PLANS AND THE STRUCTURE OF BEHAVIOR

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to be a spontaneous, argumentative, personal kind of book that should irritate our sober-sided colleagues. That would be all right—sober-sided colleagues deserve to be irritated—except that more is at stake than the authors' reputations. Some of the ideas we have used are too good to lose. It would be unfortunate if our style were to conceal the true merit of the arguments we try to present. If we had had more time together, we might have been able to argue our way through to a better-balanced composition. But a year is only a year long. So, gentle reader, if your anger starts to rise, take a deep breath, accept our apologies—and push ahead.

CHAPTER 1

IMAGES AND PLANS

Consider how an ordinary day is put together. You awaken, and as you lie in bed, or perhaps as you move slowly about in a protective shell of morning habits, you think about what the day will be like—it will be hot, it will be cold; there is too much to do, there is nothing to fill the time; you promised to see him, she may be there again today. If you are compulsive, you may worry about fitting it all in, you may make a list of all the things you have to do. Or you may launch yourself into the day with no clear notion of what you are going to do or how long it will take. But, whether it is crowded or empty, novel or routine, uniform or varied, your day has a structure of its own—it fits into the texture of your life. And as you think what your day will hold, you construct a plan to meet it. What you expect to happen foreshadows what you expect to do.

The authors of this book believe that the plans you make are interesting and that they probably have some relation to how you actually spend your time during the day. We call them "plans" without malice—we recognize that you do not draw out long and elaborate blueprints for every moment of the day. You do not need to. Rough, sketchy, flexible anticipations are usually sufficient. As you brush

your teeth you decide that you will answer that pile of letters you have been neglecting. That is enough. You do not need to list the names of the people or to draft an outline of the contents of the letters. You think simply that today there will be time for it after lunch. After lunch, if you remember, you turn to the letters. You take one and read it. You plan your answer. You may need to check on some information, you dictate or type or scribble a reply, you address an envelope, seal the folded letter, find a stamp, drop it in a mailbox. Each of these subactivities runs off as the situation arises—you did not need to enumerate them while you were planning the day. All you need is the name of the activity that you plan for that segment of the day, and from that name you then proceed to elaborate the detailed actions involved in carrying out the plan.

You *imagine* what your day is going to be and you make *plans* to cope with it. Images and plans. What does modern psychology have to say about images and plans?

Presumably, the task of modern psychology is to make sense out of what people and animals do, to find some system for understanding their behavior. If we, as psychologists, come to this task with proper scientific caution, we must begin with what we can see and we must postulate as little as possible beyond that. What we can see are movements and environmental events. The ancient subject matter of psychology—the mind and its various manifestations—is distressingly invisible, and a science with invisible content is likely to become an invisible science. We are therefore led to underline the fundamental importance of behavior and, in particular, to try to discover recurrent patterns of stimulation and response.

What an organism does depends on what happens around it. As to the way in which this dependency should be described, however, there are, as in most matters of modern psychology, two schools of thought. On the one hand are the optimists, who claim to find the dependency simple and straightforward. They model the stimulus-response relation after the classical, physiological pattern of the reflex arc and use Pavlov's discoveries to explain how new reflexes can be formed through experience. This approach is too simple for all but the most extreme optimists. Most psychologists quickly realize that

behavior in general, and human behavior in particular, is not a chain of conditioned reflexes. So the model is complicated slightly by incorporating some of the stimuli that occur after the response in addition to the stimuli that occur before the response. Once these "reinforcing" stimuli are included in the description, it becomes possible to understand a much greater variety of behaviors and to acknowledge the apparently purposive nature of behavior. That is one school of thought.

Arrayed against the reflex theorists are the pessimists, who think that living organisms are complicated, devious, poorly designed for research purposes, and so on. They maintain that the effect an event will have upon behavior depends on how the event is represented in the organism's picture of itself and its universe. They are quite sure that any correlations between stimulation and response must be mediated by an organized representation of the environment, a system of concepts and relations within which the organism is located. A human being—and probably other animals as well—builds up an internal representation, a model of the universe, a schema, a simulacrum, a cognitive map, an Image. Sir Frederic C. Bartlett, who uses the term "schema" for this internal representation, describes it in this way:

> "Schema" refers to an active organisation of past reactions, or of past experiences, which must always be supposed to be operating in any well-adapted organic response. That is, whenever there is any order or regularity of behavior, a particular response is possible only because it is related to other similar responses which have been serially organised, yet which operate, not simply as individual members coming one after another, but as a unitary mass. Determination by schemata is the most fundamental of all the ways in which we can be influenced by reactions and experiences which occurred some time in the past. All incoming impulses of a certain kind, or mode, go together to build up an active, organised setting: visual, auditory, various types of cutaneous impulses and the like, at a relatively low level; all the experiences connected by a common interest: in sport, in literature, history, art, science, philosophy, and so on, on a higher level.[1]

[1] Frederic C. Bartlett, *Remembering, A Study in Experimental and Social Psychology* (Cambridge: Cambridge University Press, 1932), p. 201.

The crux of the argument, as every psychologist knows, is whether anything so mysterious and inaccessible as "the organism's picture of itself and its universe," or "an active organisation of past reactions," etc., is really necessary. Necessary, that is to say, as an explanation for the behavior that can be observed to occur.

The view that some mediating organization of experience is necessary has a surprisingly large number of critics among hard-headed, experimentally trained psychologists. The mediating organization is, of course, a theoretical concept and, out of respect for Occam's Razor, one should not burden the science with unnecessary theoretical luggage. An unconditional proof that a completely consistent account of behavior cannot be formulated more economically does not exist, and until we are certain that simpler ideas have failed we should not rush to embrace more complicated ones. Indeed, there are many psychologists who think the simple stimulus-response-reinforcement models provide an adequate description of everything a psychologist should concern himself with.

For reasons that are not entirely clear, the battle between these two schools of thought has generally been waged at the level of animal behavior. Edward Tolman, for example, has based his defense of cognitive organization almost entirely on his studies of the behavior of rats—surely one of the least promising areas in which to investigate intellectual accomplishments. Perhaps he felt that if he could win the argument with the simpler animal, he would win it by default for the more complicated ones. If the description of a rodent's cognitive structure is necessary in order to understand its behavior, then it is just that much more important for understanding the behavior of a dog, or an ape, or a man. Tolman's position was put most simply and directly in the following paragraph:

> [The brain] is far more like a map control room than it is like an old-fashioned telephone exchange. The stimuli, which are allowed in, are not connected by just simple one-to-one switches to the outgoing responses. Rather, the incoming impulses are usually worked over and elaborated in the central control room into a tentative, cognitivelike map of the environment. And it is this tentative map, indicating routes and paths and environmental relationships, which finally determines what responses, if any, the animal will finally release.[2]

[2] Edward C. Tolman, Cognitive maps in rats and men, *Psychological Review*, 1948, 55, 189–208.

We ourselves are quite sympathetic to this kind of theorizing, since it seems obvious to us that a great deal more goes on between the stimulus and the response than can be accounted for by a simple statement about associative strengths. The pros and cons cannot be reviewed here—the argument is long and other texts [3] exist in which an interested reader can pursue it—so we shall simply announce that our theoretical preferences are all on the side of the cognitive theorists. Life is complicated.

Nevertheless, there is a criticism of the cognitive position that seems quite important and that has never, so far as we know, received an adequate answer. The criticism is that the cognitive processes Tolman and others have postulated are not, in fact, sufficient to do the job they were supposed to do. Even if you admit these ghostly inner somethings, say the critics, you will not have explained anything about the animal's behavior. Guthrie has made the point about as sharply as anyone:

> Signs, in Tolman's theory, occasion in the rat *realization*, or *cognition*, or *judgment*, or *hypotheses*, or *abstraction*, but *they do not occasion action*. In his concern with what goes on in the rat's mind, Tolman has neglected to predict what the rat will do. So far as the theory is concerned the rat is left buried in thought; if he gets to the food-box at the end that is his concern, not the concern of the theory.[4]

Perhaps the cognitive theorists have not understood the force of this criticism. It is so transparently clear to them that if a hungry rat knows where to find food—if he has a cognitive map with the food-box located on it—he will go there and eat. What more is there to explain? The answer, of course, is that a great deal is left to be explained. The gap from knowledge to action looks smaller than the gap from stimulus to action—yet the gap is still there, still indefinitely large. Tolman, the omniscient theorist, leaps over that gap when he infers the rat's cognitive organization from its behavior. But that leaves still outstanding the question of the rat's ability to leap it. Apparently, cognitive theorists have assumed that their best course was

[3] See, for example, either E. R. Hilgard, *Theories of Learning* (New York: Appleton-Century-Crofts, ed. 2, 1956), or W. K. Estes *et al.*, *Modern Learning Theory* (New York: Appleton-Century-Crofts, 1954), or D. O. Hebb, *The Organization of Behavior* (New York: Wiley, 1949).

[4] E. R. Guthrie, *The Psychology of Learning* (New York: Harper, 1935), p. 172.

to show that the reflex theories are inadequate; they seem to have been quite unprepared when the same argument—that things are even more complicated than they dared to imagine—was used against them. Yet, if Guthrie is right, more cognitive theory is needed than the cognitive theorists normally supply. That is to say, far from respecting Occam's Razor, the cognitive theorist must ask for even *more* theoretical luggage to carry around. Something is needed to bridge the gap from knowledge to action.

It is unfair to single out Tolman and criticize him for leaving the cognitive representation paralytic. Other cognitive theorists could equally well be cited. Wolfgang Köhler, for example, has been subjected to the same kind of heckling. In reporting his extremely perceptive study of the chimpanzees on Tenerife Island during the first World War, Köhler wrote:

> We can . . . distinguish sharply between the kind of behavior which from the very beginning arises out of a consideration of the structure of a situation, and one that does not. Only in the former case do we speak of insight, and only that behavior of animals definitely appears to us intelligent which takes account from the beginning of the lay of the land, and proceeds to deal with it in a single, continuous, and definite course. Hence follows this criterion of insight: *the appearance of a complete solution with reference to the whole lay-out of the field.*[5]

Other psychologists have been less confident that they could tell the difference between behavior based on an understanding of the whole layout and behavior based on less cognitive processes, so there has been a long and rather fruitless controversy over the relative merits of trial-and-error and of insight as methods of learning. The point we wish to raise here, however, is that Köhler makes the standard cognitive assumption: once the animal has grasped the whole layout he will behave appropriately. Again, the fact that grasping the whole layout may be necessary, but is certainly not sufficient as an explanation of intelligent behavior, seems to have been ignored by Köhler. Many years later, for example, we heard Karl Lashley say this to him:

> I attended the dedication, three weeks ago, of a bridge at Dyea, Alaska. The road to the bridge for nine miles was blasted along a series of cliffs. It led to a magnificent steel bridge, permanent and apparently indestructible. After the dedication ceremonies I walked across the bridge and was confronted with an impenetrable forest of shrubs and underbrush, through which only a couple of trails of bears led to indeterminate places. In a way, I feel that Professor Köhler's position is somewhat that of the bridge. . . . The neurological problem is in large part, if not entirely, the translation of the afferent pattern of impulses into the efferent pattern. The field theory in its present form includes no hint of the way in which the field forces induce and control the pattern of efferent activity. It applies to perceptual experience but seems to end there.[6]

Many other voices could be added to this dialogue. Much detailed analysis of different psychological theories could be displayed to show why the cognitive theorists feel they have answered the criticism and why their critics still maintain that they have not. But we will not pursue it. Our point is that many psychologists, including the present authors, have been disturbed by a theoretical vacuum between cognition and action. The present book is largely the record of prolonged—and frequently violent—conversations about how that vacuum might be filled.

No doubt it is perfectly obvious to the reader that we have here a modern version of an ancient puzzle. At an earlier date we might have introduced the topic directly by announcing that we intended to discuss the will. But today the will seems to have disappeared from psychological theory, assimilated anonymously into the broader topic of motivation. The last serious attempt to make sense out of the will was the early work of Kurt Lewin and his students. Lewin's contributions are so important that we will treat them in detail in Chapter 4; we cannot dismiss them summarily by a paragraph in this introduction. In order to show what a psychology of will might be like, therefore, it is necessary to return to an earlier and more philosophical generation of psychologists. William James provides the sort of discussion that was once an indispensable part of every psychology text, so let us consider briefly how he handled the topic.

The second volume of *The Principles* contains a long chapter

[5] Wolfgang Köhler, *The Mentality of Apes* (translated from the second edition by Ella Winter; London: Routledge and Kegan Paul, 1927), pp. 169–170.

[6] Lloyd A. Jeffress, ed., *Cerebral Mechanisms in Behavior* (New York: Wiley, 1951), p. 230.

(106 pages) entitled "Will." The first third of it is James's struggle against theories based on "sensations of innervation"—the notion that the innervation required to perform the appropriate action is itself a part of the cognitive representation. James maintains instead that it is the anticipation of the kinesthetic effects of the movement that is represented in consciousness. He then turns to the topic of "ideo-motor action," which provides the foundation for his explanation of all phenomena of will. If a person forms a clear image of a particular action, that action tends to occur. The occurrence may be inhibited, limited to covert tensions in the muscles, but in many cases having an idea of an action is sufficient for action. If there is anything between the cognitive representation and the overt action, it is not represented in consciousness. Introspectively, therefore, there seems to be no vacuum to be filled, and James, had he heard them, would have felt that criticisms of the sort made by Guthrie and Lashley were not justified.

But what of the more complicated cases of willing? What occurs when we force ourselves through some unpleasant task by "the slow dead heave of the will?" According to James, the feeling of effort arises from our attempt to keep our attention focused on the unpleasant idea. "The essential achievement of the will," he tells us, "is to *attend* to a difficult object and hold it fast before the mind." [7] If an idea can be maintained in attention, then the action that is envisioned in the idea occurs automatically—a direct example of ideo-motor action. All of which helps us not in the least. The bridge James gives us between the *ideo* and the *motor* is nothing but a hyphen. There seems to be no alternative but to strike out into the vacuum on our own.

The problem is to describe how actions are controlled by an organism's internal representation of its universe. If we consider what these actions are in the normal, freely ranging animal, we must be struck by the extent to which they are organized into patterns. Most psychologists maintain that these action patterns are punctuated by goals and subgoals, but that does not concern us for the moment. We wish to call attention to the fact that the organization does exist—configuration is just as important a property of behavior as it is of

[7] William James, *The Principles of Psychology*, Vol. II (New York: Holt, 1890), p. 561.

perception. The configurations of behavior, however, tend to be predominantly temporal—it is the *sequence* of motions that flows onward so smoothly as the creature runs, swims, flies, talks, or whatever. What we must provide, therefore, is some way to map the cognitive representation into the appropriate *pattern* of activity. But how are we to analyze this flowing pattern of action into manageable parts?

The difficulty in analyzing the actions of an animal does not arise from any lack of ways to do it but from an embarrassment of riches. We can describe an action as a sequence of muscle twitches, or as a sequence of movements of limbs and other parts, or as a sequence of goal-directed actions, or in even larger units. Following Tolman, most psychologists distinguish the little units from the big units by calling the little ones "molecular," the big ones, "molar." Anyone who asks which unit is the correct size to use in describing behavior is told that behavioral laws seem more obvious when molar units are used, but that just how molar he should be in any particular analysis is something he will have to learn from experience and observation in research.

The implication is relatively clear, however, that the molar units must be composed of molecular units, which we take to mean that a proper description of behavior must be made on *all levels simultaneously*. That is to say, we are trying to describe a process that is organized on several different levels, and the pattern of units at one level can be indicated only by giving the units at the next higher, or more molar, level of description. For example, the molar pattern of behavior X consists of two parts, A and B, in that order. Thus, $X = AB$. But A, in turn, consists of two parts, a and b; and B consists of three, c, d, and e. Thus, $X = AB = abcde$, and we can describe the same segment of behavior at any one of the three levels. The point, however, is that we do not want to pick one level and argue that it is somehow better than the others; the complete description must include all levels. Otherwise, the configurational properties of the behavior will be lost—if we state only $abcde$, for example, then $(ab)(cde)$ may become confused with $(abc)(de)$, which may be a very different thing.

This kind of organization of behavior is most obvious, no doubt,

in human verbal behavior. The individual phonemes are organized into morphemes, morphemes are strung together to form phrases, phrases in the proper sequence form a sentence, and a string of sentences makes up the utterance. The complete description of the utterance involves all these levels. The kind of ambiguity that results when all levels are not known is suggested by the sentence, "They are flying planes." The sequence of phonemes may remain unchanged, but the two analyses (*They*)(*are flying*)(*planes*) and (*They*)(*are*)(*flying planes*) are very different utterances.[8]

Psychologists have seldom demonstrated any reluctance to infer the existence of such molar units as "words" or even "meanings" when they have dealt with verbal behavior, even though the actual responses available to perception are merely the strings of phones, the acoustic representations of the intended phonemes. Exactly the same recognition of more molar units in nonverbal behavior deserves the same kind of multi-level description. Unfortunately, however, the psychologist usually describes behavior—or some aspect of behavior—at a single level and leaves his colleagues to use their own common sense to infer what happened at other levels. The meticulous recording of every muscle twitch, even if anyone were brave enough to try it, would still not suffice, for it would not contain the structural features that characterize the molar units—and those structural features must be *inferred* on the basis of a *theory* about behavior. Our theories of behavior, in this sense of the term, have always remained implicit and intuitive. (It is rather surprising to realize that after half a century of behaviorism this aspect of the problem of describing behavior has almost never been recognized, much less solved.)

[8] The traditional method of parsing a sentence is the prototype of the kind of behavioral description we demand. Noam Chomsky, in Chapter 4 of his monograph, *Syntactic Structures* (The Hague: Mouton, 1957), provides a formal representation of this kind of description, which linguists refer to as "constituent analysis." We shall discuss Chomsky's method of representing verbal behavior in more detail in Chapter 11. The suggestion that linguistic analysis provides a model for the description of all kinds of behavior is, of course, no novelty; it has been made frequently by both linguists and psychologists. For example, in *The Study of Language* (Cambridge: Harvard University Press, 1953), John B. Carroll, a psychologist, observed that, "From linguistic theory we get the notion of a hierarchy of units—from elemental units like the distinctive feature of a phoneme to large units like a sentence-type. It may be suggested that stretches of any kind of behavior may be organized in somewhat the same fashion" (p. 106).

In those fortunate instances that do give us adequate descriptions of behavior—instances provided almost entirely by linguists and ethologists—it is quite obvious that the behavior is organized simultaneously at several levels of complexity. We shall speak of this fact as the "hierarchical organization of behavior."[9] The hierarchy can be represented in various ways. The diagram of a hierarchy usually takes the form of a tree, the arborizations indicating progressively more molecular representations. Or it can be cast as an outline:

X.
- *A.*
  - *a.*
  - *b.*
- *B.*
  - *c.*
  - *d.*
  - *e.*

This outline shows the structure of the hypothetical example introduced on page 13. Or it can be considered as a collection of lists: *X* is a list containing the two items, *A* and *B*; *A* is a list containing two items, *a* and *b*; *B* is a list containing three items, *c*, *d*, and *e*.[10] Or it can be considered as a set of rules governing permissible substitu-

[9] Many psychologists are familiar with the notion that behavior is hierarchically organized because they remember Clark Hull's use of the phrase "habit-family hierarchy." We must hasten to say, therefore, that Hull's use of the term "hierarchy" and our present use of that term have almost nothing in common. We are talking about a hierarchy of levels of representation. Hull was talking about an ordering of alternative (interchangeable, substitutable) responses according to their strengths. See, for example, C. L. Hull, The concept of the habit-family hierarchy and maze learning, *Psychological Review*, 1934, 41, 33–54; 134–152. Closer to the spirit of the present discussion is the system of behavioral episodes used by Roger G. Barker and Herbert F. Wright, in *Midwest and Its Children* (Evanston: Row, Peterson, 1954), to describe the molar behavior of children in their natural habitats. The work of Barker and Wright is a noteworthy exception to our assertion that psychologists have not tried to describe the structural features of behavior.

[10] The tree and outline forms of representation are quite ancient and familiar, but the use of list structures for representing such organizations is, we believe, relatively new. We first became acquainted with it through the work of Newell, Shaw, and Simon on the simulation of cognitive processes by computer programs. See, for example, Allen Newell and Herbert A. Simon, The logic theory machine: A complex information processing system, *IRE Transactions on Information Theory*, 1956, Vol. IT-2, No. 3, 61-79. Also, Allen Newell and J. C. Shaw, Programming the logic theory machine, *Proceedings of the Western Joint Computer Conference*, Los Angeles, February 1957, pp. 230–240.

tions: Where *X* occurs, we can substitute for it *AB;* where *A* occurs we can substitute *ab;* etc.[11] Each of these methods of presentation of a hierarchy has its special advantages in special situations.

Now, if the hierarchical nature of the organization of behavior can be taken as axiomatic, the time has come to set aside a few terms for the special purposes of the present discussions. Because definitions make heavy reading, we shall keep the list as short as possible.

Plan. Any complete description of behavior should be adequate to serve as a set of instructions, that is, it should have the characteristics of a plan that could guide the action described. When we speak of a Plan in these pages, however, the term will refer to a *hierarchy* of instructions, and the capitalization will indicate that this special interpretation is intended. *A Plan is any hierarchical process in the organism that can control the order in which a sequence of operations is to be performed.*

A Plan is, for an organism, essentially the same as a program for a computer, especially if the program has the sort of hierarchical character described above. Newell, Shaw, and Simon have explicitly and systematically used the hierarchical structure of lists in their development of "information-processing languages" that are used to program high-speed digital computers to simulate human thought processes. Their success in this direction—which the present authors find most impressive and encouraging—argues strongly for the hypothesis that a hierarchical structure is the basic form of organization in human problem-solving. Thus, we are reasonably confident that "program" could be substituted everywhere for "Plan" in the following pages. However, the reduction of Plans to nothing but programs is still a scientific hypothesis and is still in need of further validation. For the present, therefore, it should be less confusing if we regard a computer program that simulates certain features of an organism's behavior as a theory about the organismic Plan that generated the behavior.[12]

[11] Chomsky, *op. cit.*, p. 26.

[12] It should be clearly recognized that, as Newell, Shaw, and Simon point out, comparing the sequence of operations executed by an organism and by a properly programmed computer is quite different from comparing computers with brains, or electrical relays with synapses, etc. See Allen Newell, J. C. Shaw, and Herbert A. Simon, Elements of a theory of human problem solving. *Psychological Review,* 1958, 65, 151–166. Also, Herbert A. Simon and Allen Newell, Models, their uses and limitations, in L. D. White, ed., *The State of the Social Sciences* (Chicago: University of Chicago Press, 1956), pp. 66–83.

Moreover, we shall also use the term "Plan" to designate a rough sketch of some course of action, just the major topic headings in the outline, as well as the completely detailed specification of every detailed operation.[13]

Strategy and Tactics. The concept of the hierarchical organization of behavior was introduced earlier with the distinction between molar and molecular units of analysis. Now, however, we wish to augment our terminology. The molar units in the organization of behavior will be said to comprise the behavioral *strategy,* and the molecular units, the *tactics.*

Execution. We shall say that a creature is executing a particular Plan when in fact that Plan is controlling the sequence of operations he is carrying out. When an organism executes a Plan he proceeds through it step by step, completing one part and then moving to the next. The execution of a Plan need not result in overt action—especially in man, it seems to be true that there are Plans for collecting or transforming information, as well as Plans for guiding actions. Although it is not actually necessary, we assume on intuitive grounds that only one Plan is executed at a time, although relatively rapid alternation between Plans may be possible. An organism may—probably does—store many Plans other than the ones it happens to be executing at the moment.

Image. The Image is all the accumulated, organized knowledge that the organism has about itself and its world. The Image consists of a great deal more than imagery, of course. What we have in mind when we use this term is essentially the same kind of private representation that other cognitive theorists have demanded. It includes

[13] Newell, Shaw, and Simon have also used "plan" to describe a general strategy before the details have been worked out, but they distinguish between such a plan and the program that enables a computer to use planning as one of its problem-solving techniques. See Allen Newell, J. C. Shaw, and Herbert A. Simon, A report on a general problem solving program. *Proceedings of the International Conference on Information Processing,* Paris, 1959 (in press).

Other workers have used the term "machine" rather loosely to include both the Plan and the instrument that executes it. For example, see M. L. Minsky, *Heuristic Aspects of the Artificial Intelligence Problem,* Group Report 34–55, Lincoln Laboratory, Massachusetts Institute of Technology, 17 December 1956, especially Section III.3.

everything the organism has learned—his values as well as his facts—organized by whatever concepts, images, or relations he has been able to master.

In the course of prolonged debates the present authors heard themselves using many other terms to modify "Plan" in rather special ways, but they will not be listed here. New terms will be defined and developed as they are needed in the course of the argument that follows. For the moment, however, we have defined enough to be able to say that the central problem of this book is *to explore the relation between the Image and the Plan.*

Stated so, it may seem to imply some sharp dichotomy between the two, so that it would be meaningful to ask, "Is such-and-such a process exclusively in the Plan or exclusively in the Image?" That the two points of view cannot be used in that way to classify processes into mutually exclusive categories should become apparent from such considerations as these:

—A Plan can be learned and so would be a part of the Image.

—The names that Plans have must comprise a part of the Image for human beings, since it must be part of a person's Image of himself that he is able to execute such-and-such Plans.

—Knowledge must be incorporated into the Plan, since otherwise it could not provide a basis for guiding behavior. Thus, Images can form part of a Plan.

—Changes in the Images can be effected only by executing Plans for gathering, storing, or transforming information.

—Changes in the Plans can be effected only by information drawn from the Images.

—The transformation of descriptions into instructions is, for human beings, a simple verbal trick.

Psychologists who are accustomed to think of their problem as the investigation of relations between Stimulus and Response are apt to view the present undertaking in a parallel way—as an investigation of relations between a subjective stimulus and a subjective response. If that were all we had to say, however, we would scarcely have written a book to say it. Stimulus and response are physiological concepts borrowed from the discussion of reflexes. But we have rejected the classical concept of the reflex arc as the fundamental pattern for the

organization of all behavior, and consequently we do not feel a need to extend the classic disjunction between stimulus and response variables into the realm of Images and Plans. To assume that a Plan is a covert response to some inner Image of a stimulus does nothing but parallel objective concepts with subjective equivalents and leaves the reflex arc still master—albeit a rather ghostly master—of the machinery of the mind. We are not likely to overthrow an old master without the help of a new one, so it is to the task of finding a successor that we must turn next.