PSYCHOLOGY REVIVALS

Cognition as Intuitive Statistics

Gerd Gigerenzer and
David J. Murray

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COGNITION *as* INTUITIVE STATISTICS

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The idea for the book was conceived when both authors were invited to participate in an interdisciplinary group that studied the "Probabilistic Revolution" in science. About 20 scientists, philosophers and historians of science spent a stimulating year (1982–83) at the Center for Interdisciplinary Research (ZiF) at the University of Bielefeld, West Germany. The organizer of the group and director of the ZiF at the time, Lorenz Krüger, now of the University of Göttingen, deserves our heartfelt appreciation for bringing researchers from different disciplines into a fruitful dialogue and for encouraging the present project throughout its planning. We are grateful to all members of the research group for the superfecundity of their ideas as expressed in countless discussions; their contributions have exerted a strong influence on the form this work has taken. In particular, we wish to thank Wolfgang Prinz, of the University of Bielefeld, who took an active part in the group and in the inception of this book. We are grateful to Kurt Danziger, York University, Wolfgang Hell, University of Konstanz, and William Hockley, University of Toronto, for critically reading portions of the manuscript. A special word of thanks goes to Lorraine Daston, Princeton University, whose advice on conceptual and historical matters and assistance with clarification at all stages of the writing we particularly appreciate.

The book was written after the year at Bielefeld, and collaborating across the Atlantic Ocean, between Queen's University in Ontario and the University of Konstanz was of course no easy matter. But Queen's University supported a visit by Gerd Gigerenzer to Canada; and a Leave Fellowship from the Social Sciences and Humanities Research Council of Canada, and the ZiF, each supported a visit by David Murray to Germany. We are also grateful to the ZiF for subsidizing the publication of the work. We should both like to thank Günther Fäustle, Thomas Finger, Hilde Manea, and Regina Passauer at the University of Konstanz, and Maureen Freedman at Queen's University for their assistance with the preparation for the manuscript; Maureen Freedman was supported by Operating Grant A8505 from the Natural Sciences and Engineering Research Council of Canada. We are grateful to Helen Tanner and the University of Michigan for permission to reproduce Figs. 2-4, 2-5 and 2-6.

Although the work is a cooperative effort, Gerd Gigerenzer is chiefly responsible for Chapters 1, 3, and 5, and David Murray for Chapters 2 and 4.

Finally, a word of appreciation is due our two universities, the University of Konstanz, and Queen's University at Kingston, for their support and provision of facilities. Both lie at the edge of large and scenic lakes, and one of the most attractive aspects of our endeavour has been the opportunity to appreciate the settings and academic traditions of both.

Introduction
Two Revolutions—Cognitive and Probabilistic

The present book focuses on the intersection between two recent lines of thought. Both have been called "revolutions." The term "cognitive revolution" signifies the transition from understanding humans in terms of overt behavior to understanding them in terms of mental structures and processes. The term "probabilistic revolution" describes the transition from a deterministic understanding of science, in which uncertainty and variability were not permitted, to an understanding of science where probabilistic ideas became indispensable in theorizing. The cognitive revolution is narrower in scope, restricted mainly to psychology, and is a "re-volution" in the original sense of the word. About 1960, psychologists reverted, not always aware that they were doing so, to some of the research goals and programs of the late 19th century, which were concerned with the analysis of mental events. The probabilistic revolution (see Krüger, Daston, & Heidelberger, 1987; Krüger, Gigerenzer, & Morgan, 1987) differs from the cognitive revolution (see Gardner, 1985) in the broadness of its scope and in its genuine novelty. The term "cognitive" denotes the subject matter of a discipline and may include both global conceptions, such as G. A. Kelly's (1955) notion of "man as a scientist" who construes the world in terms of "personal constructs," and localized cognitive events such as the "mental rotation" of images (Shepard & Cooper, 1982). The term "probabilistic," however, refers to a formal calculus linked with the idea of uncertainty. It may thus be interpreted within the context of any discipline and in fact, probabilistic notions have changed the very way in which some disciplines view the world. A frequently cited dramatic example is the abandoning of the "Newtonian world-machine," with God as the master clockmaker who designed a universe so perfect that it could run indefinitely without need for divine tinkering. In this context, the complacent Victorian physicist was so confident that all fundamental laws of nature had been discovered that he believed there was nothing more to do than to measure the physical constants more and more accurately. Around 1920, this deterministic view was shattered and abandoned by the indeterministic world view of quantum physics.

An equally dramatic example of a "probabilistic revolution" that has changed our thinking about humankind and religion is found in evolutionary biology. The protest against Charles Darwin, which he himself anticipated with fear, was not so much because he promulgated the theory that species had evolved from others, but because he claimed that evolution did *not* unfold according to a predetermined plan. Evolution does not lead toward an "ideal" animal or human; it can go anywhere, without predetermined direction. Darwin illustrated this by the metaphor of the "tree of nature" that has *irregular branches*. An evolution of species that leads finally to God's plan of an ideal species—is predetermined in a theological sense—could have been more easily integrated with the religious beliefs of the time. An evolution, however, that is based on chance processes like mutations or environmental contingencies and whose actual course could not be predicted seemed hard to believe and tolerate.

Let us consider more precisely what we mean by "probabilistic revolution." A common meaning of the term is that, in the history of a discipline, there was a shift from *epistemic* to *ontic* interpretations of probabilistic ideas. By an epistemic interpretation, we mean the notion that probability theory must be incorporated into the discipline because there is error in our measurements or because we are ignorant of some of the variables affecting the data we are collecting; but any randomness observed is not in nature itself. By ontic probabilism, we refer to the notion that chance is an irreducible part of the natural phenomena we are investigating (e.g. spontaneous brain activity).

However, the distinction between an epistemic and ontic interpretation needs amplifying. There is at least one important third position, which we call for convenience the *pragmatic* interpretation. The distinction between epistemic and ontic seems to imply that the scientist is a realist: either he believes that only causes are in reality or that probabilities and chance processes are also real. Not all scientists, however, are concerned with pictures of reality: some are concerned in the first place with empirical prediction and conceptual integrity. It is for these reasons rather than for reasons of ontic realism that some scientists came to consider probabilistic ideas indispensable (Cartwright, 1987). Heisenberg (1927), for instance, stated that physics should only "describe formally the relations of perceptions," and "one might be led to the conjecture that under the perceptible statistical world there is hidden a 'real' world in which the causal law holds. But it seems to us that such speculation, we emphasize explicitly, are fruitless and meaningless" (p. 197).

We shall therefore adopt a definition of "probabilistic revolution" broader than a shift from an epistemic to an ontic view. The term "probabilistic revolution" will be used in the wider sense that probabilistic ideas have become central and indispensable for a science, on the level of either theory construction or method. The transition from an epistemic to an ontic interpretation thus becomes only a special case (Krüger, Gigerenzer, & Morgan, 1987).

In this broad sense, psychology has seen *two* such revolutions, each on a different level, one following the other. The first occurred between 1940 and 1955 at the level of method, not of theory construction, when inferential statistics became widely used and soon institutionalized as the single method of inference from data to hypothesis. These statistical methods were in fact a mixture of those of R. A. Fisher, on one hand, and Jerzy Neyman and Egon S. Pearson on the other, a mixture that none of these statisticians (certainly not Neyman and Pearson) would have approved. We call this first probabilistic revolution in psychology the "inference revolution." The second revolution occurred at the level of theory, when, beginning about 1955, statistical ideas entered cognitive psychology at the level of theory construction. From the intersection of the inference revolution and the rising cognitive revolution a new understanding of the mind emerged: The mind as an "intuitive statistician." It is this second revolution on which the book focuses: It treats the new view of cognitive processes as statistical inference and hypotheses testing. But we claim that the success of the second revolution relies heavily on that of the first and that the new methods of inference have been transplanted to serve as explanations for how many cognitive processes work. And this has brought to cognitive psychology both a unifying perspective and, also, certain blind spots inherent in these institutionalized statistical tools. Cognition as statistical inference—this is the topic of the present book.

In the first chapter, we discuss the rise of the inference revolution, which institutionalized those statistical tools that later became theories of cognitive processes. In each of the following four chapters we treat one major topic of cognitive psychology and show to what degree statistical concepts transformed our understanding of those topics. The topics are (a) detection and discrimination, the classical psychophysical problems; (b) perception, in particular the problem of how properties of objects are judged and classified; (c) memory, the problems of recognition and recall; and (d) thinking, in particular the problems of inductive reasoning and rationality.

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1 The Inference Revolution

FROM TOOLS TO THEORIES: SCIENTISTS' INSTRUMENTS AS METAPHORS OF MIND

Metaphors

Metaphors have played their role in the development of all sciences. Charles Darwin, for example, took at least two human activities as metaphors for natural selection, namely, "artificial selection" and "war" (Gruber, 1977). Psychological thinking has been shaped by many a metaphor. Consider the case of memory.

Possibly the oldest metaphors to be found in psychology are those of Plato, who, in the *Theaetetus* likened the impression of a "memory" on the "mind" to the impression of a seal or stylus on a wax tablet. In the *Meno* he also drew the analogy between the mind full of memories and an aviary full of flying birds: Trying to retrieve a memory is like trying to capture a bird in flight—one knows it is there, but it is not easily caught. Through the ages many other metaphors have been introduced for the understanding of memory. St. Augustine likened it to a storehouse, and the word "store" took firm root in the vocabulary of memory theory. More recent metaphors include analogies between memory and houses, gramophones, computer programs, libraries, tape recorders, holograms, and maps (Roediger, 1980).

What is a metaphor? First, it consists of a *subject* and a *modifier* (Beardsley, 1972). In the statement "man is a machine," "man" is the subject and "is a machine" is the modifier. Second, a metaphorical statement differs from a literal one ("man is a vertebrate") by virtue of a certain tension between subject and modifier. The "mind is a statistician" reflects such a tension. Third, in contrast to assertions that are merely odd, metaphorical assertions are intelligible and acceptable, even if somewhat deviant. In poems, another case where deviant discourse is important, strong use is made of metaphorical language, which may result in an effect of beauty, whereas in science it may result in new ways of thinking. Fourth, metaphors are not falsifiable. However, by narrowing the possible flow of connotations and associations with definitions and examples, a metaphor can be transformed into precise and testable statements. This transformation is conventionally called a *model* rather than a metaphor, since it has lost its vagueness, is elaborated in a certain way (there may be other elaborations leading to other models), and offers predictions. We may think of a model as a controlled and elaborated metaphor.

What is the use of a metaphor? Its use is to be found in the construction of theories, rather than in the way they are tested. This means a metaphor may stimulate new ways of looking at the subject matter and create new interpretations of it. A metaphor cannot give us new ideas about how to test theories, but there is a connection between metaphors and theory testing that, as far as we can see, is unique to psychology. This connection is the subject of this book: Statistical tools for testing hypotheses have been considered in a new light as theories of cognitive processes in themselves. Examples are Neyman and Pearson's statistical theory, which has become a theory of object detection, known as *signal detection theory* (see chapter 2), and R. A. Fisher's analysis of variance, which has become a theory of how causal attributions are made (see chapter 5). Both have stimulated immense amounts of research during recent decades.

The Evolution of Metaphors

Metaphors common in psychology have changed over time partly as a result of the invention of new machines such as the telegraph and telephone, which ultimately led to the analogy between a person and a communication system (Attneave, 1959). Since the middle of this century, the "evolution" of metaphors has tended to focus on the *tools* that the behavioural scientist himself uses. Two major tools have been considered as important candidates for analogies with cognitive processes: computers and statistics.

The invention of the computer had, among other effects, the consequence of giving the scientist the opportunity to describe processes in terms of programs, carry out involved calculations, and manipulate lists of data (files). Each of these three aspects has been used as a metaphor of cognitive functioning.

For instance, one metaphor connected cognitive processing with the flow charts useful for depicting the steps in a computer program. Broadbent (1958) produced the first modern flowchart of the organism: He argued that information was received at the sensory receptors in parallel and was then put into short-term memory, where selective attention operated to give certain items a particular degree of "processing." The processed information could either result in an overt response, be put into long-term memory, or be recycled into short-term memory for rehearsal or further cogitation. Moreover, the planning and execution of an act has been compared with the execution of a computer program. Various cognitive processes were reinterpreted as "searching" through lists or files, represented by flow charts containing steps at each of which a decision has to be made. Newell and Simon (1972) have exhaustively examined the question of how far human problem solving can be imitated by devising programs suitable for solving the same kinds of problem by computers.

This book, however, is not concerned with the computer metaphor, but with the second major tool that became a metaphor of mind, namely, statistics.

Statistical Tools as Cognitive Theories

Between 1940 and 1955 statistical theories became indispensable tools for making inferences from data to hypothesis in psychology. The general thesis of this book is that *scientists' tools, which are considered to be indispensable and prestigious, lend themselves to transformation into metaphors of mind.* We call this briefly the *tools-to-theories hypothesis.* In particular, we maintain that statistics and computers exemplify this hypothesis. We restrict the thesis to statistics and cognitive psychology only and are aware of the ambiguity inherent in the term "indispensable." However, in what follows we shall clarify the meaning of this term by showing how statistics became institutionalized in psychology.

EMERGENCE OF STATISTICAL INFERENCE

Statistical inference does not exhaust inference. From time immemorial not only scientists but persons from all walks of life have made inferences daily. Even after the introduction of statistical methods of inference many scientists—for example, physicists—have little or no recourse to them. In this section we discuss the inception of those major theories of statistical inference and hypothesis testing that have provided the armory for the inference revolution in psychology.

Neyman (1976) credits the mathematician and astronomer Pierre Laplace (1749–1827) with the first test of significance. In astronomy, the normal distribution was used as a model for errors of observation. The problem was, what to do with outlying observations, which the normal law makes highly improbable and which seem to be due to extraneous causes. Every experimenter knows this problem of outliers that seem to deviate too much from the others. Probabilistic criteria were developed for the *rejection of outlying observations* (Swijtink, 1987). When the probabilistic ideas of the astronomers were transferred by Adolphe Quetelet into the social sciences, an important shift in interpretation took place. Whereas the astronomers usually inferred from a discrepancy between model and data that the discordant *observations* had to be rejected, social scientists usually concluded instead that the *model* had to be rejected. We shall return to this shift later, in our discussion of Sir Ronald A. Fisher's statistical ideas.

In fact, the first significance test seems to have been published about 100 years before Laplace's in 1710 by John Arbuthnot. The form of Arbuthnot's argument is strikingly similar to modern null hypothesis testing. However, since the content is so foreign to our 20th century concerns, his memoir reveals the pitfalls of this form of statistical inference more clearly than more recent examples.

The First Test of a Null Hypothesis

Arbuthnot held that the external accidents to which males are subject are far more dangerous than those which befall females. In order to repair the resulting loss of males, "provident Nature, by the Disposal of its wise Creator, brings forth more Males than Females and that in almost constant proportion" (p. 188). Arbuthnot favored this hypothesis of an intervening God over the hypothesis of mere chance—in modern terms, the null hypothesis. (He understood mere chance as implying equal chances for both sexes.) His data were 82 years of birth records in London (1629–1710), in which in every year the number of male births exceeded the female births. He calculated the expectation (the concept of probability was not yet fully developed at the time) of this data given the chance hypothesis which is $(1/2)^{82}$. Because this expectation was astronomically small, he concluded "that it is Art, not Chance, that governs" (p. 189).

In a manner similar to that used in modern psychology, he thus rejected a null hypothesis in which he had never believed. Let us bypass the small errors in his argument (first, *any* data would have had the expectation $(1/2)^{82}$, even 41 female-predominant and 41 male-predominant years; second, a chance mechanism with a probability of 18/35 for male births would fit his data well), and turn immediately to his discussion:

> From hence it follows, that Polygamy is contrary to the Laws of Nature and Justice, and to the Propagation of the Human Race; for where Males and

> Females are in equal number, if one man takes Twenty Wives, Nineteen Men must live in Celibacy, which is repugnant to the Design of Nature; nor is it probable that Twenty Women will be so well impregnated by one Man as by Twenty. (p. 189).

The inference in this first test of significance (of course, Arbuthnot did not use the term) was from the data to the existence of divine tinkering and in this sense constituted a "proof" of the existence of an active God. The parallel to the modern use (and abuse) is striking. A common practice today—one which, as we shall soon see, is statistically unsound—is to infer from the rejection of a specified null hypothesis the validity of an unspecified alternative hypothesis and to infer from this, as Arbuthnot did, the existence of a causal mechanism responsible for that deviation from "chance." In modern terms, Arbuthnot's chance hypothesis is a point hypothesis, that is, it specifies an exact value, whereas his Wise Creator hypothesis covers all other probability values *except* that single one. This is the first example of *asymmetric* hypothesis testing we know about, and it amply indicates the problems arising when alternative hypotheses are not specified.

The first modern significance test was the chi-square method developed by Karl Pearson (1900). One of the first questions to which Pearson applied his test was whether the inference that an empirical distribution is a normal distribution can be justified. As a result, Pearson's belief in the normal law as a law of nature decreased considerably. In this example, an inference from data to a hypothesis is attempted. However, as the earlier example from astronomers showed, there are other types of inference, such as inference from hypothesis to data.

Let us turn to the origins of psychology's inferential statistics. We consider three widely divergent views about the nature of statistical inference; those of Bayes, Fisher, and Neyman and Pearson. As we show later, what is taught in psychology as "inferential statistics" is in fact none of these theories, but a hybrid of ideas stemming mostly from the latter two views sometimes supplemented by a Bayesian interpretation. We shall describe these ideas only insofar as they have been incorporated into psychology and transformed into metaphors of mind. In contrast to contemporary textbooks, we shall emphasize the *controversial nature* of these statistical theories of inference, and the nonexistence of an agreed upon solution for formal inference outside psychology. This contrasts starkly with the presentation of "inferential statistics" since the early 40s in psychology as *the* uncontroversial and objective technique of inductive inference, one that can be used mechanically.

There are two poles between which ideas about the nature of inductive inference can be located. One considers inference from data to hypothesis as an *informal* cognitive process, based on informed judgment, and therefore strongly *dependent on the content* of the problem and one's specific experience with that content. According to this view, inferences are not independent of the content of the problem. Therefore, it makes little sense to apply the same formal rule of inference to every problem mechanically. This nonformal view, for example, is maintained by physcists and other natural scientists, as opposed to most social scientists. The second view considers inference as a process that can be described by a single *formal* rule, which can be applied *independent of the specific content* investigated. Probability theory has been the single candidate for all such attempts to formalize inductive inference. Since probability theory was conceptualized only around 1660 (Hacking, 1975), the problem of formal inductive inference, or induction, is rather recent, and it seems to be the only major problem in philosophy that is of modern rather than ancient origin.

Bayes

It is not surprising that one of the first attempts to formalize inference came from Laplace. He proposed the following *rule of succession* (see Keynes, 1943, pp. 367–383):

$$p(H|\mathrm{n}) = (n + 1)/(n + 2), \tag{1.1}$$

where $p(H|n)$ is the *posterior probability* of the hypothesis H that an event x will happen if the event has been observed n times successively. For instance, if you were 20 years old, you would have observed approximately 7,300 times that the sun rises in the morning. According to the rule of succession, your probability for believing in a sunrise tomorrow should be roughly (7300 + 1)/(7300 + 2), that is it approaches certainty. If something new happens to you and you have no information about the frequency of that event, you should infer that the event will occur again with a probability $p(H|n = 1) = 2/3$. Thus, if you play a lottery (without knowing the chances) for the first time and win the first prize, you should believe that if you play again tomorrow, you will win the first prize again with a probability of 2/3. Similarly, if you perform a new experiment and obtain result x, then you should expect to find x in a replication with the probability 2/3.

The rule has been criticized since Laplace's time as making too much out of almost nothing (Keynes, 1943). The criticism exposes a fundamental problem associated with such simple formal rules of inference: the neglect of context and content. (The sun may appear to go on or cease rising, depending whether you are in Paris or in Greenland. The probability that the experimental result will be found again may depend heavily on the specific content being investigated.)

Nevertheless, not only Laplace but such well-known statisticians as Karl Pearson believed in the *rule of succession* as a valid formula for inferring the future from past experiences (Keynes, 1943, pp. 379–383). The rule had been mathematically derived by Laplace from *Bayes' theorem* (by assuming uniform prior probabilities, see below). Bayes' (1763) theorem, which is better known today than in its own time, can be written as follows:

$$p(H|D) = p(H)p(\mathrm{D}|H)/(p|D) \tag{1.2}$$

The probability $P(H|D)$ is called the *posterior probability,* that is, the probability of an hypothesis H given the data D. It is a function of the *prior probability* $p(H)$, that is the probability of H before the data has been obtained, and the ratio $p(D|H)/(p(D)$, which signifies the impact of the data. The more $p(D|H)$ (the likelihood of the data given the hypothesis H) differs from $p(D)$ (the probability of the data), the greater the impact of the data. Thus, Bayes' theorem states a rule for the revision of a prior probability into a posterior probability in the light of new data. (More technical details are given in chapter 5.)

The Reverend Thomas Bayes himself seemed less enthusiastic about his formula than the psychologists of inductive thinking (e.g. Kahneman, Slovic & Tversky, 1982; Nisbett & Ross, 1980), who equate Bayes' theorem with rational thinking (see chapter 5). Bayes did not even publish the rule, although he must have thought about it for a long time, since it was mentioned for the first time as early as 1749 by David Hartley (1749/1970), years before Bayes died in 1760.

Like Laplace's rule of succession, Bayes' theorem offers a formula for the revision of probabilities of hypotheses in the light of new data. The typical interpretation is in terms of subjective degrees of belief, or "subjective probabilities."

It is this subjective element that came under heavy attack by proponents of the so-called *frequency theory* of probability, such as Venn (1888) and von Mises (1957). Specifically, in the case of Bayes' theorem, the frequentists criticized the way in which the Bayesians determined the prior probability. Since in most applications there was no single way to determine the prior probability in terms of previous experience, Bayesians applied the *principle of indifference;* that is, they set the prior probabilities of all hypotheses to be equal. This means that if we have two hypotheses, then the probability $p(H)$ in equation (1.2) is equal to .5. The principle of indifference is subjective because it is a statement about our state of knowledge or ignorance rather than about the world. The frequentists, on the other hand, believed that the only legitimate interpretation of probabilities was in terms of empirical evidence, that is, in terms of observed, relative frequencies of events. Therefore, they were suspicious