Adaptive Dynamics

The Theoretical Analysis of Behavior

J. E. R. Staddon

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In memory of my father,
an ingenious man

Contents

Preface

Mach's weakness, as I see it, lies in the fact that he believed more or less strongly that science consists merely of putting experimental results in order; that is, he did not recognize the free constructive element. . . . He thought that somehow theories arise by means of *discovery* and not by means of *invention*.

—Albert Einstein

This book is an argument for a simple proposition: that the way to understand the laws and causes of learning in animals and man is through the invention, comparison, testing, and modification or rejection of parsimonious black-box models. The models I discuss are neither physiological nor cognitive. They are *behavioristic*, in the most elementary sense: "One interesting definition [of the concept of thinking] has been proposed by A. M. Turing: a machine is termed capable of thinking if it can . . . imitate a human being by answering questions sufficiently well to deceive a human questioner for a reasonable period of time. A definition of this type has the advantage of being operational or, in the psychologists' term, behavioristic" (Shannon & McCarthy, 1956, p. v). Turing wanted to compare human and machine verbal protocols; I will be comparing the real-time behavior of animal and machine.

Behaviorism is an unfashionable position these days. It lost favor not, I believe, because it is fundamentally wrong, but because of the biases of its gurus. J. B. Watson eschewed internal states entirely. Clark Hull, now almost forgotten but once hugely influential, thought that a theory of learning could be built on fragmentary understanding of the nervous system, using the conceptual tools of philosophical positivism. But positivism (like all philosophy of science) is descriptive, not prescriptive. Hull's formal skills were modest; the computational resources available to him were primitive; and his between-group experimental method is ill-suited to the discovery of learning processes in individual organisms. It is no wonder, therefore, that his ambitious attempt largely failed. More successful was the radical behaviorism of B. F. Skinner, because his experimental methods were more powerful, his writing style more persuasive, and his public relations—linking operant conditioning to the universal cause of human betterment—much more effective. But Skinner also failed, because he did not understand or value *theory*. He condemned theories that involve "explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1950, p. 193)—an odd view that rules out of science the gene theory, atomic theory, and indeed almost any kind of reductionism. Since even the most elementary learning process, habituation, requires at least one hidden ("internal-state") variable for an adequate description, Skinner's proscription of hidden variables effectively ruled out any real learning theory (Staddon, 1997b). But theory phobia is a historical accident, not intrinsic to behaviorism.

Reflecting its ancestry in experimental psychology, the psychology of learning has for too long regarded the collection of data as its main task. Such theories as there are—most obviously Hullian theory, but also more recent theories of choice, classical conditioning, and interval timing—are like elaborate Renaissance oil paintings: constructed incrementally, brush stroke by brush stroke, experiment by experiment. They are built with the confidence that their foundations are secure. And all accept physiological reductionism, the idea that elements of the perfected theory should map easily onto neurophysiology. The addition of neural networks to the theoretical tool kit in the 1970s only underlined this implicit commitment to psychoneural isomorphism—despite the vast gulf between the simple logical structure of backpropagation networks and the messy confusion of real neurophysiology.

The alternative I offer is also theoretical, but more like watercolor than oil painting. Rather than building up a massive structure step by step, I propose instead a succession of simple sketches, each designed to capture some aspect of the learning process. I argue that our understanding of behavior can advance only through what Einstein called the "free constructive element"—the invention and testing of formal models for behavior. I emphatically dispute an unstated premise of the purely empirical approach Einstein condemned: "that science consists merely of putting experimental results in order." *Conjecture*, not just about variables but also about *processes*, is essential.

The models I propose are simple, physiologically agnostic, and disposable. Parsimony is a primary value. The data are entirely behavioral. If the model nevertheless seems to fit well with physiology (as it does, in a few cases), so much the better. But the aim is to explain *behavior*, not brain–behavior relations. And as each model is proposed and, eventually, retired, we may hope to learn a little more about the essential mechanisms of learning—so the next model will be a little better, a bit more securely founded. Some history, and the problems with bottom-up physiological reductionism, are discussed in chapter 1.

The central fact about behavior is that it is a *process*, a series of events that occur in time. It is *dynamic*. So my main emphasis is on the way behavior changes in time. As to the important "variables of which behavior is a function," in Skinner's phrase, I believe that the best guide *is* function, but in Tinbergen's (1963) sense of "adaptive function"—studied formally as optimality analysis—not Skinner's. Optimality analysis, which answers the question "What does this behavior maximize?," is perhaps the most successful attempt to date to provide a unified account of operant (instrumental) behavior in animals. Chapters 2 and 3 summarize psychological and economic optimality accounts of operant behavior. I conclude that economics is not fundamental, but it can point to variables that are likely to be important in causal accounts. Chapter 4 serves two functions: it is an introduction to elementary dynamics; and it offers models of the simplest kind of adaptive behavior, *trial and error*, in some very simple organisms. This chapter introduces most of the theoretical ingredients I use in later chapters. Chapter 5 shows how the properties of reflexes, described in every introductory learning text, can be derived from simple dynamic elements. Chapter 6 introduces a major topic: habituation, the most elementary learning process and one linked (I will argue) to general principles of memory and interval timing. Chapters 7, 8, and 9 discuss feeding regulation, a process involved in almost every reinforcement-learning experiment, but one whose dynamics are still not fully understood. I offer an extremely simple model for the regulatory dynamics of feeding and show that the model can explain how rats adapt to a wide range of environmental feeding challenges. Chapter 10 discusses assignment of credit, an essential function of both operant (What did I do that caused that?) and classical (What stimulus signaled that?) conditioning. The chapter ends with questions rather than answers, because I offer no model for classical conditioning or associative processes in general. Chapters 11 and 12 nibble at the edges of the associative question, discussing a very simple dynamic model for stimulus generalization and applying it to spatial navigation. Chapters 13, 14, and 15 deal with another associative question: interval timing. I conclude that there may be no dedicated "interval clock." Instead, interval timing, habituation, and human forgetting may all rely on the same memory process. The final chapter summarizes the basic approach. Chapters are roughly progressive, in the sense that earlier chapters assume less subject knowledge and mathematical sophistication than later ones.

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As always, without the support of my wife, Lucinda, little would have been accomplished.

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Adaptive Dynamics

1

Theoretical Behaviorism: Aim and Methods

Simple isn't the same as *obvious*. Many years ago, I attended a lecture by Georg von Békésy, distinguished physiologist and experimental psychologist and, a few years later, Nobel laureate for his work on the mechanisms of hearing. Békésy was lecturing to the large introductory course in natural science at Harvard. The course was organized along "parade of stars" lines, with various luminaries giving elementary summaries of their special topics in one or two lectures each. Békésy spoke in the vast bowllike Allston Burr Auditorium, freshly built in the brutal concrete then fashionable (but now demolished, perhaps because too many students plummeted from its higher tiers). I was sitting behind an undergraduate, and because of the steep rake of the seats, I could see her notepad clearly. At the beginning of the lecture she was writing furiously. But as elegant experiment followed elegant experiment—each one introduced with the phrase (in Békésy's thick Hungarian accent) "Eet is ferrry simple to see . . ."—her note-taking began to flag. After thirty minutes or so she turned to a fresh page and scrawled in large, block capitals "IT IS VERY SIMPLE. . . ."

Békésy's work *is* simple, in the scientific sense,[1] but obviously the ability to discern scientific simplicity is not inborn. Often forgotten in psychology is the great truth that in science, simplicity—of a more subtle kind, perhaps, than can be immediately appreciated by a beginning undergraduate—is *the* royal road to scientific truth. Not that every simple theory is true; experimental test, after all, trumps simplicity. But when two theories are compared, the simpler, in a special sense I will try to convey, is almost invariably the better one. This book aims to present a picture of the psychology of learning that is simple in Békésy's sense.

I take for granted two things with which no biobehavioral scientist would quarrel: that organisms are machines; and that the physical machinery for behavior is biological—nerves, muscles, and glands. But I also argue for a third thesis, not universally accepted: that an essential step in understanding how all this machinery works is dynamic *black-box modeling*. I propose that a black-box model will always provide the most compact summary of behavior. And such an account is likely to be *much simpler* than a neurophysiological account, if indeed one can be found (we are a long way from understanding the neurophysiological underpinnings for most behavior). I also propose that an accurate model may be essential to discovering the physical processes that underlie behavior, because it tells the neuroscientist just what task the brain is carrying out. Because this view differs in some respects from cognitive psychology, which also deals in models, and because its heritage is behavioristic, I call it *theoretical behaviorism* (Staddon, 1993, 1998; see Staddon, 2001, for a fuller account of theoretical behaviorism).

This chapter sets the framework for what follows. First, I discuss the relation between behavioral studies—theory and experiment—and neurophysiology. Many behavioral scientists, and almost all neuroscientists, still think that psychology (the study of behavior) is in principle subordinate to neuroscience (the study of the brain). They are wrong, and I'll try to explain why. If the secrets of behavior are not to be found by expanding our understanding of neurons and brain function, how *is* the job to be done? Through experiment and the creation and testing of simple models, I believe. The latter part of the chapter discusses the role of models and explains the two types of experiments that are used to study behavior. The rest of the book will explore some models that my colleagues and I have found useful in the study of learning.

Brain and Behavior

Evolution in Silicon

The best way to explain, and defend, the thesis that modeling is at the core of psychology is through an example not from psychology but from engineering. The constructions of engineers are not intrinsically mysterious; and we all know, or think we know, how machines work. If I can show, therefore, that the physical workings of some modern machines are almost as hard to understand as the neurobiology of behavior, the approach to psychobiology that I propose will seem less shocking.

The "hardwiring" of some silicon semiconductor devices can be reconfigured by software instructions, and this has led to some unexpected possibilities. An account in *Science* magazine put it this way:

> The innovation arrived, so the story has it, as an epiphany. Hugo de Garis, a computer scientist who . . . describes his avocation as building brains, was visiting George Mason University in Fairfax, Virginia, in the summer of 1992, when he had a discussion with an electrical engineer. This "E.E. guy," . . . was telling him about computer chips known as field programmable gate arrays, or FPGAs, which are, in effect, pieces of hardware that can be reconfigured by software. "You can send in a software instruction, and it tells this programmable hardware how to wire itself up," says de Garis.
>
> De Garis then had a moment of visionary clarity: "The idea occurred to me," he says, "that if you could send in a software instruction to a piece of hardware to wire it up, maybe you could look on that software instruction as the equivalent of a genetic chromosome. You could breed it, by mating chromosomes, mutate it randomly, maybe actually evolve the hardware. So then I started asking this guy, 'Could you do this infinitely; could you just keep sending in instructions, rewriting the hardware again and again and again?' And he said that for some FPGAs that was possible." (Taubes, 1997)

The story goes on to describe a number of examples of this "silicon evolution," emphasizing its engineering applications. But the most interesting example is not particularly exciting as engineering:

> Perhaps the most intriguing variant of the evolvable hardware idea is one pursued by Adrian Thompson and his colleagues at the University of Sussex in the United Kingdom. Thompson thinks computer scientists are restricting the powers of evolution unnecessarily by putting it to work only on digital logic gates. "Silicon has a much richer dynamical behavior than just flicking between ones and zeros," he says. Chip components can adopt a whole range of values intermediate between the standard 1 and 0. . . .

The point here is that Thompson is making use of the fact that these FPGAs are able, in certain hard-to-find configurations, to program rules other than straight 0–1 logic. He proposes to use the evolutionary technique to get a chip to find a configuration that will allow it to do a simple nondigital task.

> So, rather than making their FPGAs follow the rules of digital design, says Thompson, "we don't tell evolution anything about how we expect it to work. We let it find the best way."
>
> Thompson's demonstration task was to feed an FPGA a single input—a 1-kilohertz or 10-kilohertz audio tone—and evolve it to generate a signal identifying the input: 1 volt for 1 kilohertz, 5 volts for 10 kilohertz. "This is actually quite a hard task," says Thompson. "The problem is the logic gates are incredibly fast. They respond on a time scale of 2 or 3 nanoseconds. The input is orders of magnitude slower. So we were asking incredibly fast components to do something much slower and produce very nice, steady output," he says. "All evolution had [for the task] was this little bit of silicon on a chip. . . . It had to use the natural physical properties to get the job done."
>
> The system that resulted from the evolutionary process worked efficiently, says Thompson, *but he didn't have the "faintest idea how it worked"* [my italics]. Backengineering failed to decipher it, but what he did learn was that the circuit seemed to be relying on only 32 of the 100 available logic gates to achieve its task, and some of those working gates were not connected to the rest by their normal wiring. "It was either electromagnetic coupling," Thompson explains, "which is basically radio waves between components sitting right next to each other, or they were somehow interacting through the power-supply wiring."

What has Thompson done here? He has taken a relatively simple device, a 100-gate FPGA, and through an evolutionary process caused it to learn a relatively simple task: to discriminate between two input frequencies. The machinery looks simple and the behavior corresponds to a very simple model. *But the physical details—how the system is actually doing the job—are deeply obscure.* This is not an isolated example. Workers on complex systems, artificial as well as natural, increasingly find that these systems may behave in orderly and understandable ways even though the details of how they do it are incomprehensible.[2]

My question is *Why should the brain be any different?* It, too, has evolved, and continues to rewire itself during learning in a process that closely parallels evolution.[3] So why on earth should we expect "back-engineering"—purely neurobiological investigation of the physical machinery—to yield a coherent account anytime soon? On the other hand, if the *selection pressures* are consistent (as in the FPGA example), we might well expect that the behavior of the system, its function, will in fact be understandable at the level of a simple model. So, I contend, if you want to understand behavior, begin by trying to understand its rules of operation.

Complexity

Complexity of course poses problems by itself. The most complicated objects currently made by human beings are computer chips. As I write, the Intel Corporation introduced a new chip with 3.4 million "gates" (transistors). By the time you read this, the latest chip will have more than 40 million gates. The connections among all these gates are of course perfectly well defined and each gate is (supposed to be[4]) completely independent of all the others—there are no "global" factors, like circulating hormones, that allow events in one part of the chip to affect other parts in a graded way. The only effects are through well-defined wires. Even so, the task of "reverse-engineering" such a chip—figuring out from its input–output properties and from what can be seen of the internal wiring exactly how it works—is formidable. A few companies in fact make their living by doing just this, making "clones" of Intel microprocessors.[5] Nevertheless, even though the fundamental principles of chip design are well understood, even though these companies can take apart an indefinite number of Intel chips, and even though they can be absolutely certain that every Intel 80486 they buy is identical to every other, the "clone makers" do not always succeed. Only very recently have clones of Apple's computers begun to appear, for example. Reverse engineering is simple in principle, but often difficult in practice.

Reverse-Engineering the Brain

Reverse-engineering the brain is much more difficult than reverse-engineering even the most complicated computer chip. First, the brain has many more parts. The unit of brain function is the nerve cell or neuron (at least, we think so, but because of long-range effects, there may be no real functional unit). There are many different types of neurons, and there are also other types of cells in the brain, notably glial cells, that are thought to have subordinate functions.[6] There are about 10^{11} neurons in the human brain (for comparison, there are about 2.7×10^7 people in the United States and 1.4×10^6 gates in the 80486 chip). The number of glial cells is even larger.

Second, neurons are complex structures that have very many connections with other neurons—the average is about 1000 connections, according to a standard textbook on neuroscience (Kandel et al., 1991).

Each neuron is itself complicated: “The information carried by the neuron is encoded in electrical signals that travel along the axon and into the nerve terminal. At the synapse these signals are carried by one or more chemical messengers across the synaptic cleft. None of these chemical messengers carries unique information, like RNA or DNA. Indeed, some of them have several functions within cells as metabolites in other biochemical pathways. The corelease of several neuroactive substances from a presynaptic neuron and the concomitant presence of appropriate postsynaptic receptors permit an extraordinary combinatorial diversity of information transfer” (J. H. Schwartz in Kandel et al., 1991, p. 224).

Third, there are on the order of 10^{14} synaptic connections in the human brain. Most researchers assume that all changes in behavior caused by experience—including learning—are traceable in some way to changes in synaptic connectivity. Some of these changes are permanent; others are more or less transient. But there is as yet no guarantee that learning depends on synaptic changes alone.

Fourth, interactions between individual neurons are probably not the whole story of brain function. In addition to local effects, there are also diffuse, long-range effects in the brain. Neurochemicals, such as some neurotransmitters and hormones, can affect many synapses at once. Graded electrical potentials may also have long-range effects. The nonlocal effects of neurotransmitters are just beginning to be explored. The role of long-range effects in overall brain function is still largely unknown.

Fifth, glial cells do not participate directly in the electrical signaling process carried out by neurons, but they probably play some role. The task of building up a picture of how behavior is generated by the brain from ever deeper studies of local mechanisms—from channels to receptors to synapses to simple neural circuits to more complex circuits to the brain as a whole—will be monumental.

The fact of neural complexity implies that understanding the behavior of a whole organism by beginning with individual neural units (the so-called *bottom-up* approach) is likely to be a slow process. Some progress along these lines has been made through the study of very simple organisms with limited numbers of neurons, such as the sea slug

Aplysia and the nematode worm *Caenorhabditis elegans*. Considerable advances have also been made in studying particular parts, especially peripheral parts, of the brains of higher animals: sensory systems such as vision and audition, for example, and some motor systems. In higher organisms, progress has been slowest in understanding processes that depend strongly on *history*: processes like learning, where, by definition, behavior now may depend upon experiences long ago in an organism's past.

I believe that input–output modeling, the *top-down* approach, is a better strategy for understanding learned behavior. It may also be a necessary preliminary to understanding brain–behavior relations. Without cracking our Intel chip, we can learn a very great deal about how it works just by applying different input patterns and measuring the resulting output patterns. Once again, however, the clone maker has several advantages over the psychologist. Most important, he knows what to measure. In psychological terms, the electronic engineer begins with a pretty good idea of what constitutes stimulus and response: he knows that electrical impulses, particularly their spacing (not so much their amplitude), are all that is important. The psychologist is much less sure how to define his stimuli and responses. Is the "red light" the stimulus? Or light of a certain wavelength? (Color is not the same as wavelength.) Should the stimulus be defined in physical terms at all? Psychologists and philosophers of mind have puzzled over these issues for many years.

The psychologist also faces special problems posed by the fact that we are dealing with biological material. Biology implies *variability*—not in every variable (for example, body temperature varies little in endotherms, skilled movements are carried out with great precision, etc.) and not always, but under very many conditions. There are good evolutionary reasons for variability, as we will see later on. But variability means that brains, even the brains of identical twins, unlike computer chips are *not identical*. Even if the brains of twins *were* identical at some early point in their history, it is impossible to give two individuals—even individual rats, nematodes, or sea slugs, much less human beings—identical life experiences. Given different histories, even two initially identical brains will become slightly different—they will be "storing" different memories.[7] This difference, and what it implies—that experiments cannot always be replicated exactly—poses severe difficulties for a researcher who wants to understand how organisms are affected by different histories.

Repeatability

The chip engineer faces none of these problems. All 80486 chips are the same: every chip is an identical *replica* of every other, no problem with variability. And experience doesn't change a chip *irreversibly*. Although every chip has a memory, the memory is *volatile*: it requires electrical power to retain information.[8] If the power is cut off, the memory is reset to its initial state and all the stored information is lost. So any experiment with an individual chip can easily be repeated, either by resetting the memory of the chip and beginning again, or by taking another, identical, chip off the shelf. Repeatability is essential to the experimental method, and I will discuss two ways to ensure it in a moment. The point, for now, is that repeating an experiment on behavior is much trickier than repeating an experiment with dynamic-memory computer chips. The psychologist has only imperfect replicas (different organisms, even identical twins, are not really identical), and the effects of past experience cannot always be erased (animal memory, unlike computer memory, is not volatile).

How do experimenters deal with the problem of repeatability? There are two experimental methods, within-subject and between-subject. I devote the next section to a discussion of these methods at a fairly elementary level, even though most readers will already be familiar with them. Why take up space with boring methodological discussion? For two reasons, one general and the other specific to this book. The general reason is because methods tend to take on a life of their own, to be pursued not necessarily because they are the best approach but because they are the familiar, the accepted approach—and (in psychology) because they are suitable for the application of statistics. If experimental methods are not to be misused, it is essential to understand their limitations. The specific reason is my belief that the within-subject method is essential if our aim is to understand learning at the most fundamental level. But it must be accompanied, as in the past it has not been, by theoretical exploration.

Within-Subject and Between-Subject Experiments

The within-subject method is the method of physics, chemistry, physiology, and some varieties of experimental psychology.[9] The experimenter deals with a single piece of experimental material—a pendulum (say) for an early physicist, a couple of compounds for a chemist, a nerve fiber for a physiologist, a human being or a pigeon for a psychologist. Experiment involves repeated manipulations on the same subject matter: the pendulum is pushed, compounds are heated or mixed, the neuron is stimulated, the pigeon or human being is subjected to a series of training and testing procedures. Let's look first at a simple physics experiment, then at a psychology experiment, to expose the common features.

The Pendulum Experiment

First, the most difficult part: framing an experimental question that is both *answerable* and *interesting*. Finding a question that can be answered experimentally is much easier than finding an answerable question that is also interesting, that is, whose answer can lead to new developments in the field—not just dotting a few i's or crossing a few t's. We need to decide first what things we should measure and then what manipulations we should make. A pendulum (figure 1.1) has a length, a weight, and a distribution of weight (is it just a stick, or is most of the weight at the

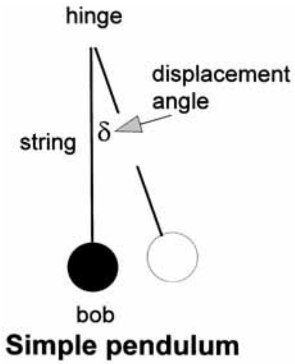

Figure 1.1
Simple pendulum.

end, in the bob?). It also has a color, a material composition, a hinge or string, a height above the ground, and numerous other properties that are not obviously related to its *behavior*, which is pretty simple: it swings back and forth with a certain *period* (time between swings) and it persists in swinging for a longer or a shorter time after an initial push (*decay time*). The thing we want to measure, the *dependent variable*, will be something to do with its swinging. The most comprehensive measure would be the entire pattern of movement in time, beginning with an initial push. But if decay time can be neglected, we can just measure the period, precisely defined as the time the pendulum takes to return to the same position after an initial displacement.

The next question is What should we *do* to the pendulum?—what manipulations should we make? Fortunately, pendulums have pretty simple behavior (that's why physics is farther ahead than psychology): about all you can do with a pendulum is push it or (which amounts to the same thing) displace it from the vertical by a certain angle. The angle of initial displacement is the *independent variable*, the thing that is manipulated by the experimenter.

Once we have decided what to measure and what to do, we can ask the critical question: Is the within-subject method indeed an appropriate way to study this system, as we have defined it (i.e., in terms of these dependent and independent variables)? The answer has to do with repeatability and a property called *history independence*:[10] Does the pendulum always behave the same way beginning with the same *initial conditions*? For example, suppose we always begin our experiments with the pendulum at rest. If we give it the same push (i.e., displace the bob through the same angle δ), does it always follow the same pattern of movement? If it does, if the behavior is repeatable, then we can ask some experimental questions. How does the decay time depend on the material of the string? Of the bob? How does period depend on the weight of the bob? On the length of the string? On the size of the initial displacement?

All experimental questions are not equal. Of these questions, only the last three turn out to be interesting—in the sense that they reveal the working of general physical laws. So let's manipulate three things: the bob weight, the string length, and the initial displacement, and see how

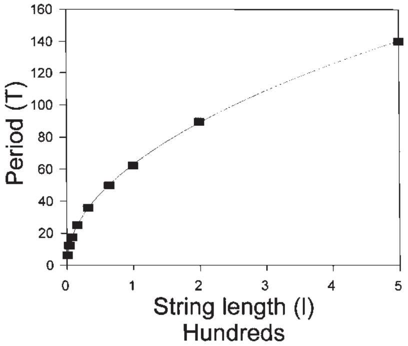

Figure 1.2
How the period of a pendulum depends on the string length. Filled squares: experimental data points; solid line: fitted equation (see text).

they affect the period. To study the effect of length, we begin with a given bob weight, say 100 gm, and vary the string length. At each string length, we displace the bob by the same fixed angle, δ, let it swing, and measure how long it takes for the bob to return to the starting point (i.e., the period). We do this for several lengths. The result of such an experiment will be a table of numbers showing how period depends on string length. The numbers from one such hypothetical experiment are graphed in figure 1.2 (the units on both axes are arbitrary).

The data are represented by the filled squares. They seem to follow a smooth curve, and I have fitted a simple power function to the points. It follows the equation $T = \sqrt{kl}$, where T is the period, l is string length, and k is a constant. There are in fact several other negatively accelerated (concave downward) functions that would fit these data almost equally well. But most of them are more complicated, and the square-root function is the correct one, because it alone can be deduced from the basic principles of Newtonian physics.

What about bob weight and initial displacement? The same experimental design will do for these also: settle on a string length, then try different bob weights and displacements, and look at the period. The result here is much simpler, though: bob weight has essentially no effect on period and neither does displacement, providing it is small. All these results—the independence of period and bob weight, the equivalence of all small displacements, and the square-root relation between period and string length—were deduced by Isaac Newton from his laws of motion.

What can we learn from this trivial example? (1) The within-subject method is appropriate when observations are repeatable: the same experimental conditions (string length, bob weight, displacement) always produce the same result (period). (2) Repeatability implies *history independence*: we always get the same input–output relation (length vs. period relation), no matter what lengths we try and no matter what, or how many, observations we make. (3) Any set of data can be fitted by several (actually, an infinite number of) mathematical functions. The decision among them must initially be made on grounds of simplicity (*parsimony*) and the degree to which each fits the data. But the final decision will depend on *theory*:[11] the best function is the one that both fits well and can be deduced from some more general theory that relates this result to others—as Newtonian physics relates the behavior of a pendulum to the motion of the planets and the trajectories of falling bodies.

This example illustrates the usual sequence in basic science, which involves several steps, almost none of which are *algorithmic* (i.e., reducible to a well-defined set of rules). The first step is usually *inductive*—find some empirical regularity. There are few rules for this. Charles Darwin believed that he set out "in true Baconian fashion" and let the facts speak for themselves. But as many philosophers have pointed out (and as Darwin himself admits, elsewhere), fact-seeking must usually be guided by some explicit or implicit notion of what is important—a tacit theory, at least. (On the other hand, the first inductive step in the pendulum example was the discovery of the principle of the pendulum, which is lost in prehistory and owes little to any preexisting theory, either tacit or explicit.) The inductive step in the pendulum example was to plot period versus string length. The next step is to see if there is some quantitative way to summarize this regularity. For the pendulum example, I did some curve-fitting, which these days *is* often algorithmic. There are numerous computer programs that will find the best-fitting function from a set of possibilities, for example. But this step still leaves you pretty close to the data.

The next step is the critical one, often omitted in psychology. It is to see if the regularity you have found can be generalized in some way. Is the square-root function for the pendulum an example of some more general principle? Charles Darwin (1809–1882) said, "The line of argument often pursued throughout my theory is to establish a point as a probability by induction and to apply it as hypotheses to other points and see whether it will solve them."[12] By "point" Darwin meant not the raw data, but a generalization based on the data, not "This bug has six legs" but the generalization "All insects have six legs."

The journey from empirical regularity (the square-root law) to (successful) theoretical generalization (Newton's laws of motion) is a difficult and mysterious one that is made relatively infrequently in science. Millions had seen the variety of nature, the perfections (and imperfections) of adaptation, and the prodigality of reproduction before Darwin connected them through the principle of natural selection. The potential generality of Gregor Mendel's (1822–1884) model for his data from breeding peas was not recognized until more than thirty years later, when "Mendelian genetics" was rediscovered by three other groups. In a criticism of Francis Bacon (1561–1626), who overemphasized the inductive step in the scientific sequence,[13] philosopher Bertrand Russell (1872–1970) commented:

> Bacon's inductive method is faulty through insufficient emphasis on hypothesis. He hoped that mere orderly arrangement of data would make the right hypothesis obvious, but this is seldom the case. As a rule, the framing of hypotheses is the most difficult part of scientific work, and the part where great ability is indispensable. So far, no method has been found which would make it possible to invent hypotheses by rule. Usually some hypothesis is a necessary preliminary to the collection of facts, since the selection of facts demands some way of determining relevance. Without something of this kind, the mere multiplicity of facts is baffling. (1946, pp. 556–557)

Russell was writing at a time when the research enterprise was a tiny fraction of its present size. His comment is even more true today than it was in the 1940s. Anyone who has tried to master the scientific literature in a growing area like neuroscience will emphatically agree with Russell that "the mere multiplicity of facts is baffling." Because U.S. science, in particular, has followed a largely Baconian road, facts tend to multiply unchecked. This causes little trouble in fields like physics and molecular biology, where the theoretical underpinnings are secure and constraining. But in psychobiology the bias for experimental facts over integrative theory has led to a tantalizing morass: thousands of meticulous and often ingenious experiments whose net contribution to human understanding is often considerably less than the sum of its parts.

The Reflex Experiment

Now for a psychology experiment. If you shine a bright light in someone's eye, the pupil constricts. This is known as the pupillary reflex. It helps control the intensity of light falling on the retina, just as the diaphragm of a camera controls the amount of light falling on the film. The aim[14] in both cases is the same: to keep the average light intensity within the optimal range for the sensitive medium—so that the film or the retinal photoreceptors are neither over- nor underexposed.

The pupillary reflex is an example of a *stimulus–response* relationship. The independent variable (the stimulus) is the intensity, wavelength, and duration of the stimulating light; the dependent variables (response properties) are the *latency* (i.e., time after the stimulus onset when some response can be detected), *duration*, and *magnitude* of the pupillary constriction.

If the reflex is elicited over and over again, the response will diminish—from fatigue or habituation. But after allowing a time for recovery, it will return to its original level. With this constraint, the pupillary reflex seems to be repeatable, just like the behavior of the pendulum. Hence, we can study it in a single subject. We might look at other subjects just to make sure that our subject is typical, but the properties of the reflex can be completely worked out with only one. Like the pendulum experiment, the data from this one can be summarized graphically, by a plot of response magnitude versus stimulus intensity, for example.

Irreversibility, Hysteresis, and Associative Learning

Now let's look at both experiments again, and note some things that might make the within-subject method less useful. Every child learns that one way to break a piece of wire is to bend it back and forth repeatedly. At first there is considerable resistance, but after a while resistance decreases and eventually the wire breaks. The effect is termed *metal fatigue*, and it reflects motion-induced changes in the crystalline composition of the metal.

Irreversibility Suppose that our pendulum bob hangs not from a piece of string but from a wire. As the pendulum swings, the wire is bent. What will be the effect of metal fatigue on the results of our experiment? It will show up first as a failure to replicate results. Because the wire is stiffer at the beginning of the experiment than at the end, after many bendings, the period will be shorter early on than later. Because the stiffness of the wire decreases with "experience," the pendulum will swing more freely later in the experiment. Consequently, a repeat of an experiment with the same wire will give a different period. Hence, an exact description of the results will have to include not just the current pendulum length but also information about the *history* of the system: How many swings has this particular wire undergone? How big were the excursions (the *amplitude* of the oscillations)? Over what period? And bob weight will no longer be irrelevant, because the heavier the bob, the more the wire will be bent, because the pendulum will go through more swings after a given displacement: the response will have a lower decay rate. Metal fatigue is an example of an *irreversible* effect: after *N* excursions, the wire will be in a unique state different from its *initial state*—and the initial state cannot be recovered.

The Between-Subject Method How can we study irreversible behavior like this? The between-subject method deals with the problem by using more than one experimental "subject." To study the effect of wire length on period, we will need several pieces of wire of the same "experience" (i.e., with the same fatigue history). Given these replicas, we can compare the periods associated with different lengths, knowing that the irrelevant historical factor does not contaminate the results. We have "controlled for" the effects of history.

Effects like fatigue are often quite *variable* (box 1.1), that is, two apparently identical pieces of wire with the same history of bending may nevertheless have different stiffnesses. If we cannot measure stiffness directly, the only way to do the pendulum experiment is to use more than

Box 1.1
Where Is the Variability?

The between-subject method is often thought to be theory-free, but the value of the results so obtained depends very much on what the underlying process really is and what the sources of error (usually termed *noise*) are. For example, suppose that the real relation between x (the independent variable) and y (the dependent variable) is a power function,

$$y = Ax^B, \tag{B1.1}$$

where A and B are parameters. Suppose that all the error in measurements of the dependent variable, y, comes from variation in parameter A. Will an arithmetic average value for y, when plotted against x, give an accurate picture of the underlying power function? A little thought shows that it will. For example, suppose that we make just two measurements of y (same x), and the two values for A on those occasions are A_1 and A_2, so that

$$y_1 = A_1x^B \text{ and } y_2 = A_2x^B. \tag{B1.2}$$

The average value for y is thus

$$\tfrac{1}{2}(y_1 + y_2) = \tfrac{1}{2}[A_1 + A_2]x^B, \tag{B1.3}$$

which is still a power function with the correct exponent, B, and a multiplier at the average of the actual values. Thus, in this case, the averaging process does not distort the true form of the underlying theoretical relation between x and y. The same is true if the noise is simply additive, that is,

$$y = Ax^B + \varepsilon, \tag{B1.4}$$

where ε is a random variable with mean 0: the relation between x and the average value for y will approach the true relation as sample size increases.

On the other hand, suppose all the error is in B. In this case, the measured relation will be

$$.5(y_1 + y_2) = .5A(x^{B_1} + x^{B_2}), \tag{B1.5}$$

which is not a simple power function. The problem can be solved for the power function by using the geometric mean, $\sqrt{y_1y_2}$, rather than the arithmetic mean, but the solution is not so simple for other theoretical relations or if there are several sources of variability. And this method is of no help if the theoretical function is unknown.

one piece of wire in each experimental condition. Thus, we might have 100 pieces of 20-cm wire, 100 40-cm pieces, and so on, and each set of 100 pieces will in turn be divided into 10 groups of 10, each with a different history. (Each of these groups of 10 is called a *sample*.) The data would then have to be averages of the period under each condition (i.e., each combination of length and history). Given a big enough sample, the group average will be *reliable* in the special sense that if we repeat the experiment with another sample of 10, we can expect to get about the same average—even though the individual pieces in each sample will all be different. This gain in reliability is a consequence of a fundamental statistical property known as the *central limit theorem*: if you generate numbers at random, the average of a sample of N numbers will (a) vary much less than the numbers themselves and (b) tend to a normal (bell-curve) distribution, no matter what the distribution of numbers themselves.

Using the Within-Subject Method with a Historical System The irreversible changes caused by bending make the wire pendulum a *historical* system, that is, a system whose behavior depends on its past experience. It has long been a convention in psychology that historical systems must be studied with between-group methods, as I just described. There is a cost to the between-group method, however. Having more subjects means more work, but the main problem is that the behavior of a group average need not accurately reflect the behavior of any individual in the group. The average American family has 2.4 children, but we would be alarmed to find a fractional child in any family. The average performance of a group of subjects learning some task always improves smoothly, but individual subjects may go from zero to perfect in one trial (see box 1.2).

The Theoretical Approach Fortunately, there is an alternative approach to the study of historical systems that preserves the advantages of the within-subject method. I'll call it the *theoretical approach* because it uses theoretical exploration to discover the causes of unrepeatability. The metal-fatigue problem provides a good illustration. Recall that the problem showed up first as a failure to replicate results: given the same

Box 1.2
Historical Note: Behavior Analysis and the Within-Subject Method

In the psychology of learning, the within-subject method is associated with the behavior-analysis movement begun by B. F. Skinner (1904–1990) and his students and associates in the 1950s (see, e.g., Ferster & Skinner, 1957; Sidman, 1960). Skinner discovered *reinforcement schedules*, procedures for arranging food reinforcement or electric shock punishment to be delivered contingent (dependent) on various properties of an animal's behavior: number or time of responses, rate of responding, etc. A striking feature of reinforcement schedules is that the pattern of behavior that animals develop after prolonged exposure—*steady-state* behavior—is more or less independent of the animal's previous history. Reinforcement schedules can therefore be studied using the within-subject method.

Nevertheless, simple tests show that an animal exposed to schedule B is not in the same state after the first exposure as after an intervening exposure to schedule A: the sequence BB does not leave the animal in the same state as the sequence AB, even though its behavior may look identical in both B conditions. The difference shows up (for example) if we add a third condition, C, which might be extinction (i.e., no reinforcement). By comparing different groups, it is possible to show that the animal will usually persist much longer in extinction in the B behavior following the BB sequence than following the AB sequence. In extinction following AB, the A behavior will after a while displace the B behavior. Thus the similarity of overt behavior in condition B in the AB and BB animals is misleading. They may look the same, but they aren't really the same, because of their different histories.

Once facts like this became widely known, the behavior-analysis movement had only two real choices: either go to the between-subject method, or retain the within-subject method and look for hidden theoretical variables to account for historical effects. The first method has been adopted by a large school of animal-learning researchers, primarily those interested in classical conditioning. But the behavior analysts stayed with the within-subject method. Unfortunately, they also accepted Skinner's curious view of theory, which caricatured its role in science. Laurence Smith summarizes Skinner's view as follows: "Skinner admits that 'theories are fun,' but he insists that the activity of conjecturing is less efficient than that of observing. Conjecturing appeals to the investigator 'whose curiosity about nature is not equal to his interest in the accuracy of his guesses.' Moreover, conjecturing is said to be 'wasteful,' to create 'a false sense of security,' and to lead to 'useless' and 'misdirected' experimentation, 'bootless theorizing,' and the necessary loss of 'much energy and skill' " (L. D. Smith, 1986, p. 272). Some of this is true, some is false, and all is misleading. For example, to oppose curiosity and conjecture is to oppose the end to the means. Curiosity prompts conjecture, and conjecture leads to experiments that may satisfy curiosity. Skinner might as well have written “Makeup appeals to women who value their appearance less than their experiments with cosmetics.”

Skinner’s view came to dominate, so that few behavior analysts were interested in hidden variables (L. C. Smith, 1986; Staddon, 1993a,b, 2000). Consequently, behavior analysis largely ignored historical effects. After the initial excitement produced by the novel technology of operant conditioning and the new findings it made possible, the field became largely static—because it had deprived itself of a major means of forward movement. It is only now beginning to get going again.

Why did Skinner (who was no fool and even did a little theorizing in his youth) eschew theory? Probably because his ultimate aims were meliorative rather than scientific. He was much more interested in controlling behavior than understanding it. Having found that schedules do indeed control behavior, the next step for Skinner was to apply them to human society, to make a better world—*Walden II* (Skinner, 1948). He would probably have agreed with the political economist who wrote that “Philosophers have only interpreted the world in various ways—the point is to change it!” (Karl Marx [1818–1883]).

pendulum and the same displacement, the measured period was different on the two occasions. What to do? We can simply accept the fact of unrepeatability and go over to the between-group method—or we can try to understand the reasons for the failure to replicate. Unfortunately, the theoretical method gives *no specific prescription* for what to do; it is *not algorithmic.*

One reason the between-group method is so popular is that it guarantees a result: numerous texts describe exactly how many subjects are needed and what statistical tests are to be used for every possible occasion, and computer programs provide painless calculation. If you follow the prescription, reliable (but not necessarily useful or meaningful) results are guaranteed. The theoretical method provides no such security. What it requires is simply a bright idea: knowing the problem area, knowing the properties of the material you’re dealing with; it is up to you, the scientist, to come up with a hypothesis about the causes of unrepeatability. The next step is to test that hypothesis directly. If your guess is wrong, you have to guess again.

The Theoretical Axiom The assumption behind the theoretical method is that a failure to get the same result when all measurable conditions are the same *implies the existence of a hidden variable* (or variables). We may think that conditions are the same for the second experiment as for the first, but if the the period we measure is not the same, then something we are *not* measuring must have changed. What that "something" is in the pendulum experiment is pretty obvious. It is the *stiffness* of the wire. The data suggest that stiffness changes with experience and affects the period. If you can figure out a way to measure stiffness, then you can go on to see how period depends on it. If the relation is 1 : 1, a given stiffness (and wire length) corresponds to a given period. Experimental repeatability is restored: given a certain pendulum length and a certain stiffness of the wire, the period is fixed. The system has been converted from a historical to an ahistorical one. And an ahistorical system can be studied using within-subject rather than between-subject methods, which are desirable for the reasons just given. (It is worth remembering that almost all the discoveries of physics have been made using the within-subject method and without the aid of statistics.)

Parameters and Variables The wire pendulum system is characterized by one *parameter* (wire length) and three *variables*: stiffness, the position of the bob, and the velocity of the bob. A parameter is just a variable that changes slowly enough to be neglected during an experiment—stiffness might be considered a parameter rather than a variable if the experiment is short or the wire is of a type that fatigues slowly. Variables are the things that define the *state* of a system. That is, if we know the values of the variables, future behavior of a (deterministic) system is perfectly determined. These terms will mean more after I have discussed more examples.

Notice the special advantage of the approach through theory: by trying to understand the causes of unrepeatability rather than just accepting it, something new has been learned about the nature of the system. The between-group method deals with variability and unrepeatability by brute force. And all it guarantees is that you have found out something about an "average subject." What you find may not be true of any individual. The within-group method deals with unrepeatability by seeking its cause, which usually means changing the experimental question. To be sure, there are many cases, particularly in applied areas, where we are not at liberty to switch questions.[15] In these cases, the between-group method is the only option. For example, given an incurable disease and two marginal[16] treatments for it, there is no short-term alternative to trying the treatments, plus a placebo "treatment," on three large groups of patients, in the hope of finding the better alternative. But we learn little about the disease from such an approach. Consequently, in basic research, where we have the leisure to choose our own questions, the theoretical approach is essential.

Hysteresis Metal fatigue is an irreversible change. Under some conditions, the pupillary reflex shows a less drastic kind of history dependence that allows for a mixed experimental strategy. Figure 1.3 shows the results of three hypothetical experiments. In all experiments, a brief pulse of light is presented to the eye (inset) and the response magnitude is measured (inset arrows). In a series of trials, the intensity of the pulse is progressively increased and then decreased: 10 increasing stimuli and then the same 10 in reverse (decreasing) order. The single line at the top is the experiment just described, in which the same stimulus always gives the same response. Trials (stimulus presentations) in this experiment were very far apart. Now look at the pair of light lines with open squares. This is the same procedure, but with trials a bit closer together. Notice the difference between the ascending and descending sequences. In the ascending sequence (upper line), as stimulus intensity increases, the response also increases, but at a slower and slower rate. But in the descending sequence (lower line), as stimulus intensity decreases, the response drops rapidly at first but then more slowly. The result is that a given stimulus intensity does not always produce the same response—a failure to replicate. The kind of unrepeatability represented by closed graphs like this is called *hysteresis*.[17]

The pair of heavy lines with filled squares is another repeat of the same experiment, but with an even shorter time between stimuli. Notice that the two curves are now farther apart and both turn down before the end of the stimulus series—a paradoxical result, because a stronger stimulus then gives a smaller response.

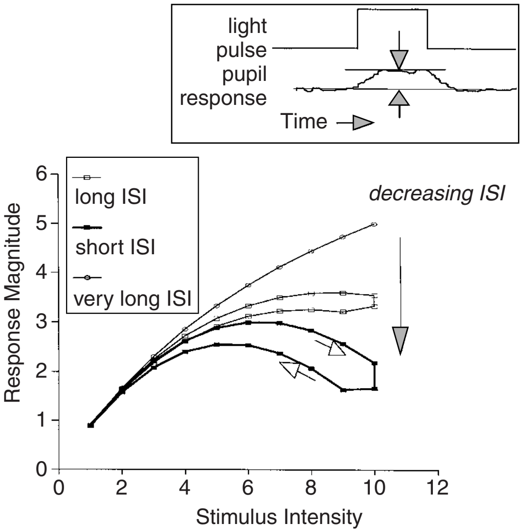

Figure 1.3
Results of three hypothetical experiments with the pupillary reflex. In all three experiments a series of stimuli is presented first in ascending (upper trace) and then descending (lower trace) order; the y-axis shows the response to each stimulus. Heavy line: short interstimulus interval (ISI). Open squares: longer ISI; open circles: very long ISI. The graphs were generated by assuming the following negatively accelerated relation between stimulus, x, and response, y, in the absence of “fatigue”:

$$y = Ax/(A + x).$$

To simulate fatigue, the “strength” of the reflex, parameter A, was allowed to change as a function of its previous value and the previous response, according to the difference equation

$$\Delta A = \alpha[1 - A(t)] - \beta y(t),$$

where t is the trial number, parameter α is directly related to the interstimulus interval, and β reflects the “fatiguing” effect of a given response. This model is a poor approximation to the behavior of a real reflex. It’s intended only to illustrate the concepts discussed in this chapter.

What might be the reason for the hysteresis here? Recall that many reflexes show reduced strength after they have been excited. I use the term *reflex strength* in a special sense here, to refer to the *relation* between stimulus and response (I'll give a more exact definition in chapter 5). For example, suppose we present a stimulus of a certain intensity and get a response of magnitude 100 (the units don't matter). Suppose we immediately present the stimulus again, and the response now is only 50. There are several possible reasons (fatigue, habituation, adaptation) for postresponse reduction in strength. I will get to them in later chapters. But for the moment, just notice that this reduction in strength often depends on interstimulus interval. The reflex has to "recover" after being stimulated. Consequently, if we wait a long time between stimuli, the reflex has fully recovered and the same stimulus always gives the same response. But if we wait a shorter time, it has not recovered completely and the strength is reduced.

It is this reduction in strength that produced the hysteresis in figure 1.3. At the end of the ascending sequence, the reflex is "fatigued" because it has been stimulated often, most recently with intense stimuli. Thus, when the descending sequence begins, reflex strength is reduced and the same stimulus elicits a weaker response, yielding the oval "hysteretic" pattern. The shorter the time between stimuli, the larger the effect and the fatter the graph. Thus, this complicated-looking graph is just a consequence of a simple effect: a transient reduction in strength of the reflex every time it is excited.

Hysteresis doesn't rule out the within-subject experimental method, but it does mean that we need to wait a while between trials to be sure that the reflex has been restored to its initial state. Many simple types of adaptive behavior retain the effects of a stimulus for a brief time in this way. The term *short-term memory* (STM) is often used to refer to this property.[18]

Associative Learning Today, a pupillary-response experiment would be carried out entirely automatically. A computer would turn the stimulus on and off at precisely programmed intervals. This is an improvement over the manual method because it gives the subject no warning. In earlier days, the stimulus would have to be turned on by the experimenter, who might be in the same room with the subject. The experimenter's activity could therefore provide the subject with a reliable warning that the stimulus was about to be turned on.

This relation between an irrelevant stimulus and something of importance to the organism may remind you of a more famous experiment from years past. The Russian physiologist Ivan Pavlov (1849–1936) encountered the unexpected results of just such a *pairing* of a "neutral" stimulus and a reflex stimulus in his experiments on the digestive function of dogs in the early 1900s. In Pavlov's work the neutral stimulus (now termed a *conditioned stimulus* or CS) was placing the experimental animal in a restraining harness. The reflex was salivation (termed the *unconditioned response*, UR), elicited by food powder (the *unconditioned stimulus*, US) placed in the animal's mouth. Pavlov noticed that his dogs very soon began to salivate in advance of the food, just in response to the preparations. He tested his conjecture that the pairing of the preparations and the food was responsible for this new learned "reflex" by explicitly pairing different neutral stimuli—a bell, the click of a metronome, and many others—with the delivery of food. He confirmed that the reliable CS–US sequence invariably caused a response (the *conditioned response*) to the CS that usually resembled the unconditioned response. This phenomenon is now known as *classical*, *Pavlovian*, or *respondent* conditioning.[19] The connection established by the Pavlovian procedure between the conditioned stimulus and the conditioned response is an example of *associative learning*.

In the pupillary reflex, if the unconditioned stimulus (the light pulse) is reliably signaled by some neutral stimulus (like setting up the stimulating apparatus or sounding a tone), after a few pairings the neutral stimulus will begin to elicit the response on its own. This acquired association between stimulus and response is associative learning, a type of change more persistent than hysteresis and different in other respects as well. Associative learning falls somewhere between hysteresis and irreversibility in terms of its persistence. There is no doubt that some associations leave no lasting trace whatever: a phone number learned and used once is soon forgotten, for example. On the other hand, many childhood experiences are never forgotten. In conditioning, even though the conditioned *response* can easily be eliminated through experimental *extinction*—repeatedly presenting the CS alone without the US—the subject is certainly not in the same state afterward as he or she was before. The conditioned response can be reacquired much more rapidly than it was learned the first time, for example. Associative learning often has irreversible effects and is usually studied by between-group methods. The approach through theory can also work, and I describe some examples in later chapters.

Summary

Complex systems that have evolved via natural or artificial selection may behave in a simple way, even though the physical details of their operation are obscure. It is likely that the neurophysiological basis for history-dependent behavior is extremely complex and may never be completely unraveled. Consequently, neurophysiology is unlikely to provide a short-cut to understanding learned behavior. History dependence implies the existence of hidden variables. Because the prospects for uncovering these through neurophysiology are not good, and because the methods of introspection are philosophically suspect and are in any case inapplicable to animals, the most promising approach is through theoretical exploration—the invention and testing of parsimonious black-box models. Within-subject experiments are best for this purpose. These ideas summarize the approach of *theoretical behaviorism*.

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