hope this book will do is provoke discussion on these points. Some of the things we seem to be committed to strike me, frankly, as a little wild. I should be glad to know if there are ways of saving the psychology while avoiding those commitments.

Finally, *qua* speculative psychologist one seeks to elaborate empirical theories of the mind which are, if not philosophically untendentious, at least philosophically respectable. But, of course, there is more than one view of what constitutes philosophical respectability, and one has to choose. I have pursued the discussion in this book on the assumption that realism is a better philosophy of science than reductionism, and that, in general, it is unadvisable for philosophers to try to make ontological points by using epistemological arguments. I acknowledge, however, the (bare) possibility that this assumption is wrong. If it is, the account of mental processes that I shall argue for is going to be badly off the mark.

This book is not entirely my fault. For one thing, it is in large part a sequel to a book I wrote with Professors T. Bever and M. Garrett (Fodor, Bever, and Garrett, 1974). Many of the ideas that get examined here were

## X PREFACE

prompted by the experience of writing, with them, an extensive review of the current experimental and theoretical literature in psycholinguistics. In the course of that exercise, we returned again and again to discussions of the underpinnings of the discipline. Much of what went on in those discussions is replicated here. My indebtedness to my coauthors probably verges on plagiarism, and my gratitude for what they taught me is unbounded.

Even so, this book would not have gotten written except for a sabbatical grant from M.I.T. and a concurrent fellowship from the Guggenheim Foundation, which, together, freed me from academic duties during the year 1973–1974. My obligation to both institutions is hereby gratefully acknowledged.

I tried out early versions of some of the material in this book in series of lectures at the Department of Psychology at the University of Oxford and the Department of Philosophy at University College, University of London. I should like to offer my thanks to Dr. Ann Treisman for arranging the former lectures and to Professor Richard Wollheim for arranging the others; also to the students and faculty at both institutions for providing useful comments and criticisms.

Finally, a number of friends and relations have read all or parts of the manuscript, invariably to good effect. Alas, none of the following are responsible for the residual errors: Professors Ned Block, Susan Carey Block, George Boolos, Noam Chomsky, Janet Dean Fodor, Jerrold Katz, Edward Martin, and George Miller. I am especially obliged to Mr. Georges Rey, who read the manuscript with great care and provided invaluable criticism and advice; and to Mrs. Cornelia Parkes, who helped with the bibliography.

The second half of the Introduction to this book is a version, slightly revised, of a paper called “Special Sciences,” which first appeared in *Synthese* (Fodor, 1974). Permission to republish this material is gratefully acknowledged. Material quoted from Chapters one and two and the conclusion of *The Construction of Reality in the Child* by Jean Piaget (translated by Marjorie Cooke) is copyrighted 1954 by Basic Books, Inc. Publishers, New York, and is used with their permission. Other quoted materials are used with the permission of D. Riedel Publishing Co.; Penguin Books Ltd.; John Wiley and Sons Inc.; The Humanities Press Inc.; and Routledge and Kegan Paul, Ltd.

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## **INTRODUCTION: TWO KINDS OF REDUCTIONISM**

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*The man who laughs is the one who  
has not yet heard the terrible news.*

**BERTHOLD BRECHT**

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I propose, in this book, to discuss some aspects of the theory of mental processes. Many readers may, however, feel that this choice of topic is ill-advised: either because they think there are no such processes to discuss or because they think there is no theory about them whose aspects will bear discussing. The second of these worries is substantive, and its consideration must be deferred to the body of the text. The best demonstration that speculative psychology can be done is, after all, to do some. But I am aware that the distrust with which many philosophers, and many philosophically sophisticated psychologists, view the kind of inquiry I shall undertake stems from something more than a jaundiced appreciation of the empirical literature. It is with the sources of this suspicion that the present chapter will primarily be concerned.

The integrity of psychological theorizing has always been jeopardized by two kinds of reductionism, each of which would vitiate the psychologist's claim to study mental phenomena. For those influenced by the tradition of logical behaviorism, such phenomena are allowed no ontological status distinct from the behavioral events that psychological theories explain. Psychology is thus deprived of its theoretical terms except where these can be construed as nonce locutions for which behavioral reductions will eventually be provided. To all intents and purposes, this means that psychologists can provide methodologically reputable accounts only of such aspects of behavior as are the effects of environmental variables.

Not surprisingly, many psychologists have found this sort of methodology intolerably restrictive: The contribution of the organism's internal states to the causation of its own behavior seems sufficiently undisputable, given the spontaneity and freedom from local environmental control that

## INTRODUCTION: TWO KINDS OF REDUCTIONISM

behavior often exhibits. Behaviorism thus invites us to deny the undisputable, but, in fact, we need not do so; there is an alternative that frequently gets endorsed. We can acknowledge that behavior is largely the effect of organic processes so long as we bear in mind that these processes *are* organic: i.e., that they are physiological processes located, presumably, in the nervous systems of organisms. Psychology can thus avoid behavioral reduction by opting for physiological reduction, but it must opt one way or the other.

Either way, the psychologist loses. Insofar as psychological explanations are allowed a theoretical vocabulary, it is the vocabulary of some *different* science (neurology or physiology). Insofar as there *are* laws about the ways in which behavior is contingent upon internal processes, it is the neurologist or the physiologist who will, in the long run, get to state them. However psychologists choose between the available reductions, their discipline is left without a proprietary subject matter. The best a working psychologist can hope for is an interim existence eked out between the horns of this dilemma and (just) tolerated by colleagues in the 'hard' sciences.

I think, however, that this is a false dilemma. I know of no convincing reason why a science should not seek to exhibit the contingency of an organism's behavior upon its internal states, and I know of no convincing reason why a science which succeeds in doing so should be reducible to brain science; not, at least, in the sense of reduction which would entail that psychological theories can somehow be *replaced* by their physiological counterparts. I shall try, in this introductory chapter, to show that both horns of the dilemma are, in fact, blunt. By doing so I hope to undermine a number of the arguments that are usually alleged against types of psychological explanations which, in succeeding chapters, I shall be taking very seriously indeed.

## LOGICAL BEHAVIORISM

Many philosophers, and some scientists, seem to hold that the sorts of theories now widely endorsed by cognitive psychologists could not conceivably illuminate the character of mental processes. For, it is claimed, such theories assume a view of psychological explanation which is, and has been shown to be, fundamentally incoherent. The line, to put it crudely, is that Ryle and Wittgenstein killed this sort of psychology some time about 1945, and there is no point to speculating on the prospects of the deceased.

I shall not attempt a full-dress refutation of this view. If the Wittgensteinian tradition in the philosophy of mind does, indeed, offer a coherent attack upon the methodology of current cognitive psychology, it is one

which depends on a complex of assumptions about the nature of explanation, the ontological status of theoretical entities, and the a priori conditions upon the possibility of linguistic communication. To meet that attack head on would require showing—what, in fact, I believe is true—that these assumptions, insofar as they are clear, are unwarranted. But that is a book's work in itself, and not a book that I feel much like writing. The best that I can do here is to sketch a preliminary defense of the methodological commitments implicit in the kind of psychological theorizing with which I shall be mainly concerned. Insofar as these commitments differ from what many philosophers have been willing to accept, even a sketch of their defense may prove to be revealing.

Among the many passages in Ryle's *Concept of Mind* (1949) that repay close attention, there is one (around p. 33) in which the cards are more than usually on the table. Ryle is discussing the question: 'What makes a clown's clowning intelligent (witty, clever, ingenious, etc.)?' The doctrine he is disapproving goes as follows: What makes the clowning intelligent is the fact that it is the consequence of certain mental operations (computations, calculations) privy to the clown and causally responsible for the production of the clown's behavior. Had these operations been other than they were, then (the doctrine claims) either the clowning would have been witless or at least it would have been witty clowning of some different kind. In short, the clown's clowning was clever in the way that it was because the mental operations upon which the clowning was causally contingent had whatever character they did have. And, though Ryle doesn't say so, it is presumably implied by this doctrine that a psychologist interested in explaining the success of the clown's performance would ipso facto be in the business of saying what those operations were and how, precisely, they were related to the overt pratfalls that the crowd saw.

Strictly speaking, this is not a single theory but a batch of closely connected ones. In particular, one can distinguish at least three claims about the character of the events upon which the clown's behavior is said to be causally contingent:

1. That some of them are mental events;
2. That some (or all) of the mental events are privy to the clown in at least the sense that they are normally unobserved by someone who observes the clown's performance, and, perhaps, also in the stronger sense that they are in principle unobservable by anyone except the clown;
3. That it is the fact that the behavior was caused by such events that makes it the kind of behavior it is; that intelligent behavior *is* intelligent because it has the kind of etiology it has.

I want to distinguish these doctrines because a psychologist might accept the sorts of theories that Ryle doesn't like without wanting to commit himself to the full implications of what Ryle calls 'Cartesianism'. For exam-

ple, Ryle assumes (as most psychologists who take a Realistic view of the designata of mental terms in psychological theories would not) that a mentalist must be a dualist; in particular, that mentalism and materialism are mutually exclusive. I have argued elsewhere that confusing mentalism with dualism is the original sin of the Wittgensteinian tradition (cf. Fodor, 1968, especially Chap. 2). Suffice it to remark here that one result of this confusion is the tendency to see the options of dualism and behaviorism as exhaustive in the philosophy of mind.

Similarly, it seems to me, one might accept some such view as that of item 3 without embracing a doctrinaire reading of item 2. It may be that some of the mental processes that are causally responsible for the clown's behavior are de facto unobservable by the crowd. It may be, for that matter, that some of these processes are de facto unobservable by the clown. But there would seem to be nothing in the project of explaining behavior by reference to mental processes which requires a commitment to epistemological privacy in the traditional sense of that notion. Indeed, for better or for worse, a materialist *cannot* accept such a commitment since his view is that mental events are species of physical events, and physical events are publicly observable at least in principle.<sup>1, 2</sup>

It is notorious that, even granting these caveats, Ryle doesn't think this kind of account could possibly be true. For this theory says that what makes the clown's clowning clever is the fact that it is the effect of a certain kind

<sup>1</sup> The purist will note that this last point depends on the (reasonable) assumption that the context 'is publicly observable at least in principle' is transparent to substitutivity of identicals.

<sup>2</sup> It might be replied that if we allow the possibility that mental events might be physical events, that some mental events might be unconscious, and that no mental event is essentially private, we will have so attenuated the term 'mental' as to deprive it of all force. It is, of course, true that the very notion of a mental event is often specified in ways that presuppose dualism and/or a strong doctrine of epistemological privacy. What is unclear, however, is what we want a definition of 'mental event' *for* in the first place.

Surely not, in any event, in order that it should be possible to do psychology in a methodologically respectable way. *Pre-theoretically* we identify mental events by reference to clear cases. *Post-theoretically* it is sufficient to identify them as the ones which fall under psychological laws. This characterization is, of course, question-begging since it rests upon an unexplained distinction between psychological laws and all the others. The present point, however, is that we are in no better position vis-à-vis such notions as chemical event (or meteorological event, or geological event . . . , etc.), a state of affairs which does not prejudice the rational pursuit of chemistry. A chemical event is one that falls under chemical laws; chemical laws are those which follow from (ideally completed) chemical theories; chemical theories are theories in chemistry; and chemistry, like all other special sciences, is individualized large post facto and by reference to its typical problems and predicates. (For example, chemistry is that science which concerns itself with such matters as the combinatorial properties of elements, the analysis and synthesis of compounds, etc.) Why, precisely, is this not good enough?



of cause. But what, in Ryle's view, actually *does* make the clowning clever is something quite else: For example, the fact that it happens out where the audience can see it; the fact that the things that the clown does are not the things that the audience expected him to do; the fact that the man he hit with the pie was dressed in evening clothes, etc.

There are two points to notice. First, none of *these* facts are in any sense private to the clown. They are not even *de facto* private in the sense of being facts about things going on in the clown's nervous system. On the contrary, what makes the clown's clowning clever is precisely the *public* aspects of his performance; precisely the things that the audience *can* see. The second point is that what makes the clowning clever is not the character of the *causes* of the clown's behavior, but rather the character of the behavior itself. It counts for the pratfall being clever that it occurred when it wasn't expected, but its occurring when it wasn't expected surely wasn't one of its causes on any conceivable construal of 'cause'. In short, what makes the clowning clever is not some event distinct from, and causally responsible for, the behavior that the clown produces. A fortiori, it is not a mental event prior to the pratfall. Surely, then, if the mentalist program involves the identification and characterization of such an event, that program is doomed from the start.

Alas for the psychology of clever clowning. We had assumed that psychologists would identify the (mental) causes upon which clever clowning is contingent and *thereby* answer the question: 'What makes the clowning clever?' Now all that appears to be left of the enterprise is the alliterations. Nor does Ryle restrict his use of this pattern of argument to undermining the psychology of clowns. Precisely similar moves are made to show that the psychology of perception is a muddle since what makes something (e.g.) the recognition of a robin or a tune is not the occurrence of some or other mental event, but rather the fact that what was claimed to be a robin was in fact a robin, and what was taken to be a rendition of "Lillibullero" was one. It is, in fact, hard to think of an area of cognitive psychology in which this sort of argument would not apply or where Ryle does not apply it. Indeed, it is perhaps Ryle's *central* point that 'Cartesian' (i.e., mentalistic) psychological theories treat what is really a *logical* relation between aspects of a single event as though it were a causal relation between pairs of distinct events. It is this tendency to give mechanistic answers to conceptual questions which, according to Ryle, leads the mentalist to orgies of regrettable hypostasis: i.e., to attempting to explain behavior by reference to underlying psychological mechanisms.<sup>3</sup>

<sup>3</sup> 'Criterion' isn't one of Ryle's words: Nevertheless, the line of argument just reviewed relates Ryle's work closely to the criteriological tradition in post-Wittgensteinian philosophy of mind. Roughly, what in Ryle's terms "makes" *a* be *F* is *a*'s possession of those properties which are criterial for the application of '*F*' to *xs*.

If this *is* a mistake I am in trouble. For it will be the pervasive assumption of my discussion that such explanations, however often they may prove to be empirically unsound, are, in principle, methodologically impeccable. What I propose to do throughout this book is to take such explanations absolutely seriously and try to sketch at least the outlines of the general picture of mental life to which they lead. So something will have to be done to meet Ryle's argument. Let's, to begin with, vary the example.

Consider the question: 'What makes Wheaties the breakfast of champions?' (Wheaties, in case anyone hasn't heard, is, or are, a sort of packaged cereal. The details are very inessential.) There are, it will be noticed, at least two kinds of answers that one might give.<sup>4</sup> A sketch of one answer, which belongs to what I shall call the 'causal story' might be: 'What make Wheaties the breakfast of champions are the health-giving vitamins and minerals that it contains'; or 'It's the carbohydrates in Wheaties, which give one the energy one needs for hard days on the high hurdle'; or 'It's the special springiness of all the little molecules in Wheaties, which gives Wheaties eaters their unusually high coefficient of restitution', etc.

It's not important to my point that any of these specimen answers should be true. What *is* essential is that some causal story or other must be true if Wheaties really *are* the breakfast of champions as they are claimed to be. Answers propose causal stories insofar as they seek to specify properties of Wheaties which may be causally implicated in the processes that make champions of Wheaties eaters. Very roughly, such answers suggest provisional values of *P* in the explanation schema: '*P* causes ((*x* eats Wheaties) brings about (*x* becomes a champion)) for significantly many values of *x*'. I assume that, if Wheaties *do* make champions of those who eat them, then there must be at least one value of *P* which makes this schema true. Since that assumption is simply the denial of the miracle theory of Wheaties, it ought not be in dispute.

<sup>4</sup> I am reading 'What makes Wheaties the breakfast of champions?' as asking 'What about Wheaties makes champions of (some, many, so many) Wheaties eaters?' rather than 'What about Wheaties makes (some, many, so many) champions eat them?' The latter question invites the reasons that champions give for eating Wheaties; and though these *may* include reference to properties Wheaties have by virtue of which its eaters become champions, they need not do so. Thus, a plausible answer to the second question which is *not* plausibly an answer to the first might be: 'They taste good'.

I am uncertain which of these questions the Wheaties people have in mind when they ask 'What makes Wheaties the breakfast of champions?' rhetorically, as, I believe, they are wont to do. Much of their advertising consists of publicizing statements by champions to the effect that they (the champions) do, in fact, eat Wheaties. If, as may be the case, such statements are offered as arguments for the truth of the presupposition of the question on its *first* reading (viz., that there *is* something about Wheaties that makes champions of those who eat them), then it would appear that General Mills has either misused the method of differences or committed the fallacy of affirmation of the consequent.

Philosophy can be made out of anything. Or less.