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The Principle of Parsimony and Some Applications in Psychology

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A modern principle of parsimony may be stated as follows: Where we have no reason to do otherwise and where two theories account for the same facts, we should prefer the one which is briefer, which makes assumptions with which we can easily dispense, which refers to observables, and which has the greatest possible generality. Psychologists often violate this principle, particularly in attributing complex behavior to cognitive processes. The practice is exemplified by recent accounts of chimpanzee behavior.

In this essay I first develop a modern variant of what has been called the "principle of parsimony" by commenting on a quotation on the nature of science by Ernst Mach. I then briefly trace the history of the concept in modern psychology and, subsequently, apply the concept to recent research with both chimpanzees and pigeons. No defense of the principle is offered, for, as I note below, I believe that no definitive defense is possible and acknowledge that the principle does not guarantee that a theory will be adequate or correct (cf. Barker, 1961; Goodman, 1972; Sober, 1981; Sober and Lewontin, 1982). I simply assume it, as did Ockham and others, as a first principle, one which, in the absense of arguments to the contrary, must always be applied.

A Principle of Parsimony

"Science," wrote Mach, "may be regarded as a minimal problem consisting of the completest presentment of the facts with the *least possible expenditure of thought*" (Mach, 1893/1960, p. 586; italics original). By his own definition the statement is not very scientific, for neither its meaning nor its implications for scientific practice are apparent. Scientists in most fields would agree that they strive to give "the completest presentation of the facts," but what does it mean

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to say that this should be done "with the least possible expenditure of thought"? Four possibilities suggest themselves.

Brevity. First—crudely substituting the word "speech" for "thought"—we might conclude that a good scientific theory is one of few words. The book of Genesis begins with a rather succinct account of creation: "In the beginning God created the Heaven and the Earth." No physicist can compete with such simplicity. One of several reasons why the Bible fails as a scientific theory is because of the first part of Mach's statement: Physical theories account for more facts about the universe as we know it than does the Bible. Brevity per se is not a criterion of good science. On the other hand, where two theories account for the same facts and where we have no other reason to prefer one over the other, we should probably prefer the briefer (cf. Goodman, 1961).

Assumptions. Second, perhaps Mach was talking about assumptions—statements the truth of which are taken for granted and which may otherwise be unsupported by fact. In modern science we often assume that the better theory is the one which makes fewer assumptions. But one must be cautious here, for the nature of the assumptions must be considered. The creationists have attacked evolutionary theory precisely on the grounds that evolutionary theory makes many assumptions (for example, about the validity of carbon-dating techniques or the significance of geological strata), whereas "creation science" makes only one (Gurin, 1981; Lewin, 1982). Many cognitive psychologists also make only one basic assumption to support elaborate theories of human cognition, namely that humans are "information processors"—that, like computers, we are instruction-driven symbol manipulators (e.g., Newell and Simon, 1972; Kosslyn, 1980; cf. Epstein, 1981, in press-a). Given this one apparently innocuous assumption, they claim special knowledge about the nature of human problem solving, memory, attention, and so on.

More critical than the number of assumptions is the scope of the assumptions; the more expansive each assumption, the more dependent and hence vulnerable the theory. An extremely expansive assumption can be a theory's *sine qua non.* If there is no Creator, creation science tumbles to the ground; whereas, if the carbon-dating technique is invalid, evolutionary theory continues to stand relatively unshaken on other facts and assumptions of paleontology, as well as those of genetics, geology, comparative zoology, and so on. By the same token, if humans are not really information processors—an assumption which is unsupported by fact—computer simulations of cognition may prove to be of little value (cf. Edelman, 1982; Epstein, in press-a; Neisser, 1976; Miller, 1981).

One might also consider the utility of the assumptions in other domains. An assumption that proves useful in more than one domain seems preferable to one which must be contrived for a single case. (See the discussion on "generality" below.)

One might be tempted to try to define scope and utility more precisely and then to try to delineate the trade-offs between scope, utility, and number. I will merely assert what I believe would be one practical outcome of such an analysis: Some theories can survive the loss of one or more assumptions more easily than other theories. *Ceteris paribus*, we should probably prefer the theory that is less dependent on its assumptions. Given two theories which depend on comparable assumptions and which are equally dependent on them, we should probably prefer the one with fewer assumptions.

Observables. Third, Mach may have been warning against descriptions or accounts of natural phenomena which appeal to unobservable entities (cf. Gooding, 1982). It takes little brainpower to see a chair, but one can not see the self-concept, an atom, or the ether. Indeed, one can do little with respect to such concepts but "expend thought." Simple concepts may require only a small expenditure. Someone may show you the Rutherford-Bohr model of the atom—the one that looks like a solar system—and ask you to imagine it much smaller. Complex unobservables, such as the "mental image" or Schrödinger's mathematical model of the atom, may require a great expenditure—so great that we resort to metaphors to characterize the concepts. Kosslyn (1980), for example, compares the mental image to a display on a CRT screen. The Schrödinger atom is perhaps beyond our ability to envision; it is usually represented in physics texts as a cloud of points, which hardly does it justice.

Mach was indeed skeptical about unobservables. He was, for example, reluctant to accept the utility of the concept of the atom. Shortly before his death, Einstein recalled trying, many years earlier, to convince Mach of the utility of atomic theory. He managed, finally, to get a concession: "[If] an atomic hypothesis would make it possible to connect by logic some observable properties which would remain unconnected without this hypothesis, then, Mach said, he would have to accept it. Under these circumstances it would be 'economical' . . ." (Cohen, 1955, p. 73).

Atomic theory won out, of course, over Mach's skepticism. Unobservables have proven themselves invaluable in modern physics. We cannot reject outright the use of unobservables in our theories; rather, we can assert, as did Mach to Einstein, that where we can account for as many facts with observables as with unobservables, we should probably prefer the former.

Generality. Fourth, Mach may have been saying that the principles we use to present our multitude of facts should be applicable to as many domains as possible. If one set of principles can account for both the Doppler shift and Brownian motion, or for both the diversity of species and the fossil record, we should prefer that to two separate sets that explain each phenomenon separately. The great drive in theoretical physics is toward a "unified field theory"—one theory that will account for known properties of the four basic forces in nature: the strong and weak forces of the atom, electromagnetic force, and gravitational force. One recent version, which characterizes the universe as an infinity of bubbles, has been praised on the grounds that it has the added merit of offering an account of the origin of matter and energy (Waldrop, 1982). The principles go farther.

My discussion of Mach's statement may be summarized as follows: Science may be regarded as a minimal problem consisting of the completest presentation of the facts in the briefest possible terms, which makes assumptions with which we can easily dispense, which refers to observables when observables will do, and which has the greatest possible generality. We now have a variant of what has been called the "principle of parsimony."

Ockham and Morgan

The first statement of such a principle is usually credited to William of Ockham, a fourteenth century English scholastic and philosopher, though the concept can be found in Aristotle and though, in Ockham's day, it was first stated by Duns Scottus (Boehner, 1957). Ockham proposed a rule of logic which has come to be called "Ockham's Razor." He stated it variously: "Plurality is not to be posited without necessity" (*Pluralitas non est ponenda sine necessitae*) or "What can be explained by the assumption of fewer things is vainly explained by the assumption of more things" (*Frustra fit per plura quod potest fieri per pauciora*).

Taken out of context, such statements seem to imply a strict rule of parsimony, consistent in part with the one I have developed above. But Ockham was first a man of religion, and he applied his logic only insofar as it was consistent with religious dogma. The "real meaning" of such statements, taken in context, is said by a noted scholar of Ockham to be as follows: "We are not allowed to affirm a statement to be true or to maintain that certain things exist, unless we are forced to do so either by its self-evidence or by revelation or by experience or by a logical deduction from either a revealed truth or by a proposition verified by observation" (Boehner, 1957, p. xx). If something were "proved by the authority of a holy scripture," other considerations would be ignored. Ockham's texts could not be used to defend the theory of evolution.

Psychologists, and particularly early behaviorists, were more directly influenced by C. Lloyd Morgan, Edward L. Thorndike, and Jacques Loeb. Morgan was a British psychologist and biologist who, in *An Introduction to Comparative Psychology*, published in 1894, challenged the tendency of some naturalists of his day to attribute human characteristics to animals. For example, George J. Romanes had argued, following Darwin, that "there must be a psychological, no less than a physiological, continuity extending the length and breadth of the animal kingdom" (Romanes, 1888, p. 10). Especially in cases in which we can show that an animal learns, he said, "we have the same right to predicate mind as existing in such an animal that we have to predicate it as existing in any human being other than ourselves" (Romanes, 1888, p. 7). Consciousness and mental states were, after all, only inferred in other people from their behavior. Given that there is continuity in nature, should we not give the same credit to animals?

Morgan was no less a mentalist than Romanes, but he took a more conservative stand. Just as evolution had produced organisms that varied from the simple to the complex, he argued, so must it have produced minds that varied from the simple to the complex. It would therefore be presumptuous of us to infer higher mental activities in animals where simpler ones would do. He expressed this position in his famous Canon, sometimes called the Canon of Parsimony: "*In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale*" (Morgan, 1894, p. 53, italics original).

Thorndike, who, while a graduate student at Harvard, apparently attended a lecture that Morgan gave there on the topic in 1896, bolstered Morgan's position by showing in his famous puzzle-box experiments that simple mechanistic laws of learning could account for some problem-solving behavior in animals (Thorndike, 1898, 1911). Thorndike was still a mentalist, but, as Skinner (1963) has pointed out, it was only a matter of time before Romanes' argument would be turned around completely. Jacques Loeb, for example, a German-born physiologist who was on the faculty for many years at the University of Chicago, argued that animal behavior consisted largely of tropisms, forced orienting movements determined by physical and chemical reactions. And Pavlov and several of his predecessors in Russia went so far as to characterize all animal behavior—including all human behavior—as reflexive.

As progress was made in explaining animal behavior with simple laws of conditioning, mentalistic accounts became less popular. It was inevitable that non-mentalistic accounts of human behavior would be proposed. The theory of evolution, which had been applied in one way by Romanes to justify the attribution of a mental life to animals and a second way by Morgan to warn against such attributions, could now be applied yet a third way: *Given that animal behavior could be accounted for by laws of conditioning and given that there is continuity in the animal kingdom (which includes Man), human behavior, like animal behavior, should be explainable without reference to mind.* Skinner (e.g., 1945, 1963, 1977) has defended this view on many occasions.

Parsimony in the Interpretation of Behavior

Behaviorism. The statement in italics above is the rationale for early behaviorism. Note that it contains three assertions, none of which is universally accepted by modern psychologists:

First, it implies that all animal behavior—or at least all animal behavior which would normally lead people to speak about the mind—can be accounted for in terms of conditioning. This was certainly not true in 1913 when behaviorism formally began and, to my knowledge, is still not true. Indeed, a variety of complex behavior in animals in general and in chimpanzees in particular has been said to defy conditioning accounts (e.g., Hulse, Fowler, and Honig, 1978; Köhler, 1925; Premack, 1983; Roitblat, 1982; Savage-Rumbaugh, Rumbaugh, Smith, and Lawson, 1980; Tolman, 1932).

Second, the statement implies a continuity theory which is, ironically, much closer to Romanes' than to Morgan's. Morgan had stressed that evolutionary theory predicted *differences* and *gradations* among traits, whereas both Romanes and the behaviorists insisted that species—or at least human and non-human animals—had a great deal in common. And the debate continues. Modern mentalists assert, as did Romanes, that the mental world is common to many species, or, as did Morgan, that perhaps only humans and a few close relatives possess higher mental processes—that evolution can create discontinuities. Gallup (1977), for example, supports his assertion that only the higher primates have certain cognitive capacities in part by citing biochemical data which show remarkable similarities between humans and chimpanzees. And behaviorists (e.g., Epstein, 1981, in press-a; Epstein, Kirshnit, Lanza, and Rubin, 1984; Epstein, Lanza, and Skinner, 1980) continue to assert with equal conviction that extrapolations from animal behavior are warranted.

Third and most important, the statement assumes the validity of some variant of the principle of parsimony. A non-mentalistic account of animal behavior is preferable to a mentalistic one presumably because it refers to observables and because it makes fewer and less critical assumptions (about the existence and nature of mind, for example). Note, however, that mentalistic accounts are brief and that they have great generality; their use in psychology has been defended on such grounds. Perhaps more commonly, though, many modern psychologists have simply rejected the principle of parsimony. For example, Gallup (1979) has noted that, unlike other animals, a chimpanzee that has been exposed to a mirror for a long period of time will come to treat its mirror image as an image of its own body. From this he infers the existence of a "cognitive entity" called the "self-concept," which he then proposes as the *explanation* for the chimpanzee's behavior. Though more parsimonious explanations would seem desirable and are indeed possible (Epstein, in press-b, in press-c; Epstein, Lanza, and Skinner, 1981), Gallup asserts, "As far as the self-concept is concerned, it would appear that on the morning before God created the great apes, maybe he became distracted by his own reflection in the mirror and forgot to shave with Occam's [sic] razor" (1977, p. 337).[1]

[1]Philosophers, too, sometimes reject the principle of parsimony as a criterion of good science. For example, Bunge (1961) notes "Simplicity is ambiguous as a term and double-edged as a prescription, and it must be controlled by the symptoms of truth rather than be regarded as a

Representation. Chimpanzee behavior in particular is often interpreted in terms of higher mental processes. For example, Savage-Rumbaugh et al. (1980) presented data said to show that chimpanzees are capable of a "representational symbolic function." They first established discriminations between three foods and three tools, photographs of those foods and tools, and symbols that the chimpanzees had learned to pair with those foods and tools. Two of the three chimpanzees they tested could then successfully categorize novel foods and tools, photographs of novel foods and tools, and symbols that they had learned to pair with novel foods and tools. In claiming that these results were possible only if the chimps were capable of "symbolic encoding" or "representation," the authors were saying, in effect, that the chimpanzee had to "think of" the referent of the symbol it was shown in order to categorize that symbol correctly as either a food or a tool.

I have argued (Epstein, 1982a; cf. Epstein, 1982b) that the training the chimpanzees had received should have produced reasonably good categorization of novel foods and tools or corresponding symbols given only rudimentary processes of conditioning. Before the reported tests, symbols for food had necessarily been paired with food more than symbols for tools had been paired with food. Through classical conditioning, symbols for food would come to elicit food-related responses, such as salivation. The discriminations that were subsequently trained and the subsequent categorization responses could have been based, then, on rather simple contingencies: Early in training, a chimpanzee earned reinforcement by placing into the "food" bin items in whose presence food-related responses were elicited and by placing into the "tool" bin items in whose presence food-related responses did not occur. Though undoubtedly not the whole story, this history could easily account for successful performances in subsequent tests, as well as for some of the reported errors.

My interpretation of the chimpanzee data is, I admit, unappealing, but that is beside the point. Sir William Hamilton (1859) wrote that the law of parsimony "forbids . . . above all, the postulation of an unknown force where a known impotence can account for the effect" (p. 395). Should we accept an account of the chimpanzees' behavior in terms of "symbolic encoding," "concept formation," and "representation" if a simple history of conditioning will suffice?

The kinds of variables I have specified are observable and, in this case, manipulatable. The processes have been shown to be relevant to an understanding of a wide range of behaviors and to many species. An explanation in terms

factor of truth . . . . Ockham's razor—like all razors—must be handled with care to prevent beheading science in the attempt to shave off some of its pilosities. In science, as in the barber shop, better alive and bearded than dead and cleanly shaven" (p. 149). Other philosphers assert parsimony as a fundamental of science (e.g., Goodman, 1972; Walsh, 1979; cf. Mach, 1893/1960), sometimes without defense.

of conditioning is simple and, in this case, testable in detail. A representation is, in contrast, an unobservable, and the inference of representation is just that—an inference.

Chimpanzees. The Savage-Rumbaugh et al. (1980) study was not the first in which chimpanzee behavior was unnecessarily overinterpreted. Researchers who have in recent years tried to teach human-like language to chimpanzees have been criticized by psycholinguists (Chomsky and Premack, 1979), behavioral psychologists (Terrace, Pettito, Sanders, and Bever, 1979), and ethologists (Sebeok and Umiker-Sebeok, 1980) alike. Terrace and his colleagues have pointed out, for example, that imitation and some simple principles of learning can account for much of the language-like behavior. And Epstein et al. (1980) showed that an exchange between two chimpanzees which had been unnecessarily attributed to the "information," "knowledge," and "intentions" of the chimpanzees could be closely approximated with two pigeons and could be accounted for in terms of simple conditioning procedures.

Overinterpretation in research with chimpanzees is ironic, considering the plight of modern cognitive psychology. Cognitivists generally have no interest in either the environmental or genetic origins of the behavior they study—in how, for example, language might have been learned. That is understandable, in a way. The origins of complex behavior are often complicated and, of course, lost in a subject's past. With chimpanzees, on the other hand, the antecedents—the training—the environmental histories—have usually been *programmed.* They are well known, and hence parsimonious accounts of the behavior can at least be attempted.

Tool use in the pigeon. Complex, human-like behavior in animals leads almost invariably to uneconomical, usually anthropomorphic, explanations. Consider the following example: A pigeon is placed in a large cylindrical chamber, about a yard in diameter and equipped with a standard feeder. A hexagonal box, about 6-inches in diameter and 1-inch high is on the floor in the center of the chamber. The pigeon ignores the box. At the base of a clear Plexigas wall is a small metal plate, about 1-inch square. The pigeon pecks repeatedly at this plate, and pecks are reinforced intermittently with food. Each peck operates a microswitch and thus produces a brief high-pitched tone. Over the course of a few sessions, the plate is moved back behind the wall a few inches. The bird can see it clearly through the Plexiglas wall. It continues to peck the plate repeatedly by stretching its neck beneath the 2-inch gap at the base of the wall.

Finally, the plate is moved back a full 6-and-a-half inches behind the wall—too far for the bird to reach. The bird has never been faced with this situation before. What does it do?

A normal 5-year-old child and at least one of Köhler's (1925) chimpanzees would probably, after a fashion, have "solved the problem." A young boy might perform as follows: He reaches repeatedly beneath the wall (or, say, the sofa) and grabs for the metal plate (or, say, the marble). He gives up, perhaps showing signs of frustration. He may have done this dozens of times before and given up each time. But this time he perseveres. He looks pensive, he looks around the room, and, finally, he reaches for a large object on the floor beside him (say, a magazine), and thrusts it under the couch toward the marble. After a few awkward thrusts, he hits the marble and perhaps thus moves it to a location he can reach. We might say that the child had spontaneously used a tool. Lay explanations would invoke the child's "intelligence," "knowledge," "expectations," "intentions," and "imagination." So would the explanations of many psychologists.

But the pigeon did the same things that our hypothetical child did. The pigeon first stretched repeatedly toward the metal plate. After about 30 seconds, it pecked weakly at the hexagonal box. It stretched again a few times toward the metal plate and then began, somehow, to look "frustrated" and "confused" and even "pensive." It pecked at the wall and the floor. It scraped its feet on the floor and rubbed up against the wall. It looked back and forth several times from the box to the plate. Suddenly, after a minute-and-a-half, it began to push the box directly toward the Plexiglas wall. When the box was under the wall, the pigeon lost control of it for a few seconds. It looked again at the plate, made some adjustments, and then pushed the box solidly against the plate and pecked it repeatedly, thus activating the high-pitched tone. It had, it seems, "spontaneously" used the box as an extension of its own beak to solve a simple problem (Epstein and Medalie, 1983).[2]

There are two disturbing things about this result: First, it would probably have been publishable without reporting the environmental history of the animal. We could have claimed ignorance—the tactic of many developmental psychologists—or disinterest—the tactic of many "cognitive scientists." We could have attributed the entire performance to "cognitions" and "intentions" (cf. Roitblat, 1982; Tolman, 1932). Second and far worse, we could have done what researchers who work with chimpanzees sometimes do: We could have briefly described the environmental history—at least summarized the training the animal had had recently—and *then* attributed the entire performance to "cognitions" and "intentions."

I have analyzed the behavior of the tool-using pigeon elsewhere (Epstein, in press-a; Epstein and Medalie, 1983) and here will merely state some critical facts and make what I hope is a tantalizing assertion: (a) The pigeon had recently had some experiences that were "relevant" to the solution to the problem—just as chimpanzees and children have had hundreds or thousands of such experiences before they are successful in similar situations. (b) The pigeon had never been confronted with this problem before, had never pushed things under a barrier, and had never pushed a box toward the metal

[2]Epstein and Medalie (1983) report the performance of only one pigeon. Similar performances have since been achieved with two others.

plate; its performance, in other words, was genuinely novel. (c) A moment-to-moment account of its behavior is possible in terms of its environmental history and some basic principles of behavior—and without any recourse to "unknown forces."

Caveats

I have not in this essay attempted to justify the principle of parsimony (though cf. Feuer, 1957; Goodman, 1972; Kordig, 1971; Mach, 1893/1960; Rolston, 1976; Russell, 1951; Walsh, 1979). In my opinion, no definitive justification can be made. The principle is itself, ironically, an assertion, one that pervades science but that remains, for the most part, unexamined by scientists themselves. Since it is a criterion by which a theory is judged to be better or worse than another, it may be little more than a value (cf. Goodman, 1972; Walsh, 1979). The principle probably evolved for reasons that are less grand than any post hoc justifications we might devise. As is true of other pre-scientific or scientific concepts (Epstein, 1982c), the principle may be little more than a reflection of our own limitations: As both theory and research grew more complex, simplification would surely have become a practical concern. The principle of parsimony may be nothing more than an instantiation of the principle of "least effort," and hence we might interpret Mach's "least possible expenditure of thought" literally (cf. Walsh, 1979).

The law of parsimony is accepted as an important criterion in science for judging the merits of a theory or an explanation, but it is not the only one. A theory that is parsimonious need not be "right"—which is to say, it may not be the most effective or useful description of the body of facts for which it is said to account (Barker, 1961; Bunge, 1961; Sober, 1981; Sober and Lewontin, 1982). There are even occasions upon which one can predict with confidence that a parsimonious theory is likely to be wrong. For example, Anderson (1978), a cognitive psychologist, has noted that the most "parsimonious" computer program will probably not be the best one to represent cognition. There is no criterion of parsimony in evolution; redundant and supernumerary organs and mechanisms abound in nature. And there is no reason to believe that human cognition—or its counterpart in the real world, the nervous system—has been spared nature's disinterest.

Should this weaken our faith in parsimony? I think not. We should recognize the limitations of the principle, but we would do ourselves an injustice if we did not admit how well the principle has served. For the principle of parsimony is, as Mach said, what science is all about.

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