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HUMAN INFERENCE:

strategies and shortcomings of social judgment

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preface

One of philosophy's oldest paradoxes is the apparent contradiction between the great triumphs and the dramatic failures of the human mind. The same organism that routinely solves inferential problems too subtle and complex for the mightiest computers often makes errors in the simplest of judgments about everyday events. The errors, moreover, often seem traceable to violations of the same inferential rules that underlie people's most impressive successes.

The paradox persists in contemporary psychological theory and research. In European psychology it is the work of Piaget and Freud that perhaps provides the best illustration. Piaget's elegant and provocative theories of cognitive development offer an account of the stages through which the initially helpless and ignorant human infant passes on its journey to become an adult. And what a piece of work is Piaget's adult—so impressively armed with problem-solving tools and insights into the social and physical world. In contrast, the adult human to which Freud introduced us is far less notable for

mature rationality and sophistication than for the vestiges of its infancy. In everyday thought and action, and in the greatest personal and social crises, Freud's adult is governed by unconscious strivings and by primitive, often irrational inferential processes. Can the Piagetian adult, the master of formal operations, be the same organism as the Freudian adult whose mental life and behavior are so compromised by the thought processes and unfulfilled needs of the infant?

The same paradox is apparent in contemporary North American social psychology. Many social psychologists are preoccupied with the irrational or destructive elements of social life—with intergroup conflict, with the deficiencies of the "group mind," with social apathy and pathology, with ethnic and racial prejudice, and with biased cognitive processes which reconcile dissonance between action and belief and which allow illusions of personal merit to be sustained in the face of manifestly contradictory facts. At the same time, another cadre of scientists explores the rational capacities and insights that allow people to decipher the meaning and the causes of social events and that bring coherence, harmony and predictability to social life. Again, the reconciliation seems difficult and tenuous. How can any creature skilled enough to build and maintain complex organizations, or sophisticated enough to appreciate the nuances of social intercourse, be foolish enough to mouth racist clichés or spill its lifeblood in pointless wars?

The position of this book is integrative; it maintains that people's inferential failures are cut from the same cloth as their inferential successes are. Specifically, we contend that people's inferential strategies are well adapted to deal with a wide range of problems, but that these same strategies become a liability when they are applied beyond that range, particularly when they are applied to problems requiring some appreciation of the normative principles that help to guide the professional scientist's more formal inferences. We should warn the reader that the following pages give relatively more attention to shortcomings, and less to triumphs, than any balanced survey of everyday judgments would justify. This emphasis on error follows from much the same premise that leads many of our colleagues to study perceptual illusions or thinking disorders—the belief that the nature of cognitive structures and processes can be revealed by the defects which they produce.

The human adult portrayed in this book does solve difficult problems, sometimes using deceptively simple strategies first learned in early childhood. At the same time, this human also makes egregious judgments and ill-considered decisions, harming himself and others through the misapplication of informal, but usually helpful strategies and through the inability or unwillingness to apply more formal inferential principles.

We believe this characterization will prove to be a less paradoxical figure than does the Piagetian portrait of a sophisticated paragon who nevertheless somehow lapses into specific errors inconsistent with his general capacities.

We also believe it is also less paradoxical than the Freudian portrait of a tormented creature whose errors are largely motivational, the product of buried needs, wishes, conflicts, traumas, and defense mechanisms. Finally, we believe our portrait of lay inference helps to tie together a considerable body of work in contemporary social psychology. Research and theory dealing with such classic social psychological phenomena as attitude change, social influence, and prejudice are presented along with more recent efforts of the attribution theorists and their kin in the "cognitive-social" area. Our intent is to show the continuity between those processes underlying intellectual and social achievements and those underlying judgmental errors and social conflicts.

The organization of the book merits a brief introduction. In the initial section (chapters 1 to 3), we introduce the reader to the analogy between the inferential tasks of the layperson or "intuitive scientist" and those of the formal scientist, an analogy that figures heavily in the plan of the book. We then discuss the simplistic but generally helpful cognitive strategies used by the intuitive scientist and show how their overapplication or misapplication can result in major judgmental or inferential errors. In the middle section (chapters 4 through 8), we deal with specific inferential tasks, from the initial collection and coding of the data, to the testing and revision of informal psychological theories, that people perform so imperfectly. Throughout this section we discuss the formal or "normative" rules of inference of which people sometimes seem ignorant, rules which they understand primarily in particular narrow and familiar contexts or which they understand only in the abstract, without really being able to apply them appropriately and broadly. In the final section (chapters 9 through 12), we first treat two specific issues in more detail: the special case of inferences about oneself and one's own behavior (chapter 9) and the general problem of distinguishing between motivationally based errors and intellectually based ones (chapter 10). Then, in the last two chapters, we cover the personal and social costs of human inferential shortcomings (and the factors limiting such costs), and the possibilities of improving people's inferential strategies.

acknowledgments

Our earliest and most abiding intellectual debt is owed to Stanley Schachter, who was our graduate advisor in Columbia's social psychology program. He showed us the continuity between everyday judgmental strategies and the formal rules of science. It was from him that we learned that when the social scientist is most serious and creative he thinks and talks like a layperson, using familiar intuitive strategies and focusing always on the phenomena of daily social existence. He made us understand that while the formal tools of the scientist are essential, they generally serve the goal of verifying insights, not of producing them.

Our second great debt is to Amos Tversky. It soon will be apparent to the reader that this book could not have been written without the foundations laid by Daniel Kahneman and Amos Tversky in their work on judgmental heuristics. That work alone would not have made this book possible. Our conversations with Tversky (and on a few delightful occasions with Kahneman, as well) were essential to helping us appreciate the implications of

their work for the immediate concerns of our own research and for the broader concerns of social psychology. In earlier drafts we tried to indicate the specific ideas whose origins could be traced to these informal conversations. But in order to spare the reader from seeing over and over again in each chapter the refrain, "We are indebted to Amos Tversky for this idea," we now simply acknowledge, with gratitude, the magnitude of our personal and professional debt.

Another important debt is to two philosophers, Alvin Goldman and Stephen Stich. Their vigorous criticism helped us to clarify our arguments and, even more importantly, helped us to recognize this book's relevance to traditional epistemological concerns.

Our colleague and academic editor, Walter Mischel, also merits special mention. His early encouragement, and his unfailingly helpful suggestions for revision, have well justified our intention to publish under his distinguished editorship in the Century Series.

Many other people—too many to acknowledge each without risking an embarrassing omission—have generously donated their time to commenting on earlier versions of the book or on our earlier research reports. Some of these informal reviewers, however, have exerted such an impact on so many chapters or saved us from such serious errors, that their help must be acknowledged publicly. This list of generous colleagues includes Robert Abelson, Teresa Amabile, Daryl Bem, Eugene Borgida, Philip Brickman, Gordon Bower, Robyn Dawes, Phoebe Ellsworth, Patricia Gurin, Tory Higgins, Dennis Jennings, Edward E. Jones, Harold Kelley, David Krantz, Ronald Lemley, Richard Lempert, Mark Lepper, Hazel Markus, Shelley Taylor, and Timothy Wilson. We also are deeply indebted to the dozens of fine graduate students and to our stimulating colleagues at Michigan and Stanford who have helped us over the last several years to formulate and clarify the ideas presented in this book. The authors' research reported in this book was greatly facilitated by generous research support from the National Science Foundation and the National Institute of Mental Health.

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part I

INTUITIVE STRATEGIES OF INFERENCE

1

an introduction to the intuitive scientist

> The logic of science is also that of business and life.
>
> *John Stuart Mill.*

This book is about human inference and human error. It portrays people as intuitive scientists who are gifted and generally successful, but whose attempts to understand, predict, and control events in the social sphere are seriously compromised by specific inferential shortcomings. In part, these shortcomings reflect people's failure to use the normative principles and inferential tools that guide formal scientific inquiry. They also reflect people's readiness to apply more simplistic inferential strategies beyond their appropriate limits.

The book will document both underutilization of normatively appropriate strategies and overutilization of more primitive intuitive strategies. Part 1 describes some of the intuitive strategies and illustrates their overuse. Part 2 draws an analogy between the inferential tasks of the formal scientist and those of the layperson and documents the layperson's errors made by failing to employ normatively correct strategies in daily life judgments. Part 3 ex-

amines the origins of inferential failings, the costs of these failings, and the possibility of improving people's inferential strategies.

THE FRAMEWORK OF THE BOOK

In many ways, this book represents a return to a currently unfashionable tradition in Western thought—the epistemological tradition of simultaneous concern with psychological description and with normative prescription. The early epistemologists, for example, Bacon, Descartes, Locke, and Spinoza, were as much psychologists as philosophers. Much of their work was concerned with describing, in the form of empirically testable propositions, the structures and processes of human thought. These descriptions then were used as departure points for criticism and for proposing more appropriate and effective habits of thought. This tradition of equal concern with description and prescription was continued in the work of such later philosophers as Hume, Kant, and Mill.

Around the turn of the century, philosophers began to abandon descriptive work, leaving this to the new discipline of psychology, which was developing empirical methods for describing thought processes. There was a new division of labor, in which psychologists focused on the essentially empirical task of description and philosophers focused on the normative tasks of criticism and prescription. The predominantly normative emphasis is already evident in the work of such early twentieth-century epistemologists as Russell, Carnap, and Popper.

Lately, some epistemologists, notably Alvin Goldman (1978, 1979), have begun to recognize that normative issues are illuminated by the recent descriptive work of cognitive psychologists and have devoted themselves to learning this work. The literature of cognitive psychology presents evidence of the sorts of mental processes that people use for particular problems and provides strong hints of the limits of human cognitive capacities. Goldman (1978) has argued that it makes little sense for philosophers to ignore such work and to propose cognitive procedures which people may be incapable of actually using, and he and other epistemologists have recognized that it is similarly pointless to criticize cognitive procedures that people do not actually use.

The concerns of this book are complementary. Recent descriptive work by cognitive psychologists and by social psychologists concerned with inference has unearthed cognitive procedures ranging from the dubiously effective to the patently inadequate. It makes little sense simply to describe such procedures and move on; they cry out for criticism. Indeed, some psychologists working in this tradition have assumed the critic's role. Beginning with classic work on decision making (Edwards 1954) and on clinical

versus statistical prediction by Meehl (1955), psychologists in the judgment and decision-making traditions have allowed their empirical work to be guided by normative models and have compared both the layperson and the scientist to formal canons of inference. Investigators working in this newer, critical tradition include Herbert Simon and his collaborators, Daniel Kahneman and Amos Tversky, and the Oregon Decision Research Group of Paul Slovic, Sarah Lichtenstein, Robin Dawes, and Baruch Fischhoff. This book grows out of this critical tradition and draws heavily on the ideas and research of these investigators.

An equally important influence on the book comes from the cognitive tradition within social psychology. This tradition was founded in America by the neo-Gestaltist Kurt Lewin. Lewin's chief concern was with the cognitive structures and processes that produce actions in the social sphere. His representation of these structures and processes was quasi-geometrical, stemming perhaps from Gestalt psychology's spatial perception concerns. Lewin's representational mode depicting the "life space" of the individual by goal regions, barriers, forces, and vectors did not prove useful to investigators working in the tradition he established and, for the most part, has been abandoned. It has been replaced by more purely verbal concepts and relations such as those proposed by Fritz Heider in his classic book *The Psychology of Interpersonal Relations*. Heider proposed a very small list of subject terms (for example, *person, other, life space*) and predicate relations (for example, *can, ought, tries, suffers, benefits, harms, causes, wants*) to describe the phenomenology of person perception and the processes of interpersonal relations. Heider also introduced the notion of causal attribution as a fundamental process underlying much of social perception and action. Harold Kelley (1967), building on Jones's and Davis's (1965) earlier efforts, formalized Heider's conceptions of causal inference and drew the parallel between the layperson's methods of analysis and the scientist's.

Kelley's formulation of attribution theory brought the study of causal inference to the center stage of social psychology. What Kelley proposed, essentially, is that the layperson makes causal inferences using criteria analogous to those used by the trained social scientist. The layperson notes the covariation between particular "effects" (social acts and outcomes) and potential "causes" (the presence or absence of specific actors and particular features of the situation). From such observations, the layperson arrives at roughly the same conclusions that the academically tutored scientist would reach through more formal statistical analyses and more rigorously applied logical principles. For example, Sally infers that the "cause" of Jane's enthusiastic recommendation of Chez Paris was the quality of the restaurant rather than Jane's undiscriminating palate, ulterior motives, or chance, to the extent that certain essentially statistical criteria are met: Specifically, the recommendation should show *consistency* (Jane makes the same recommendation on many

occasions and in many contexts), *distinctiveness* (Jane does not make similarly enthusiastic recommendations of all restaurants), and *consensus* (other patrons of Chez Paris seem to share Jane's enthusiasm).

The layperson, in traditional attribution theory, is capable of applying psychological, as well as logical and statistical insights. The layperson seems to know much about the situational pressures and constraints that influence human behavior, and causal attributions reflect such knowledge. Thus, a dollar given to a panhandler is apt to be attributed to the giver's generosity only to the extent that the situational context is free of the usual reasons for eliciting such gifts from "people in general." If the panhandler's demeanor suggested that a less generous "gift" might bring insults or physical abuse, or if the panhandler's plight seemed particularly sad, the intuitive psychologist would be far less likely to assume that the giver is particularly generous or to assign a critical causal role to such "generosity."

Kelley, in his 1967 article and in subsequent contributions (1972, 1973), cited many studies showing that lay causal attributions do correspond to normative standards of inference. Subsequent investigators (notably McArthur, 1972, 1976) also supported this view.

Kelley's formulation of attribution theory and his parallel between the inferential tasks of the layperson and those of the scientist provided the impetus for the present book. Each of us has done research on causal attribution processes. But through that work we have become increasingly more impressed with the evidence of people's departures from normative standards of inference and less impressed with the evidence of their adherence to them (Ross, 1977a). This concern has been augmented by our contact with researchers, most notably Amos Tversky and Daniel Kahneman, who are concerned with informal judgment. Their work has revealed profound, systematic, and fundamental errors in judgments and inferences. These errors are the major concerns of this book. We will consider the ways in which these errors deviate from normative standards of logical and scientific inference, the origins and underlying causes of these errors, and the implications of these errors both for social psychological theory and for the problems of everyday social life.

THE TOOLS OF THE INTUITIVE SCIENTIST

This book contends that the seeds of inferential failure are sown with the same implements that produce the intuitive scientist's more typical successes. These intuitive implements are of two broad types— "knowledge structures" which allow the individual to define and interpret the data of physical and

social life and "judgmental heuristics" which reduce complex inferential tasks to simple judgmental operations.

Few, if any, stimuli are approached for the first time by the adult. Instead, they are processed through preexisting systems of schematized and abstracted knowledge—beliefs, theories, propositions, and schemas. These knowledge structures label and categorize objects and events quickly and, for the most part, accurately. They also define a set of expectations about objects and events and suggest appropriate responses to them. A price is paid for this mental economy, however. The knowledge structures themselves are not infallible guides to the nature of physical or social reality. Some beliefs, theories, and schemas are relatively poor and inaccurate representations of the external world. More dangerous, objects and events are not always labeled accurately and sometimes are processed through entirely inappropriate knowledge structures. Without these structures stored in memory, life would be a buzzing confusion, but their clarity is helpful only in proportion to their validity and to the accuracy with which they are applied to the data at hand.

Besides knowledge structures, people also use a few simple judgmental heuristics. These cognitive strategies, or rules of thumb, are the layperson's tools for solving a variety of inferential tasks. The two heuristics on which Kahneman and Tversky focused are the representativeness heuristic and the availability heuristic.

The representativeness heuristic allows the individual to reduce many inferential tasks to what are essentially simple similarity judgments. An object is assigned to one conceptual category rather than to another according to the extent to which its principal features represent or resemble one category more than another. A botanist assigns a plant to one species rather than another by using this judgmental strategy. The plant is categorized as belonging to the species that its principal features most nearly resemble. The problem with the heuristic is that it is sometimes used as the only judgmental strategy when it cannot alone provide an accurate judgment. When the known features of an object cannot categorize it definitely, statistical considerations become important to correct categorization. In particular, the relative frequency of the categories in the population under consideration becomes the normatively appropriate guide to categorization to the extent that the known features of the object are ambiguous guides to categorization. The work of Kahneman and Tversky (1973) shows that people have little understanding of these statistical considerations or of how to combine the statistical considerations with representativeness considerations.

The availability heuristic is used when judging frequency, probability, and even causality. Objects or events are judged as frequent, probable, or causally efficacious to the extent that they are readily "available" in memory. The heuristic is a fallible guide for such judgments because many factors

besides actual frequency, statistical probability, or true causal efficacy affect the memorial availability of objects and events.

A final intuitive strategy involves weighting the relevance of one's data. We argue that people effectively assign inferential weight to physical and social data in proportion to the data's salience and vividness. Information is heeded, processed, stored, and retrieved in proportion to its sensory, cognitive, and affective salience. By default, more vivid information is more likely to enter inferential processes than is less vivid information. This strategy of evidential weighting is dangerous because the vividness of information is normally related only obliquely at best to its true value as evidence.

Thus, the layperson approaches the inferential tasks of everyday life armed with knowledge and with strategies that, for all their general helpfulness and efficiency, have many flaws. These flaws are compounded by the knowledge and the strategies that the layperson does *not* possess, or at any rate fails to use in many inferential tasks to which they are essential.

INFERENTIAL PROBLEMS AND THE FORMAL SCIENTIFIC REQUIREMENTS FOR THEIR SOLUTION

The formal inferential rule system followed by professional scientists is the standard against which the layperson is compared throughout this book. We believe that this comparison is not forced or artificial, nor is it entirely unfair to the layperson. Many of the inferential tasks that confront the layperson are directly analogous to those that confront the scientist. In addition, the layperson's inferential failings, we believe, often can be traced directly to ignorance of the formal scientific requirements for solving these inferential problems. It will be useful, therefore, to describe some of the inferential tasks confronting both the formal and the lay scientist.

The most basic inferential tasks are *descriptive*. The formal scientist must be able to characterize and describe accurately the individual *datum*—object, or event. When, as is usually the case, there is more than one object or event to be considered, the scientist must accurately characterize the *sample* of data, a task which entails several, mainly arithmetic operations. Often the sample of objects or events is treated as the evidential base for generalizing to some *population* of objects or events, a task which entails several statistical as well as arithmetic operations.

Many tasks require more than the mere description of single events (or samples or populations of a single type of event) and are concerned primarily with relationships between different types of events. The scientist often needs

to observe *covariation* between events and to measure the magnitude of the covariation in terms of some defined criterion. If possible, principles or theories capable of *causally explaining* the covariation are formulated. The observed covariation and the postulated causal explanations then are used as the basis for *predicting* future events. For the formal scientist, all three tasks are governed by fixed statistical and logical principles.

Finally, the formal scientist adheres to a set of general rules for *theory testing,* which operate at every stage of the inquiry. There are rules for testing whether a given characterization of the datum, sample, or population is correct, whether events covary as believed, whether causal explanations are adequate, and whether predictions are valid guides to future events.

We argue that, in the context of daily life, the intuitive scientist is prone to make certain errors in solving the analogous inferential problems. These errors do not result from any inability to comprehend the principles of scientific inference. Indeed, almost all of the most frequently violated principles can be described on some level of abstraction and can be illustrated in some particular context that would be readily understood and accepted by any bright adolescent. The intuitive scientist, moreover, *does* readily apply many of the principles under certain circumstances—for example, when an appropriate "problem-solving set" is established and when the data and problem format prompt appropriate strategies and discourage inappropriate ones. Conversely, even the formal scientist is susceptible to many of the failings we shall describe, in everyday life and occasionally even in scientific endeavors. We will preview the intuitive scientist's shortcomings, each of which will be examined in later chapters.

Description. In characterizing the single *datum*, the intuitive scientist often is misled by prior theories of the object or event in question. Preexisting knowledge structures influence unduly, and often without the individual's awareness, the characterization of a given event. In characterizing *samples,* the lay scientist usually is at the mercy of the sample of events that can be retrieved from memory. This would present little difficulty if the only determinant of the memorability of events was their relative frequency. This clearly is not the case, however, since salience, retrievability, and other factors unrelated to true frequency, often influence the make-up of the sample of events that the individual can recall. Such biases in the "read-out" will create distortions in characterizing the sample. When the individual attempts to generalize from the characteristics of the sample to those of the *population,* two considerations become paramount—the size of the sample and its freedom from bias. The evidence indicates, however, that people have little understanding of the importance of either consideration. They seem to have little conception of the relative stability to be expected of the characteristics of a large set of observations or, conversely, of the instability and unreliability of the characteristics displayed in a small set of observations. More seriously, people

seem to have little conception of the damage done by sample bias. They seem to be almost as willing to generalize from samples known to be biased in crucial parameters as to generalize from samples known to be unbiased.

Detection of covariation. There is no assumption as critical to contemporary attribution theory (or to any theory that assumes the layperson's general adequacy as an intuitive scientist) as the assumption that people can detect covariation among events, estimate its magnitude from some satisfactory metric, and draw appropriate inferences based on such estimates. There is mounting evidence that people are extremely poor at performing such covariation assessment tasks. In particular, it appears that a priori theories or expectations may be more important to the perception of covariation than are the actually observed data configurations. That is, if the layperson has a plausible theory that predicts covariation between two events, then a substantial degree of covariation will be perceived, even if it is present only to a very slight degree or even if it is totally absent. Conversely, even powerful empirical relationships are apt not to be detected or to be radically underestimated if the layperson is not led to expect such a covariation.

Causal inference. When people have managed a detect a pattern of covariation, they still may fail to analyze correctly the possible causes of the covariation. In some instances, strong prior theories of causality may override the implications of the covariation pattern. In other instances, peoples' inferences about causality appear almost arbitrary or capricious because they are so heavily influenced by the perceptual salience of particular causal candidates. In still other instances, causal "schemas" or analytic strategies appropriate in some domains may "intrude" into domains to which they are inappropriate, causing more appropriate and helpful schemas to be overlooked.

Prediction. Even when people have recognized the true degree of covariation in a data set and have made correct causal inferences about the basis for the covariation, they may make normatively inappropriate predictions about future cases. Most critically, people appear to have little concept of the relevance of population base rates in general and regression phenomena in particular. When required to predict the outcome of an event, people tend to ignore the base rates, or relative frequencies of the outcome possibilities. When dealing with parametric observations, people tend to make "nonregressive" predictions that would be justified only if the empirical covariation were far greater than is usually the case in the social domain.

Theory testing. People have few of the formal scientist's skeptical or disconfirmatory skills. Once formulated or adopted, theories and beliefs tend to persist, despite an array of evidence that should invalidate or even reverse them. When "testing" theories, the layperson seems to remember primarily confirmatory evidence and to ignore potentially disconfirmatory evidence. When confronted forcibly by disconfirmatory evidence, people appear to behave as if they believed that "the exception proves the rule."

JUDGMENT AND BEHAVIOR

Part 2 of the book documents errors in each of the judgmental domains just described. The reader not familiar with the research on judgment and inference might wonder whether people's *behavior* reflects the type of judgmental errors we discuss. We hasten to assure such readers that there is substantial evidence showing that behavior often reflects, indeed sometimes amplifies, these judgmental errors.

The attribution literature shows that people do not merely verbalize dubiously correct causal explanations but express them behaviorally both inside and outside the laboratory (when they often are unaware that they are subjects in an experiment). Behaviors which have been shown to reflect prior causal attributions include insomniacs' sleeplessness, stutterers' verbal productions, nursery-school children's willingness to cheat at games and college students' willingness to cheat on examinations, adults', adolescents', and children's time spent working on and playing at various activities, elementary school students' littering of school grounds, and housewives' willingness to place an ugly billboard advertising natural beauty on their lawns!

In the literature on judgment and decision making, the connection is even stronger since the judgments studied *are themselves* typically behaviors of very great importance: judgments by clinicians of the degree of patients' mental illness, judgments by social workers of a client's inclination toward child abuse, judgments by parole officers of convicts' likelihood of recidivism, judgments by faculty admissions committees of acceptance of applicants for graduate school, and judgments by stockbrokers of the advisability of purchasing particular stocks.

We do not emphasize these real-world, behavioral effects in our review of the literature because we believe the main points are not in dispute: People's judgments often affect their behavior, and both their judgments and their behavior reflect inferential errors outside the laboratory as well as in it.

We also say little about precisely *how* people's judgments affect their behavior. This is neither an oversight nor a deliberate choice. We simply acknowledge that we share our field's inability to bridge the gap between cognition and behavior, a gap that in our opinion is the most serious failing of modern cognitive psychology. The problem was perhaps first noted by E. R. Guthrie in his famous gibe at E. C. Tolman, who may be considered the founder of modern cognitive psychology. Professor Tolman's rat, Guthrie joked, can never reach the goal at the end of the maze but rather, is left at the start box—with its hypotheses, theories, and expectations—"buried deep in thought." Cognitive psychologists had made so little progress in treating behavioral matters that, decades later, Daryl Bem was obliged to admit that

cognitive theories, including his own self-perception theory, "remain mute about any phenomenon in which the noncognitive response classes play the dependent variable role; as dependent variables such response classes are extra-theoretical. . . . How do attributional models account for noncognitive response classes? They do not" (1974, p. 217).

There is surely no more important task for cognitive psychologists than to bring these noncognitive response classes under their theoretical umbrella—to provide a way of explaining the circumstances under which, the manner in which and the degree to which, particular judgments influence particular behavior. Had we ourselves been able to make even tentative inroads into these problems, we would have done so.

COGNITIONS HOT AND COLD

Since this book is about inferential errors, the authors are bound to be asked whether such errors are not also the products of people's *motivational* and *emotional* make-up. We should confess from the outset the prejudice in our viewpoint. We proceed from the working hypothesis that inferential and judgmental errors arise primarily from nonmotivational—perceptual and cognitive—sources. Such errors, we contend, are almost inevitable products of human information-processing strategies. In ordinary social experience, people often look for the wrong data, often see the wrong data, often retain the wrong data, often weight the data improperly, often fail to ask the correct questions of the data, and often make the wrong inferences on the basis of their understanding of the data. With so many errors on the cognitive side, it is often redundant and unparsimonious to look also for motivational errors. We argue that many phenomena generally regarded as motivational (for example, self-serving perceptions and attributions, ethnocentric beliefs, and many types of human conflict), can be understood better as products of relatively passionless information-processing errors than of deep-seated motivational forces.

We have no wish, however, to deny that motives and emotions can influence human inference or to assert that all human follies can be explained by program error. Indeed, the perceptual and cognitive biases that we have described often operate on social data biased already by motivational and emotional factors. One's choice of friends, residence, occupation, news media, topics for reading and conversation, and perhaps even the social theories and theorists to whom we expose ourselves, all show the impact of such factors. To the extent that motives dictate social behavior, which in turn influences one's samples of information about the world, there is ample opportunity for social inferences to be distorted by needs and wishes. Our working assumption, therefore, is simply that self-serving motivational factors need not be introduced to explain most of the fundamental inferential or judgmental biases discussed in this book. In fact, as we shall see, the erroneous judgments, predictions, and causal assessments reached by the intuitive psychologist—far from being self-serving—often *undermine* self-esteem and *limit* the individual's capacity to satisfy personal needs.

THE QUESTION OF NORMATIVENESS: THE SCIENTIST AS CRITIC

Since this book is about inferential error, the question bound to arise in the minds of many readers is just how one knows that certain inferences are "erroneous," or, to turn the question around, how one knows that a given inferential strategy is "correct" or normatively appropriate. Our answer to this question is straightforward: We follow conventional practice by using the term "normative" to describe the use of a rule when there is a consensus among formal scientists that the rule is appropriate for the particular problem (cf. Stich & Nisbett, 1979). Some of the problems that we discuss, however, are relatively novel, and such "consensus" as exists is confined to the authors and the few colleagues with whom we have discussed the problems. In some cases, the term "normative" means "what the authors (and at least some other people) take to be the appropriate strategy." In these cases, the judgment that a given strategy is normatively appropriate is a tentative one that ultimately must be upheld or reversed by the court of informed opinion.

We do not doubt that many of our own current judgments about what is normatively appropriate will be overturned. In writing the book, we have changed our minds about too many normative questions too often to be under any illusions of the durability of these judgments. Moreover, we have become increasingly aware of the difficulty of defining what is "normative" when one moves beyond the relatively simple question of how to solve correctly some particular problem. "Normatively appropriate" strategies for the solution of some problems are extremely time consuming and expensive. It may be clear what must be done if one wishes a correct answer to such problems, but sometimes it may be even clearer that the correct solution is not worth the effort. This gives rise to more important questions of normativeness which are not fundamentally empirical in nature: How much effort, for what kinds of problems, should be expended to obtain a correct solution?

We have become excited by such normative questions and are pleased that our book highlights them. We have not been able to make much progress toward their solution, however, and do not return to them until the final

chapters, in which they are discussed only briefly. It is our hope that others, particularly philosophers who are more comfortable with such questions, will be motivated to pursue them.

We wish to say something else about the role of scientist as critic. We know from past experience in presenting this material that some people are offended by the spectacle of their fellow human beings caught in inferential traps designed by smarter-than-thou psychologists. It may be helpful in forestalling this understandable response to emphasize that this book was written in a spirit of genuine humility. We have found that our richest source of data demonstrating human inferential failings comes not from the undergraduates in our experiments or classrooms but from ourselves and our friends, most of whom are trained social scientists. Consistent with this belief, we have mixed personal anecdotes with formal data throughout the book. For us, and we trust for the reader as well, these anecdotes often will prove more disconcerting than the formal empirical work.

A second reason for our humility is that we remain enormously impressed with the power and accomplishments of the human mind. In writing this book, we began to feel that workers in human inference are now in a position akin to that of investigators of human perception. Perception researchers have shown that in spite of, and largely because of, people's exquisite perceptual capacities, they are subject to certain perceptual illusions. No serious scientist, however, is led by such demonstrations to conclude that the perceptual system under study is inherently faulty. Similarly, we conclude from our own research that we are observing not an inherently faulty cognitive apparatus but rather, one that manifests certain explicable flaws. Indeed, in human inference as in perception, we suspect that many of people's failings will prove to be closely related to, or even an unavoidable cost of, their greatest strengths.

Our respect for intuitive inferences, however, should not obscure our conviction that there is considerable room for improvement. That improvement in inferential abilities can result from formal training, especially training in statistics, is readily documented. The general question of prophylaxis and cure for inferential shortcomings is the topic of our final chapter.

We wish, finally, to address a quite different kind of reader. In our experience, some people respond to the material in this book with the attitude "Well, I always knew most people were stupid, and this just proves it." We hope to show that this comforting attitude is not tenable. We hold a contrary view, expressed by Amos Tversky, that there is *no* inferential failure that can be demonstrated with untrained undergraduates that cannot also (at least with a little ingenuity) be demonstrated in somewhat more subtle form in the highly trained scientist. For any readers who may be inclined to exempt themselves from the inferential failings cited in this book, we present the following thought-experiment, which will serve as a touchstone for us at several points.

> Let us suppose that you wish to buy a new car and have decided that on grounds of economy and longevity you want to purchase one of those solid, stalwart, middle-class Swedish cars—either a Volvo or a Saab. As a prudent and sensible buyer, you go to *Consumer Reports,* which informs you that the consensus of their experts is that the Volvo is mechanically superior, and the consensus of the readership is that the Volvo has the better repair record. Armed with this information, you decide to go and strike a bargain with the Volvo dealer before the week is out. In the interim, however, you go to a cocktail party where you announce this intention to an acquaintance. He reacts with disbelief and alarm: "A Volvo! You've got to be kidding. My brother-in-law had a Volvo. First, that fancy fuel injection computer thing went out. 250 bucks. Next he started having trouble with the rear end. Had to replace it. Then the transmission and the clutch. Finally sold it in three years for junk." (Nisbett, Borgida, Crandall, & Reed 1976, p. 129)

The logical status of the cocktail party information is that the number of Volvo-owners has increased from several hundred to several hundred and one, and that the frequency-of-repair record perhaps should be changed by an iota on a few dimensions. We trust that readers will recognize that their own response to the encounter would not be so trivial a statistical adjustment or be the calm observation that "every now and again one does sample from the tails of the distribution." We hope that the following chapters offer several such glimpses of the shortcomings of human inference in everyday life, and we are confident that the reader's response to some of these portraits will be a sheepish grin.

SUMMARY

The book traces the source of many inferential errors to two tendencies: the overutilization of certain generally valid, intuitive, inferential strategies and the underutilization of certain formal, logical, and statistical, strategies. The intuitive strategies include the application of preexisting "knowledge structures"—schemas, beliefs, and theories—and the utilization of the "representativeness" and "availability" heuristics.

The tasks of lay inference are compared with the tasks of the formal scientist, including characterization of events, samples and populations; assessments of covariation, causal analysis, prediction, and theory testing. The layperson's characterizations of events often are unduly influenced by prior beliefs or knowledge structures. Characterization of samples is distorted by

the differential "availability" in experience and memory of various events. Characterization of the population is compromised by ignorance of statistical considerations, chiefly those of sample size and sample bias. Covariation assessment is overly influenced by prior theories of expected covariation and is insufficiently influenced by actual data configurations. Causal analysis suffers from a similar overutilization of prior theories and from overreliance on the sheer conspicuousness of potential causal candidates. People have little knowledge of the regression considerations underlying prediction tasks and substitute simple similarity or representativeness judgments. Finally, people have little appreciation of strategies for disconfirmation of theories and often persist in adhering to a theory when the number of exceptions to the theory exceeds the number of confirmations.

It is argued that people's behavior often reflects the errors apparent in their verbal judgments and that the erroneous judgments are best understood as cognitive failings rather than as motivational ones. The book raises, but makes little attempt to answer, important "normative" questions.

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judgmental heuristics and knowledge structures

> The most characteristic thing about mental life, over and beyond the fact that one apprehends the events of the world around one, is that one constantly goes beyond the information given.
>
> *Jerome Bruner.*

The perceiver, as Bruner (1957) recognized, is not simply a dutiful clerk who passively registers items of information. Rather, the perceiver is an active interpreter, one who resolves ambiguities, makes educated guesses about events that cannot be observed directly, and forms inferences about associations and causal relations. In this chapter we explore the strategies that permit and encourage the perceiver to "go beyond the information given," that is, to venture beyond the most immediate implications of the data. We sketch some of the "knowledge structures" applied to understanding the world. These range from broad propositional theories about people in general to more schematic representations of objects, events, and actors. These structures house the person's generic knowledge and preconceptions about the world and provide the basis for quick, coherent, but occasionally erroneous interpretations of new experience.

Before discussing these structures, we will introduce the reader to the "availability heuristic" and the "representativeness heuristic"—two simple