

ORDINARY PERSONOLOGY

DANIEL T. GILBERT, *Harvard University*

A wonderful fact to reflect upon, that every human creature is constituted to be that profound secret and mystery to every other.

—CHARLES DICKENS, *A Tale of Two Cities*

Social psychologists have studied person perception for more than half a century, and for more than half a century have bemoaned the deficiencies of the phrase. Many have offered alternatives—from *social perception* to *person cognition* to *connaissance d'autrui*—but none of these has promised to repay a change of habit with a significant increase in rhetorical force. So the phrase *person perception*, like the necktie and the talent show, has survived unchanged for generations, despite the fact that nobody much likes it. And there is much not to like. The phrase appears to describe both an activity (perception) and the object toward which that activity is directed (a person), but, the object is described inadequately and the activity incorrectly. Person perception is about persons, yes, and not about toasters, but what aspect of the person is being perceived—temperament, marital status, melting point, or suitability for barber college? Worse yet, person perception has little to do with perception per se. Social psychologists generally do not study the processes by which patterns of reflected light or vibrating air enable people to identify the extent and location of their neighbors. Rather, they study the thoughts, judgments, beliefs, and opinions that people form about each other. Calling this process perception is a bit like naming one's cat Dog. The family gets used to it eventually, but when guests arrive there is always a lot of explaining to do.

The writing of this chapter was supported by a Research Scientist Development Award (KO2-MH00939) from the National Institute of Mental Health. I thank Reuben Baron, David Funder, Tory Higgins, Dave Kenny, Len Newman, Eliot Smith, Shelley Taylor, Jim Uleman, Dan Wegner, and Leslie Zebrowitz for their comments on an earlier draft.

This is the fourth chapter on person perception to appear in as many editions of the *Handbook of Social Psychology*, and the first not to include all or part of that phrase in its title (see Bruner & Tagiuri, 1954; Ross & Fletcher, 1985; Tagiuri, 1967). Instead, this chapter uses the phrase *ordinary personology* to refer to the ways in which ordinary people come to know about each other's temporary states (such as emotions, intentions, and desires) and enduring dispositions (such as beliefs, traits, and abilities). Personology is the scientific study of the characteristics of individuals; thus "ordinary personology" refers to the processes by which ordinary people achieve this same end. The phrase has something to recommend it. First, it does not designate a particular psychological activity and thus serves as a general rubric for the perceptual, cognitive, and even behavioral processes that enable people to know each other. Second, the word "ordinary" distinguishes scientists from nonscientists without the somewhat pejorative connotations of words such as "naive," "amateur," or "mundane." Finally, the word "personology" refers precisely to the understanding (*logos*) of the attributes of individuals (*personae*), which is precisely what person perception is supposed to be all about.

BRUNSWICK'S CHILDREN: THE ROOTS OF ORDINARY PERSONOLOGY

By any name, ordinary personology has been a topic of concern to philosophers since Aristotle, and a topic of scientific inquiry since at least the late nineteenth century, when the indefatigable Charles Darwin (1872/1979) attempted to map the facial expressions that enable people to know what others are feeling. This inquiry piqued the in-

terest of psychologists who, just after the turn of the twentieth century, picked up approximately where Darwin had left off (see Boring & Titchener, 1923, Felekey, 1914; Langfeld, 1918). By 1954, Bruner and Tagiuri described "the recognition or identification of emotion in others" as one of the "two traditional areas of inquiry" that constituted the study of ordinary personology (p. 634). That area of inquiry has since developed into the more general study of nonverbal communication, which has become so specialized as to warrant its own treatment in this edition of the *Handbook* (see DePaulo & Friedman, 1998). The other area of inquiry, which Bruner and Tagiuri (1954) described as "the judgment or perception of personality" (p. 634), diverged and evolved quite separately from the first, and this chapter is the story of that evolution.

The Objective Approach

The phrase "person perception" may be unfortunate, but it is hardly accidental. Social psychologists initially treated the understanding of persons as a problem analogous to that of seeing them. Emotions—like meteors, mushrooms, and farm machinery—were objects that existed in space and time and hence could be apprehended correctly or incorrectly. If one wanted to know how well people apprehended such objects, one had to measure the object, measure the person's apprehension of the object, and subtract. Some people, some emotions, and some circumstances would surely produce greater discrepancies than others, and the psychologist's job was to measure these objective and subjective realities, compute the difference between them, and make a note of it. This approach was simmered to perfection by Brunswick's (1947) lens model, which suggested that physical objects (distal stimuli) have properties that manifest as visual information (media), that this information may or may not be received by a subject (proximal stimulus), and that if it is received, it may or may not be properly interpreted (percept). For example, a waiter may feel happy, which may lead to him to turn his mouth into a smile, which may be noticed by a customer, who may infer that the waiter is happy. When everything goes right, a waiter's experience of happiness becomes a customer's judgment of happiness and an emotion is correctly discerned. If not, the lens model provides a framework for understanding where things might have gone wrong. In short, there is real stuff out there, and if one wants to know how well people see it, then one need only measure the real stuff and the perceived stuff and compare the measurements. This approach seemed so sensible that psychologists used it to move beyond the perception of emotion and into the perception of more enduring and complex characteristics, such as personality traits and intelligence. By the early 1940s social psychology seemed to

be well on its way to discovering how accurately people read each other.

But the enthusiasm soon exceeded its warrant, and in the relative blink of an eye, the objective approach that had dominated the study of ordinary personology for nearly half a century fell into a disfavor from which it never quite recovered. Different writers emphasize different reasons for the dramatic shift in social psychology's approach, but most agree that the shift was tectonic. What went wrong with the objective approach? Four things, at least.

Inconclusive Results The objective approach spawned many studies, which unfortunately arrived at many conclusions. Perhaps because of the mélange of methods, the objective approach to ordinary personology did not produce a core collection of uncomplicated truths on which researchers could stake their graduate students' careers. Rather, it weakly suggested that some people were sometimes better than some others at judging people, but that this finding depended largely on how one measured the objective and subjective components. If anything became clear, it was that the ability to judge other people ("social sensitivity") was not an ability after all, but rather a collection of component skills that were conceptually independent. Cronbach (1958) noted that "the literature has broken out with a rash of results which are interesting, statistically significant, and exasperatingly inconsistent" (p. 353). Bruner and Tagiuri (1954) concluded somewhat more politely that "studies on the 'accuracy' of judging others have not progressed to a point at which firm substantive conclusions can be brought to bear upon a theory of judgment" (p. 646), and they suggested that psychologists should "look more carefully at the insights of the dramatists and poets—if only in the spirit of searching for ideas to test. The achievements of professional psychologists in this field provides little justification for creating barriers against 'outside ideas'" (p. 639). In other words, after half a century of scientific investigation, the best advice the field's leaders could offer was to reread Shakespeare. Perhaps social psychologists expected too much from their infant discipline, but the fact was that in its first fifty years, the objective approach yielded little in the way of new and sustainable facts.

Fuzzy Objects The objective approach required the measurement and comparison of two realities—one inside and one outside the subject. The first of these was reasonably easy to measure. Subjective reality is, after all, a fancy term for someone's opinion, and if Hardy declares that Laurel is an introvert then there is every reason to assume that the statement more or less reflects Hardy's view. Alas, as modern personality psychologists will attest, measuring Laurel's introversion is a somewhat more complicated matter. The objective approach worked splendidly in the areas

of psychology in which the object of perception was literally an object. For example, a line could be measured with a ruler, everyone could agree about the meaning of "inch," the line did not become grumpy when measured too often, and the ruler offered the same measurements each time it was applied. But personalities proved to be particularly ill-behaved objects that defied simple measurement and clear definition, and—just when they were finally measured and defined—had the unfortunate habit of changing. By 1968, Mischel suggested that broad, context-independent personality traits were difficult to measure because, in fact, they did not exist, and although personality psychologists responded with great vigor (and some venom), when the din subsided they found it was not a simple matter to prove Mischel wrong. Today, even those who believe in the reality of personality traits agree that "difficulties in developing a criterion measure are the most significant obstacle in accuracy research" (Kenny, 1994, p. 136). A second problem with the objective approach, then, was that the object was at best elusive.

Missing Theories Perhaps one of the reasons why the objective approach netted so little was that it was something of a hunt without a quarry. Young sciences often use experiments to develop theories, but history suggests that theories must soon thereafter be used to develop experiments or the young science does not become an old one. Simply "looking around carefully" is not a profitable strategy for very long, and the objective approach to ordinary personology never matured much beyond dust bowl empiricism. Although Brunswick's lens model provided a conceptual framework for organizing the findings, it left psychologists all too free to jump from question to question without theoretical guidance. Are women better judges of extroversion than men? What facial expression betokens anger? Is it easier to judge leadership ability from a photograph or from a voice recording? It seemed as though psychologists expected the scatterplot of their subjects' hits and misses to trace the outline of a theory—a tactic that is the intellectual equivalent of randomly bombing the ocean with the hope of divining an enemy's naval strategy. In both cases one ends up with little more than diminished morale and dead fish. By 1954, Bruner and Tagiuri's summary of the harvest constituted a list of contradictory findings from a literature that suffered from "an excess of empirical enthusiasm and a deficit of theoretical surmise" (p. 650). The contradictions were distressing, but not as distressing as the fact that they coalesced into nothing more clever than a list. As is said of bad restaurants, not only was the food awful, but there wasn't much of it.

Bad Math The objective approach might well have recovered from its measurement troubles, its paucity of theo-

ries, and its lack of clear conclusions. After all, most new sciences stumble around for a while before eventually finding their feet. These problems were not fatal for the objective approach, but they did lower its resistance and leave it susceptible to attack, which a renowned psychometrician generously provided. In a series of mathematical demonstrations that were not quite esoteric enough for the average psychologist to ignore, Cronbach suggested that whatever meager truths the objective approach had uncovered, it had uncovered incorrectly (Cronbach, 1955, 1958; Gage & Cronbach, 1955). Cronbach showed that merely subtracting a target person's true score from a judge's estimate of that score yielded a variety of arithmetical artifacts that could masquerade as psychological wisdom. One judge might score high in accuracy for all the right reasons and another for all the wrong reasons, and a simple difference score could never distinguish between the two (see Kenny & Albright, 1987). In short, Cronbach made it suddenly and painfully obvious that everyone had been going about the study of ordinary personology in the wrong way.

This methodological scolding was not the sole cause, nor probably the most important cause, of the collapse of the objective approach. But it was the coup de grâce. Recent historians have argued that as a direct result of Cronbach's critique, "in the mid-1950s, all of the interest and enthusiasm about accuracy research came to a crashing halt" (Kenny, 1994, p. 136). Those who lived through that period have agreed. "An important consequence of [these] papers on interpersonal judgment . . . was to make researchers very skittish about asking *any* questions that involved assessing judgmental accuracy" (Jones, 1990, p. 25). Yet Cronbach was not the exterminator that history remembers. The division of accuracy scores into four conceptual components gave psychologists a superior method for the measurement of accuracy that, with the benefit of hindsight, might have served as the seed for a theory of ordinary personology, thereby solving two problems at once. But instead of grabbing the helm, psychologists reacted to Cronbach's papers by crowding the exits, abandoning the methods and issues that had occupied them for decades. In retrospect, this reaction seems every bit as sensible as biologists abandoning their trade in response to the invention of the microscope. So why did it happen?

Although Cronbach's techniques required more mathematical sophistication than did the simple difference score on which psychologists had come to rely, the real causes of the great retreat seem to have had more to do with opportunity than with arithmetic. The field had been shaken by its failure to produce a substantive theory, a coherent set of observations, or even a satisfying method for measuring its object, and these weaknesses combined with Cronbach's critique to provide the push. But the real engine of change was in the pull. Rather than staying the course and curing

their ills, most social psychologists thought they saw a way to circumvent their troubles by taking an altogether different approach to ordinary personology—an approach that did not rely on measuring the difference between stuff in here and stuff out there. The old ship was leaking but not sinking, and the crew fled because they had already booked passage on a very different vessel—one that appeared sturdy, strong, and steaming toward the future.

The Logical Approach

There is something satisfying, albeit simplifying, about historical trends whose origins can be traced to a single meeting. Malta and Versailles come quickly to mind, as do initial encounters between Breuer and Freud, Watson and Crick, and Lennon and McCartney. It is probably no more fanciful to trace the shift in social psychology's approach to ordinary personology to a conference that was held at Harvard University on March 17, 1957 (see the proceedings in Tagiuri & Petrillo, 1958). On this and two subsequent days, many of the field's major players—Solomon Asch, Jerome Bruner, Lee Cronbach, Albert Hastorf, Fritz Heider, to name a few—assembled both to eulogize the past and to chart the future of an enterprise that was already beginning to vibrate with change. The pattern of those vibrations had been sensed a few years earlier by Bruner and Tagiuri (1954), who noted that in social psychology,

the trend appears to be in the direction of investigating what kinds of organized impressions are formed under varying conditions of cue, role, set and prior information. There appears to be a de-emphasis of interest in the nature of judgmental accuracy, and a renewed emphasis on the judging process, whether it produces correct or erroneous impressions. (p. 648)

In other words, Bruner and Tagiuri saw the field shifting toward questions of *how* and away from questions of *how well*.

No one now doubts that the field experienced a sea change, and Bruner and Tagiuri's characterization of that transformation is correct in its essentials and is accepted by most modern writers (e.g., Funder, 1995; Jones, 1985, 1990; Kenny, 1994; cf. Kruglanski, 1989). Nonetheless, the questions of *how* and *how well* a system operates are surely too interdependent to be so easily decoupled, and a close reading of the literature suggests that there was more to the transformation. With the subsequent advent of the information-processing metaphor, the field found a new way to talk about the processes by which judgments of people are made, and the availability of a powerful theoretical language led to a new emphasis on the processes of ordinary personology. But it did not do so at the expense of an emphasis on accuracy—in fact, errors and biases in ordinary

personology have been among the most intensely studied topics of the last twenty years (see Nisbett & Ross, 1980). Error is to accuracy as short is to tall, and mere semantics should not obscure the fact that in either case one is talking about height. Although social psychologists acquired a new interest in the processes by which people judge each other, they never lost their old interest in whether and when such judgments went right or wrong. Rather, as we shall see, they changed the way they defined right and wrong, and in so doing, they lost the objective approach.

A New Method The Harvard symposium marked the confluence of two intellectual streams that, when allowed to run together, created the rushing river that was to become the *logical approach*. The headwaters of the first stream were in Germany and ran directly through the person of Solomon Asch, who was interested in the problem of impression formation, or how people integrate separate pieces of information about another person's traits into a coherent view of that person. Asch had been weaned on the Gestalt psychology of Köhler and Wertheimer, and if Gestaltists were fixated on any one notion, it was that wholes are perceived as more than the mere sum of their parts. Asch recognized that a person's impression of another was a wonderful example of such an irreducible whole, and he set out to investigate the mental alchemy by which informational elements were combined into a whole impression. His sublimely simple method consisted of reading aloud strings of trait adjectives that described a hypothetical person and then asking subjects to form an impression of the person so described. In a typical study, Asch (1946) manipulated the order in which the adjectives were read such that some subjects formed an impression of a person who was "intelligent, industrious, impulsive, critical, stubborn, and envious" while others formed an impression of a person who was "envious, stubborn, critical, impulsive, industrious, and intelligent." Asch found that subjects who heard the first ordering liked the hypothetical person more than did subjects who heard the second, ostensibly because the early information served as a lens through which the subsequent information was filtered (cf. Anderson, 1974). Here, then, was the sleekest and simplest of all experiments delivering what social psychologists had so desperately yearned for and so clearly lacked—a rule, a principle, an actual law of ordinary personology: Early information structures later information. Bingo. And the law had been derived and demonstrated without the mismeasurement of a single fuzzy object or the miscalculation of a single difference score.

Although this and others of Asch's laws proved important and robust, his most enduring contribution lay in the methods rather than the results of his studies. Like Lewin, Asch believed that one could learn about ordinary personology by manipulating aspects of the information that sub-

jects received and then measuring the effects of those manipulations on the subject's impressions. But Asch's studies differed from Lewin's in two important ways. First, Asch's experiments suggested that psychologists need not worry about how to measure a personality trait or an emotion *really*, because one need not present subjects with real objects in order to learn something valuable about the process by which objects were judged.

We do not intend to imply that observations of actual persons would not involve other processes which we have failed to find under the present conditions. But we see no reason to doubt that the basic features we were able to observe are also present in the judgment of actual persons. (Asch, 1946, p. 283)

In other words, although the method did not reveal everything there was to know about ordinary personology, what it did reveal was meaningful. Real people were not required as experimental stimuli, because crisp, clear, hypothetical stand-ins would do just fine—better than fine, in fact, because all the factors that made real people such annoyingly complicated objects of judgment were eliminated when one used artificial proxies. What Ebbinghaus had done a century earlier for the study of memory, Asch did for (or, some would later say, to) the study of ordinary personology.

Second, because the stimuli in Asch's experiments were not real people with real traits and real emotions, there were no "right answers" for subjects to hit, miss, or approximate. Whereas "the approach of the more careful studies in this region has centered mainly on questions of validity in the final product of judgment," Asch's approach dealt "explicitly with the process of forming an impression" (Asch, 1946, p. 260). In Asch's paradigm there were various configurations of information—trait adjectives strung backward and forward and subdivided—and the experimenter's job was not to pronounce subjects' judgments of these configurations correct or incorrect, but to map the relations between the configurations and the judgments they induced and to formulate laws that explained those relations. Much more will be said about Asch's legacy later in this chapter; for now it is important only to note that in one fell swoop, Asch's method seemed to solve most of the problems that had bedeviled the objective approach—not by offering a *better* way to measure objects or calculate difference scores, but by offering a method that did not require either.

A New Standard The second stream of the logical approach also originated in Gestalt psychology and made sharp detours through phenomenology, functionalism, and Kansas, where another European refugee, Fritz Heider, was trying to tie person perception to object perception with the concept of phenomenal causality (*cf.* Michotte, 1963). Heider argued that the inferential processes that allow ordinary

people to comprehend the properties of objects by their motion and appearance are much the same as the inferential processes that enable people to understand the characteristics of people by their actions. When people see a bowling ball strike a pin they have a sense of how hard the ball must have been thrown, how far the pin is likely to roll, and so on. In other words, people have a naive physics, so to speak, that enables them to understand what "makes things go," and Heider suggested that people also have a naive psychology that enables them to understand what "makes people do" (Heider, 1944; Heider & Simmel, 1944). The behavior of a person was, according to Heider, like the motion of an object, and to understand either was to analyze the cause of its actions. Heider's goal was to describe the rules that underwrote such analyses. Unlike Asch, Heider could not be bothered with the experimental identification of ordinary personology's support beams and pillars. His aim was nothing less than the complete description of its architecture.

Heider had little use for experiments or data and preferred to reason from informal observation, personal experience, language, and literature. Whereas Asch had substituted hypothetical stimuli for real stimuli and, from the pattern of subjects' responses, attempted to induce the laws that governed the judgment of those stimuli, Heider eliminated the stimulus, the subject, and the data entirely. He approached his work as a functionalist (by framing the real-world problem that people had been designed by nature to solve) and as a phenomenologist (by thinking about it). People, Heider reasoned, need to predict and control the behavior of others, and to do so they must somehow draw inferences about the stable characteristics of others by observing what others do. Just as people follow rules when they construct a three-dimensional representation of the physical world from the two-dimensional pattern of light that falls on the retina, so too must they follow rules when they construct an understanding of another's character from observations of that person's bodily movements in space and in time. Heider's goal was to articulate these rules. For example, one rule suggests that if observers are to use an actor's performance (how fast he rows a boat) to estimate the actor's ability (his strength), they must first factor out extraneous environmental influences on that performance (a friendly tailwind or choppy water). Here again was a bona fide law of ordinary personology. But unlike Asch's laws, it did not describe what people in a laboratory had done. It described what people in Heider's head had done. It described what people *might* do, or *could* do, or (and this is the place to listen closely) *should* do if they wanted to solve the puzzle posed by the behavior of others. The people in Heider's head were right—not when their judgments of objects matched the objects themselves, but when they followed the rules.

A New Deal On three spring days in Cambridge in 1957, the excitement generated by Heider's and Asch's contribu-

tions was palpable. To a field that was starving for theory, Heider's insights were manna. To a field that had been wounded by its methodological naiveté, Asch's experiments were a balm. Together, they suggested that real objects such as emotions and personality traits need not be the standards against which personological judgments were measured, but rather that logical rules could serve as a "rational baseline" (Jones & McGillis, 1976, p. 404) to which people's judgments could be compared. And why not? If one wished to know how accurately a child could add two and two, one did not check the child's answer by stepping into the hallway, putting two pairs of apples in a box, shaking, and recounting the fruit. Rather, one compared the child's answer to what one knew it *should* be—to what the rules of arithmetic demanded it *must* be. It was patently obvious that people *should* take tailwinds and waves and broken oars into account if they want to know the true strength of a rower, and the only questions were: Do they? And if so, when? And if not, why not? The study of ordinary personology had been slowly devolving since Darwin, but now, with a new method in hand and a new approach in mind, social psychologists suddenly had a great deal of work to do. Heider encouraged them to articulate rules, Asch provided a way to determine whether those rules were being followed, and together they gave the study of ordinary personology a new purpose, a new direction, and a new destiny.

The logical approach meant that accuracy was no longer defined by the difference between a scientist's measurement of a stimulus and a subject's judgment of that stimulus. Instead, it was defined by the difference between what a subject did with a particular pattern of information and what a logical rule said she should have done. Although the logical approach has been challenged on many fronts (more about that later), it continues to dominate research on ordinary personology. Indeed, its core distinction has been so influential that one does little violence to the work of the past forty years by describing it in terms of two conceptually distinct enterprises: the theoretical development of systems of inferential rules (described in the next section) and the empirical study of rule-consistent and rule-inconsistent behavior (described in the two subsequent sections).

HEIDER'S CHILDREN: THE LOGIC OF ORDINARY PERSONOLOGY

The Naïve Psychology

Trying to describe Fritz Heider's influence on the development of the logical approach is a bit like trying to say something sensible about the importance of water: it is everywhere, in everything, and the world that one imagines without it is nothing like the world we know. Philosophers like to say that modern philosophy is merely a meditation

on Aristotle, and it is probably no more facile to describe social psychology's approach to ordinary personology as a series of theoretical extensions and experimental explorations of Heider's system of thinking. That system, which was described in his only book (Heider, 1958), can be characterized by five core ideas.

Heider's Core Ideas

Behaviors Express Invariances, but Not Simply Throughout his book, Heider pushed the analogy between person perception and object perception as far as it would go, and then some. Heider began by assuming that just as objects have enduring qualities that determine their appearances, so people have stable psychological characteristics that determine their behavior. Behavior is no more interesting in and of itself than, say, the font in which a word is printed because it is a consequence of something much deeper and more essential. Behavior is merely a "medium for the transmission of psychological characteristics" (Heider, 1958, p. 35), which, like the shape or color or mass of an object, are relatively enduring or invariant across time. Heider (1958) called these properties *dispositions*:

The term dispositional properties is applied to those properties that "dispose" objects and events to manifest themselves in certain ways under certain conditions. Dispositional properties are the invariances that make possible a more or less stable, predictable, and controllable world. They refer to the relatively unchanging structures and processes that characterize or underlie phenomena. (p. 80)

By definition, dispositional properties vary less than their manifestations. Although an egg has but one true shape and one true color, its outline changes from circular to elliptical and the appearance of its shell from white to gray as one views it from different perspectives and in different lights. Similarly, a person has but one true character but may behave differently in different instances, and these apparently disparate behaviors are to be understood as superficially distinct manifestations of one unseeable, characterological thing. According to Heider, there is a lawful relation between dispositional properties and behaviors, but not an isomorphic relation. Two dispositions (liking and dutifulness) may manifest as the same behavior (helping an elderly person across the street), and one disposition (liking) may manifest as two behaviors (helping and protecting). These facts (which Heider referred to as the problems of ambiguous and synonymous mediation; see Heider, 1958, p. 36) mean that although behaviors express dispositions, particular behaviors do not simply and necessarily express particular dispositions. In other words, the

link between what a person is and what a person appears to be is a complex one that can be penetrated only by a complex logic.

Attribution Extracts Invariances from Behavior For Heider, the most striking achievement of the visual system was its ability to provide a stable phenomenal world for the person who is bombarded by unstable patterns of sensory stimulation. As one circles an egg, the size and shape of the retinal images of the egg are constantly changing, yet one perceives the egg's shape as invariant. The magic of the visual system is that it somehow extracts the invariance from the variable series of retinal images—in a sense, it reconstitutes the actual invariances (such as true shape and true color) that exist in the environment. According to Heider, the attributional system enables people to understand other people in the same way. Both attributional and visual systems extract phenomenal invariance ("The egg looks round" or "Duane seems irritable") from variable manifestations (a series of retinal images or behaviors) of actual invariance (the egg is round and Duane is irritable). The word "extracts" is important here, and substituting the word "constructs" would mischaracterize Heider (see McArthur & Baron, 1983). Neither attributional nor visual systems typically make things up. Both systems enable observers to know the real stuff that is actually out there in the real world, even though observers do not have direct contact with that real stuff.

Attribution Is Vital One also fails to understand Heider if one fails to appreciate how vital he considered the extraction of invariance to be. Heider (1958) argued that diagnosing a person's dispositions "enables one to grasp an unlimited variety of behavioral manifestations by a single concept" (p. 30) and thus serves "to integrate a bewildering mass of data in the most economical terms" (p. 53). Indeed, an observer "grasps reality, and can predict and control it (only) by referring transient and variable behavior and events to relatively unchanging underlying conditions" (p. 79). Grasping reality is an important end for any organism to achieve, and thus the attributional system that reconstitutes the dispositions of other people is vital. Without such a system, the observer would be adrift in a sea of transient and variable behaviors that lacked meaning and cohesion.

To say that it would be confusing if the shape of objects were transformed with every positional shift, or if persons were perceived as changing character with every action . . . is but a great understatement of how much more disturbing the world would be. We need to perceive things and people with their invariant properties. (Heider, 1958, p. 53)

The need to extract dispositional invariances was, for Heider, as pressing as the need to eat or sleep.

Attributions Are Not Necessarily Conscious How is this extraction accomplished? Heider's defense of "commonsense psychology" is sometimes misunderstood as an attempt to elevate folk wisdom to the status of scientific principle. Commonsense psychology is not the study of what ordinary folk say about their own attempts to do personology, because, according to Heider, ordinary folk cannot say how they extract characterological invariances from behavior any more than they can explain how their eyes and brains extract physical invariances from a dance of retinal displays. Whatever rules the attributional and perceptual systems use, they tend to use unconsciously. People are often no more aware that they are extracting characterological invariances from a stream of behavior than they are aware that they are consolidating a flux of retinal images as they stroll around an egg. "These conclusions become the recorded reality for us, so much so that most typically they are not experienced as interpretations at all" (Heider, 1958, p. 82). In other words, attributions have a "given" quality, and although scientists recognize them as the accomplishment of a sophisticated inferential system, observers experience them as simple reflections of reality. Heider (1958) recognized that although "the ordinary person has a great and profound understanding of himself and of other people," that knowledge is generally "unformulated or only vaguely conceived" (p. 2). Because people can do wondrous attributional tricks, they must know *how* to do wondrous attributional tricks, but this knowledge is often tacit. Attribution is an ability, not a philosophical system to which people subscribe.

Attribution Is a Form of Causal Analysis Observers of variable behavior, then, perform an unconscious attributional analysis that delivers conclusions about an actor's invariant dispositions. What are the axioms and principles that inform this analysis? Heider suggested that behaviors are the joint products of two general classes of variables—the dispositions of persons and the dispositions of environments—and that the attributional system uses a few basic principles to decompose the behavioral effect into its constituent causes. Just as the visual system factors out environmental contributions to the appearance of an object—taking into account, for example, the degree to which an egg's apparently gray shell is a result of poor illumination—so the attributional system factors out situational causes of behavior, leaving only the dispositional causes that the observer wishes to understand. Of course, the visual system can automatically and effortlessly perform this analysis of an egg because it "knows" a great deal about how light and shadow and parallax combine to influence

an object's appearance, and it uses this information to tease apart the various factors that might cause an egg to seem beige or round or smooth or nearby. What does the attributional system "know" about the causes of behavior?

Heider's most enduring theoretical contribution lies in his description of the causal model that the attributional system uses to extract dispositional invariances. In brief, Heider claimed that the attributional system thinks of behavior as an interactive product of a network of temporary and enduring causes (see Figure 1). First, behavior requires that an actor both *can* and *try* to do it. As such, a performance implies that the actor has both the capacity and motivation to perform. The actor's capacity for performance is itself a joint function of the actor's powers or abilities (e.g., skills, talents, strengths) and the environment's facilitating or inhibiting influences (e.g., task difficulty, opportunity, chance). The actor's motivation, on the other hand, is a joint function of the actor's intentions (e.g., goals and plans) and exertion (e.g., effort expenditure). In a sense, the attributional system knows the equation that describes how these factors combine to create behavior, and it can use that equation to solve for unknowns. For example, if a pitcher who wishes to retire a batter (motivation) throws a burning fastball (action) directly into the wind (environmental influence), then the observer should conclude that the pitcher has a particularly strong arm (ability). If a batter tries to hit that ball (motivation) but fails (action), then the observer should conclude that the batter lacked coordination (ability) or was blinded by the sun (environmental influence). This, then, is the general scheme that enables the person to perform a "naive factor analysis of action" (Heider, 1958, p. 123) and extract from behavior the enduring dispositions of others.

A Theory of Correspondent Inferences

Heider framed the problem that attributional systems must solve and described the rules they should use to solve it. But if one sat down to develop software for an attributional analyzer based on Heider's book, one would not get beyond the pencil sharpening. Heider's love of literature and philosophy may have deepened his insight, but it also complicated his presentation beyond the patience and ken of most working psychologists. Indeed, extracting invariances from Heider's writing proved a somewhat more challenging task than watching an egg retain its shape, and although his book was widely admired for its conceptual riches, no one knew quite what to do with it when the gushing was over. "Heider's comments are comprehensive, perceptive, and provocative. His exposition does not lend itself readily, however, to the formulation of interrelated propositional statements" (Jones & Davis, 1965, p. 220). Because such formulations and their subsequent experi-

mental tests defined the business of social psychology, Heider's wares were on clearance before the store opened.

Although Heider's theory had little direct influence on social psychology, it was indirectly influential in that it guided the development of other theories, the first of which was more successful because it was less ambitious. Edward Jones (one of the youngest presenters at the 1957 Harvard symposium) was a great admirer of Heider's theorizing and a shrewd experimentalist, and was thus perfectly positioned to play Prometheus by reformulating and extrapolating pieces of Heider's system into a recognizable psychological theory that could be used to generate research. Heider had articulated the rules by which an attributional system might infer a variety of dispositions (abilities, motivations, environmental influences, intentions) from a variety of behaviors (achievements, emotional experiences, decisions). Jones restricted the range of his own theory by concentrating instead on the rules by which attributional systems identify the specific intentions that underlie specific decisions. What the quadratic equation was to algebra, Jones and Davis's (1965) theory of correspondent inferences was to Heider's naive psychology.

Like Heider, Jones and Davis (1965) began with the assumption that "the person-perceiver's fundamental task is to interpret or infer the causal antecedents of action" (p. 220). However, Jones and Davis were particularly interested in one kind of action and one kind of causal antecedent. "Our purpose is to construct a theory which systematically accounts for a perceiver's inferences about what an actor was trying to achieve by a particular action" (Jones & Davis, 1965, p. 222). The inference of aims and intentions occupied only fifteen pages of Heider's magnum opus, but with this modest declaration, Jones and Davis proceeded to prove that less is more. Gone were the trappings of Brunswick's lens model, gone was the tight analogy to perceptual psychology, gone were the discussions of bad lighting and stiff tailwinds, gone were extended treatments of ability, skill, and task difficulty. What remained was a clear and tractable pair of questions: How do people figure out what other people are trying to do, and once they do figure it out, what does this information tell them about those other people? Jones and Davis suggested that the attributional system uses two features of an actor's decision—the number and the desirability of the decision's unique consequences—to answer these questions.

Identifying Intentions Decisions generally involve choosing between alternatives that have many features, and determining an actor's intentions or reasons for making a particular choice means determining which of these features actually drove the actor's decision. In a charmingly dated scenario entitled "Miss Adams Chooses a Husband," Jones and Davis (1965) illustrated this point by describing

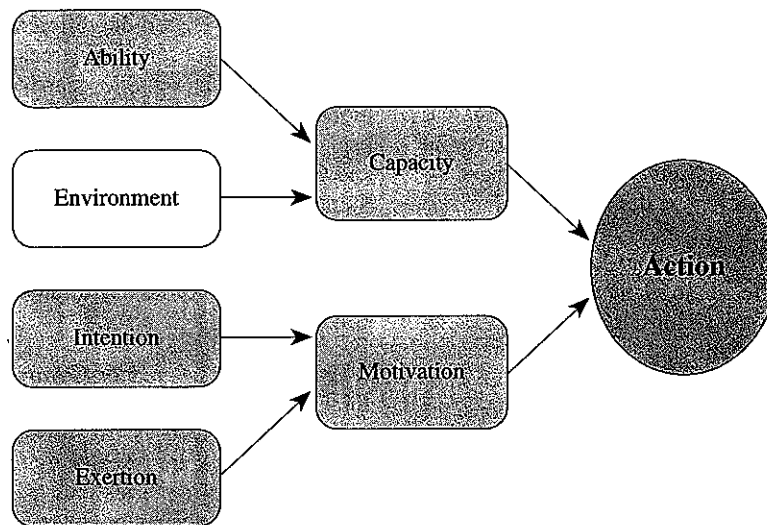


FIGURE 1 Heider's Attribution Theory.

the problem faced by an observer who wishes to determine why Miss Adams married a particular suitor (pp. 230–232). Jones and Davis began by noting that only the unique attributes of the chosen and unchosen suitors could provide an adequate explanation for Miss Adams's choice. Mr. Caldwell was eager to sire children, Mr. Bagby was enviably wealthy, Mr. Dexter had scintillating conversational skills, and any of these distinctive attributes could have driven Miss Adams's decision. On the other hand, all the suitors were splendidly handsome, so physical attractiveness clearly did not explain her choice. The point, then, is that the unique consequences—or *noncommon effects*—of a decision deliver information about the actor's intentions. Indeed, they *are* what the actor was aiming for.

Of course, alternatives can have numerous noncommon effects, and the more noncommon effects they have, the more possible intentions the actor might have had. And the more possible intentions an actor might have had, the less certain an observer can be about which of these intentions guided the actor's decision. Jones and Davis suggested that under such difficult circumstances, observers may narrow the range of possibilities by considering the *social desirability* of the noncommon effects. Heider (1958) noted, "If a person brings about a number of changes in the environment, and one of them is generally considered much more attractive than the others, we will assume that it was the person's goal" (p. 115), and Jones and Davis adopted this notion as their own. People rarely seek fatal illness, financial losses, or prolonged dental pain, and such realizations guide the observer who wishes to know which of many unique ends an actor sought. It is usually safe to assume

that someone who buys a new car "desired the car so much that he was willing to go into debt for it (and) not that he was willing to accept the burden of an automobile for the privilege of being a debtor" (Jones & Davis, 1965, p. 226). In short, knowledge about what most people want goes a long way toward understanding what any one person is trying to do.

Inferring Dispositions Jones and Davis's model suggested that both the social desirability and number of the consequences that distinguish an action from its alternatives may be used to isolate the actor's intention. For Heider, such intentions were dispositional properties; thus the attribution of intention was itself an inferential end. But Jones and Davis claimed that intentions were not the kind of dispositional properties that satisfied an observer's epistemic hunger and that the inference of an intention was merely the appetizer in an attributional banquet. Heider had suggested that observers are interested in isolating the invariant properties of persons, but Jones and Davis went further, suggesting that observers are interested in isolating a special class of invariant properties—namely, those invariant properties that distinguish one person from another. They referred to the discovery of such properties as the achievement of a *correspondent inference*. "The inference that domineering action reflects an underlying trait of dominance is correspondent to the extent that the actor's dominance is seen as greater than that of the average person (and is) . . . somewhat more intense and noteworthy than we would normally expect" (Jones & Davis, 1965, p. 224).

The notion of correspondence is predicated on the sober

insight that most of what people do says very little about them. An actor's decision may point unequivocally to a particular intention ("He gave the mugger his wallet because he didn't want to be murdered and not because it was getting too heavy"), but that intention may in turn point only to a disposition that any observer might well have assumed the actor had anyway (e.g., a desire to live). The fact that Miss Adams would probably marry Prince Charles before Charles Manson says little about her—not only because the two suitors differ in so many respects, but also because these differences are so lopsided in their mass appeal. An appreciation of wealth and fame and an aversion to criminal insanity are common preferences among normal people. In the terms of Jones and Davis's theory, the resulting paradox is delicious: not only does the social desirability of noncommon effects make the actor's intention clear ("She must have been aiming for the good thing"), it also suggests that the dispositions that one can clearly infer from such intentions are unworthy of discovery ("So what? Everyone aims for good things"). Indeed, because most ordinary people spend their days doing what most people would ordinarily do, most observers spend their days bored. One way to picture Jones and Davis's theory is to modify the diagram that illustrates Heider's (see Figure 2).

Jones and Davis's Core Ideas History is irony. Just as Heider's genius had been largely lost on his audience, Jones and Davis's attempt to set Heider to music met with only polite applause. In its first few years, the paper was rarely cited by anyone but its authors. Half the trouble was that the theory focused on only one piece of Heider's system (the inference of intention) and left the remainder of the attributional universe uncharted. The other half of the trouble was that even this contribution was buried deep in the special language of correspondence inference theory. If Heider was opaque, then Jones and Davis were only translucent. But by squinting, one finds in Jones and Davis (1965) a theory that is characterized by three very simple, very profound ideas.

The Covariation of an Actor's Behavior with Its Effects Reveals the Actor's Intentions A choice always involves at least two alternatives and thus can be thought of as comprising two conceptually distinct behaviors: acceptance of one alternative and rejection of another. When observers use the law of noncommon effects, they are using information about how an actor's behavior correlates or covaries with the noncommon features of these alternatives. For example, when an actor's behavior varies from *acceptance* ("Yes, I'll marry you, Mr. Bagby") to *rejection* ("Sorry, Mr. Dexter") as some feature of the alternative (a large bank account) simultaneously varies from *present* (Mr. Bagby is rich) to *absent* (Mr. Dexter is not), then an observer may conclude that the difference in the features of

the alternatives caused the difference in the actor's behavior, and thus the observer may make an inference about the actor's intentions ("She married Mr. Bagby for his money") and perhaps her dispositions ("She's a gold digger"). Indeed, effects that are common to the action taken and the action foregone (e.g., the physical attractiveness of the suitors) cannot provide information about an actor's intentions precisely because they do not covary with the actor's behavior. The perceived covariation of behavior and its effects, then, is the key to understanding an observer's conclusions about an actor.

When an Actor's Behavior Covaries with More Than One Effect, the Actor's Intentions Are Ambiguous The law of noncommon effects is two-pronged. It suggests that an actor's intentions are revealed by the effect with which the actor's behavior covaries, but ipso facto it suggests that when the actor's behavior covaries with more than one effect, then the observer cannot know the actor's intentions with complete confidence. This notion "may be stated in simpler terms as a near tautology. The more distinctive reasons a person has for an action . . . the less informative that action is concerning the identifying attributes of the person" (Jones & Davis, 1965, p. 228). In short, when an actor's choice covaries with many effects, it reveals many possible intentions (and hence, many possible dispositions) and the observer cannot know which of these many intentions actually guided the actor's behavior. We might say that when effects are confounded, then so is the observer.

The Covariation of Other People's Behavior with an Effect Reveals the Extraordinariness of the Actor's Dispositions The law of noncommon effects offers suggestions about how an observer can both formulate and evaluate hypotheses about an actor's intentions and the dispositions to which those intentions point. According to Jones and Davis, however, observers are not in the market for just any dispositions; rather, they are shopping for evidence of extraordinary dispositions, and they obtain such evidence by considering the social desirability of the effect that reveals the actor's intention. Social desirability is, of course, merely a convenient way to talk about how others would probably act if faced with the same alternatives; the presumed behavior of others is what tells the observer whether she should infer an ordinary or an extraordinary disposition. "To learn that a man makes the conventional choice is to learn only that he is like most other men" (Jones & Davis, 1965, p. 227; note that this turns out to be true of women also). In short, just as dispositions are revealed by the covariation of an actor's behavior with a noncommon effect, the extraordinariness of those dispositions is revealed by the covariation of other people's behavior with the same effect. One might summarize the theory of correspondent inferences this way: We know what people intend

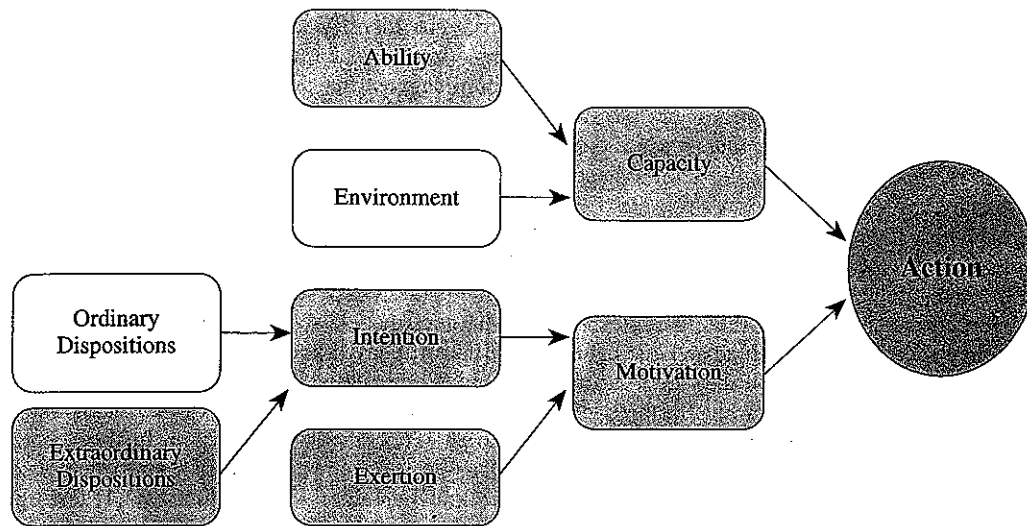


FIGURE 2 Jones and Davis's Attribution Theory.

by considering the differences between what they did and didn't do. We know what people *are* by considering whether others would have done the same.

A Theory of Causal Attribution

Science is sometimes characterized as a search for ever more general principles. In 1967, Harold Kelley suggested that the rules of ordinary personology could be understood as a special case of yet more fundamental rules—the rules of causal analysis. Heider had explicitly suggested that both attributional and perceptual systems performed causal analyses of a sort, decomposing the appearance of objects and the behavior of people into those constituents that were invariant across time and circumstance. But Kelley did more than just suggest. In a set of well-reasoned papers (1967, 1971a, 1971b), Kelley pressed the analogy and showed that the tools that enable ordinary people to do personology are the same tools that enable scientists to do science. Kelley's formulation was expansive—roaming across five years and three chapters—but its essence is captured by two core ideas.

Kelley's Core Ideas Kelley's analysis began with a question that had been one of the pet obsessions of every epistemologist since Descartes, but that at first blush seems far removed from the concerns of the ordinary personologist: How do people know whether their perceptions of the world are valid? Sometimes people swear up and down that they are seeing things as they are ("I'm telling you, the spider was at least a foot wide"); other times they admit that their perceptions may be inaccurate ("But maybe I

was temporarily confused by its barking"). Kelley suggested that the subjective validity of perceptions derives from a trio of inferential tests whose results tell the person whether her perceptions actually covary with the things perceived. For example, if one's perception of a spider in the tub is caused by, say, the presence of a spider in the tub, then at least three things should happen: First, one should not continue to see the spider when it has been washed down the drain, hauled away, or shot; that is, the experience of spiders should be a *distinctive* experience that occurs only in the presence of arachnids and not in the presence of milk chocolate, prime ministers, or empty bathtubs. Second, if the spider is a genuine spider, one should continue to see it every time one glances tubward; that is, the experience of the spider should endure or be *consistent*. Finally, a reliable property of actual spiders in actual tubs is that one's friends can see them too, and if one's experience of a foot-long spider is not a *consensual* experience, then new glasses or new friends are in order. In short, one's own experience and other people's experiences of a stimulus should covary with the enduring presence or absence of that stimulus, and if they do not, then one is hallucinating, dreaming, drunk, or at least badly mistaken. From this observation came Kelley's two core ideas.

Behaviors Are Effects That May Covary with Any or All of Three Causes The idea that causal relations are established by observation of covariation between causes and effects is the backbone of scientific epistemology, and as a doctrine, was most thoroughly developed by Hume (1740/1978) and Mill (1843/1986). Kelley claimed that this *covariation principle* was at the heart not only of scientific

logic, but also of ordinary personology. His key insight was that the trio of inferential tests that people use to verify that their reactions to an object are, in fact, caused by the object (rather than by, say, their own dispositions) are the same tests that observers might use to verify that an actor's reaction to an object is, in fact, caused by the actor's dispositions (rather than by, say, the object). How so?

Kelley agreed with Jones and Davis that, from the observer's point of view, the kinds of dispositions worth knowing are those that represent an actor's general tendencies to think, feel, or behave in ways that distinguish her from others. Kelley added that these broad and distinguishing tendencies must also be enduring if they are to warrant the trouble of discovery. Interestingly, when the three inferential tests are performed on someone else's behavior rather than on one's own perceptions, their results describe the same three properties of the actor's dispositions—durability, breadth, and uniqueness. If, for example, an actor's behavior is consistent over time ("Roy is routinely rude to Robert"), then the disposition that caused it is enduring. If the actor's behavior is not distinctly associated with one instance of a stimulus ("Roy is rude to Rachel as well"), then the disposition is broad and general. Finally, if the actor's behavior is not consensual ("Rhonda and Roger are rarely rude to Robert") then the disposition is unique or extraordinary. And if the disposition is enduring, broad, and unique, it is worth discovering. In short, people may test for three kinds of covariation—covariation of behavior with actors, with stimuli, and with time—and these tests constitute a powerful tool for the validation of one's perceptions of reality. Kelley showed that this tool was the Swiss Army knife of human inference because it could be flipped and twisted and used to discover the dispositions of others as well.

Strangely enough, modern writers often present Kelley's model as an alternative to Jones and Davis's, and befuddled students learn about two theories that seem vaguely but inexplicably connected (cf. Anderson, 1974; Jones & McGillis, 1976; and Medcof, 1990, for attempts to integrate the theories). In fact, Kelley's theory is a straightforward translation of Jones and Davis's theory into a new language, with one addition. Consider how Kelley's theory renders the theory of correspondent inferences in the language of causal analysis. As noted, the law of noncommon effects and the principle of social desirability are statements about how one may draw inferences based on the covariation of an effect with a particular actor's behavior and with most other actors' behaviors, respectively, and these two principles can be brought together in a covariation matrix (as shown in Figure 3). Imagine that Ms. Adams (no longer a Miss and now on her second marriage) is choosing between Mr. Ellsworth and Mr. Harding. In the grid shown in the figure, the rows show the alternatives between which she or any other actor may choose, and the

columns identify the actors who may make (or may be imagined to make) that choice. The entries in the left column suggest that Ms. Adams has chosen Mr. Ellsworth over Mr. Harding, and the law of noncommon effects suggests that the covariation of her behavior with the features of the two suitors clearly spells out her intention: Ms. Adams admires the fact that Mr. Ellsworth has a marvelous collection of Bill Evans records. Although few people cultivate a taste for jazz, almost everyone appreciates the finer points of a beach house in Malibu; thus one may presume that most people would have chosen Mr. Harding over Mr. Ellsworth. By the principle of social desirability, the pattern of covariation between most people's behavior and the effects tells the observer that Ms. Adams' intention points to an extraordinary disposition. In short, both Jones and Davis's and Kelley's theories suggest that observers come to know an actor's dispositions by (1) testing for the covariation of an actor's behavior with attributes of the chosen and unchosen stimuli (the law of noncommon effects, or the "distinctiveness test") and (2) testing for the covariation of everyone else's behavior with those same attributes (the principle of social desirability, or the "consensus test"). Kelley added that observers also test for the covariation of an actor's behavior with the circumstances in which it was enacted (the "consistency test").

When Behavior Covaries with More Than One Potential Cause, Its Actual Cause Is Ambiguous Recall that the law of noncommon effects is two-pronged. It does not state merely that the covariation of an actor's behavior with a particular effect reveals the actor's intention. It also states that when the actor's behavior covaries with more than one such effect, then ipso facto, the observer's confidence in her personological inference must be diminished. Kelley elevated this second prong of the law of noncommon effects to the status of a separate core axiom, which he called the *discounting principle*: "The role of a given cause in producing a given effect is discounted if other plausible causes are also present" (Kelley, 1971a, p. 8). Imagine, for example, that in addition to his jazz records, Mr. Ellsworth owns several pairs of pink trousers, and that Mr. Harding does not. In this case, Ms. Adams's decision to marry Mr. Ellsworth would leave an observer puzzled about her intention, and though one could surely conclude that "there must be something about that Ellsworth chap," one could not conclude confidently what that something was, because there is more than one "plausible cause," namely, jazz records and pink pants. The discounting principle, then, is a causal analytic version of the second prong of the law of noncommon effects, and the conjunction of this principle with the covariation principle constitutes Kelley's theory. In a nutshell, the covariation principle encourages the observer to test for covariation between an actor's behavior

		PERSONS	
		Ms. Adams	Everyone Else
STIMULI	Ellsworth:		
	(a) Kind	Chosen	Unchosen
	(b) Smart		
	(c) Trustworthy		
	(d) Bill Evans records		
	Harding:		
	(a) Kind	Unchosen	Chosen
	(b) Smart		
	(c) Trustworthy		
	(d) Malibu beach house		

FIGURE 3 Ms. Adams Chooses a Husband, Again.

and three possible causes of that behavior, and the discounting principle cautions the observer not to draw firm conclusions if any one of these tests is confounded.

Situational Causation Speakers of English have an odd habit and a not so odd habit. The not so odd habit is that they describe behavior that is driven by extraordinary dispositions as having been driven by extraordinary dispositions. The odd habit is that they describe behavior that is driven by ordinary dispositions as having been caused by external agencies. When one runs screaming from a baby rabbit, one usually owes the bystanders an explanation. Such explanations are acceptable when they are couched in terms of one's extraordinary dispositions—for example, "I have a morbid fear of fur" or "I sometimes mistake baby rabbits for Hitler." On the other hand, when one retreats from a hissing rattlesnake, one does not typically explain that behavior in terms of ordinary dispositions ("I dislike being injected with venom" or "I feel death is bad") but rather, in terms of the stimuli that invoked them ("It shook its thing at me"). When situations appeal to or invoke ordinary dispositions, speakers naturally talk about the resulting action as having been "caused by the situation." In a strict sense, such actions are just as dispositionally driven as are any others; thus this odd habit is a mere figure of speech. The person who delivers her purse to a gun-wielding thug is driven by the extremely ordinary desire to continue breathing and the extremely ordinary belief that bullets can waylay that plan, but the conversational injunction to talk about that which is extraordinary (Grice, 1975) leads good communicators in such instances to discuss ab-

normal conditions and adopt externalizing language (see Hilton & Slugoski, 1986).

Kelley was a good communicator who adopted externalizing language, and in so doing, he fostered an important and enduring change in social psychology's habit of speaking and thinking about the causes of behavior. Heider had emphasized the distinction between personal and situational causes of behavior, but when Heider spoke of a situational force he usually meant it literally. Heider's situational forces were typically winds that helped pitchers and lights that blinded batters; they were physical forces that emanated from the environment and directly facilitated or inhibited an actor's ability to execute a behavior (see Figure 1 again, but come right back). In effect, Kelley took the external influences on capacity that Heider had discussed (tailwinds and bright lights), and the external influences on motivation that Jones and Davis had discussed (bribes and threats); poured, mixed, and stirred; and referred to all members of the new class as situational causes. For students of ordinary personology, this way of speaking extended the concept of situational causation considerably, and suddenly behaviors that were caused by rain, sleet, snow, gloom of night, and ordinary dispositions, were all thought of as having been "caused by the situation." Kelley (1972a) even managed to talk about causes that were located strictly between an actor's ears as external:

The external cause relevant to the behavior in question . . . has been defined by such things as a dependency relationship between the actor and some other person, surveillance by a supervisor, the experimental instruc-

tions, a job assignment given a student by his instructor or coach, the importance placed on demonstrating one's compatibility with another person, and even the individual's own ability. (p. 10)

Ability as an *external* cause? What could that mean? If talk of external causes is just an odd habit—if *external cause* is itself just a code word for *ordinary disposition*—then such claims make sense. Alas, Kelley's would become known as a theory that described how ordinary people decide whether to attribute behavior "to the person" or "to the situation," and because that distinction appears innocent at first glance, few of the consumers of the theory would move beyond the first glance to decode the phrase. Rather, they would erroneously conclude that ordinary personologists explain behavior as the product of two, qualitatively different *kinds* of causes—one inside the actor and one outside the actor—and they would fail to recognize that situational causation was nothing more than a colloquial way of talking about those dispositions that everybody seems to have. When consumers of Kelley's theory took the phrase "situational causation" literally and equated bribes and punishments with windstorms and bullets, they created a dualism that even Descartes would envy and embroiled attribution theory in a philosophical conundrum from which it has never quite escaped (see Kruglanski, 1975; Ross, 1977; Solomon, 1978; White, 1991). With the publication of Kelley's model, then, the occasionally imponderable distinction between the internal and external origins of action replaced the consistently clear distinction between ordinary and extraordinary dispositions as attribution theory's critical dichotomy.

A Familiar Metaphor "There is not one but many attribution 'theories' and the term refers to several different kinds of problem" (Kelley & Michela, 1980, p. 458). True enough. But if any model deserves to be called *the* attribution theory, it is Kelley's. His papers had the widespread, powerful influence that neither Heider nor Jones and Davis had managed, and his work continues to serve as the foundation for theorizing about the nature of causal inference (e.g., Cheng & Novick, 1992; Hewstone & Jaspars, 1987; Hilton & Slugoski, 1986). The model succeeded in part because it was marvelously synthetic, suggesting that a few basic principles could be used to redescribe theories as diverse as Jones and Davis's (1965) theory of correspondent inferences, Bem's (1967) theory of self-perception, and Schachter and Singer's (1962) theory of emotion. Kelley's model gathered a handful of smaller models under one conceptual umbrella and thus looked suspiciously like the sort of unified field theory over which scientists often burst small blood vessels. At least it was a closer approximation to that ideal than were most of the social psychological theories that had gone before, and it raised hope that some-

day social psychology's separate domains would prove to be islands joined beneath an attributional sea.

The success of Kelley's model lay in its scope, and also in the appeal of its core metaphor (see Gigerenzer, 1991). Although most social psychologists probably cannot define *attribute-effect linkage* or *synonymous mediation*, despite having read Jones and Davis and Heider in their youths, every one of them could define *covariation* long before they ever read Kelley. Kelley's crucial move was to describe the rules of ordinary personology in a language in which social psychologists were already fluent. The analysis of variance was the field's favorite statistical tool, and like a wise anthropologist, Kelley spoke to social psychology in its native tongue rather than asking it to learn a foreign one. Heider's murky prose and Jones and Davis's relentless neologisms were replaced by the clean and familiar language of experimental design. Social psychologists had struggled for decades to understand how ordinary people think, and Kelley solved that problem in one sweet stroke: ordinary people, he claimed, think like social psychologists.

ASCH'S CHILDREN: THE PROCESS OF ORDINARY PERSONOLOGY

The foregoing discussion may give the mistaken impression that attribution theorists didn't get out much. In fact, attributional theories inspired a good deal of attributional research, most of which sought to determine whether and when people actually followed the rules that the theories specified. For example, Jones, Davis, and Gergen (1961) showed that the social desirability and noncommon effects of an action could influence an observer's willingness to draw correspondent inferences about an actor, much as Jones and Davis's (1965) model said they should. McArthur (1972) showed that observers use consistency, consensus, and distinctiveness information much as Kelley's (1967) model said they should. But as Smith (1994) noted, even when "people's responses follow the predictions of a theory stated at the algorithmic level, that fact is *not informative* about properties of the implementation level" (p. 83). In other words, attribution theories described logical rules, and in turn, attribution research showed that these rules described people's judgments. But just as geometry might be used to describe the structure of a honeycomb without saying how bees make one, neither attribution theory nor attribution research could say how rules such as the law of noncommon effects or the discounting principle were put into practice by creatures of flesh and bone. Just as math is not biology, so logic is not psychology.

Although neither Heider nor his intellectual progeny had had much to say in this regard, Asch had made the *process* of ordinary personology his primary concern. In a

seminal monograph, "Forming Impressions of Personality," Asch (1946) described several elegant experiments that were designed to answer two questions: "In what manner are these impressions established? Are there lawful principles regulating their formation?" (p. 258). Just as Gestalt laws were thought to govern the process by which people construct their experience of a whole object from perception of its individual features, Asch believed there must be laws that govern the process by which people construct a unified impression of a person from knowledge of the person's individual attributes. With this conviction in mind, Asch read strings of trait adjectives to his subjects and asked them to write down the impressions they formed of the hypothetical person whom those adjectives described. Each of the ten experiments described in his monograph was a parametric variation on this theme. In some he altered the content of a critical word, in some he reversed the order in which the words were read, in some he decreased the number of words in a string, in some he manipulated the position of a word in the string, in some he read two strings that described two different people and then later confessed to his subjects that the two strings described only one individual. His primary measure in each of these experiments was the subject's free description of the impression that he or she had formed. Asch was a lifelong aficionado of J. S. Bach, and the intricate patterning of his experimental variations looked suspiciously like an exercise in baroque counterpoint.

On the basis of what his subjects said, Asch drew several conclusions about the laws that govern impression formation. Of these, one was paramount:

In different ways the observations have demonstrated that forming an impression is an organized process; that characteristics are perceived in their dynamic relations; that central qualities are discovered, leading to the distinction between them and the peripheral qualities; that relations of harmony and contradiction are observed . . . But we are not content simply to note inconsistencies or to let them sit where they are. The contradiction is puzzling, and prompts us to look more deeply. Disturbing factors arouse a trend to maintain the unity of the impression, to search for the most sensible way in which the characteristics could exist together. (Asch, 1946, pp. 284-285)

In other words, the whole is more than the sum of its parts. People go far beyond the discrete items of information given in a list and attempt to form a unified impression that makes sense of these items by considering their relations. People do not think of others as a collection of unrelated features (educated, important, unshaven); rather, they find a unifying explanation for those features ("He's a rabbi!"), and that unifying explanation *is* the impression. In this

way, Asch's psychology was very much like Heider's. Both saw the ordinary personologist as striving toward an understanding that could be achieved only by penetrating the surface of behaviors or traits and glimpsing the psychological essence of which behaviors and traits were merely manifestations. The fact that a person winked at another was not, Heider argued, important in and of itself because behavior is merely the "medium for the transmission of psychological characteristics" (Heider, 1958, p. 35). The fact that a person was extroverted was not, Asch argued, important in and of itself, because "the subject seeks to reach the core of the person *through* the trait or traits" (Asch, 1946, p. 284). Both Asch and Heider thought of ordinary personology as the process of finding a lot in a little.

Asch's studies examined just ten of the many possible permutations of word order, list length, and word position. This fact, and the appealing simplicity of his experimental method, constituted an open invitation for others to join the game, and in fairly short order researchers began varying this and that parameter to show that this or that mechanism could explain this or that result. For example, researchers argued about whether Asch's primacy effect (the tendency for the earliest words in a list to dominate the impression) was due to the fact that early words (e.g., intelligent) change the subject's interpretation of the later words (e.g., "Hmmm, *impulsive* must mean that he gets easily excited by good ideas") or to the fact that subjects give less weight to later words (either because later words are presumed to be less important or because the subject just gets tired). Several investigators offered compelling evidence in favor of some version of the change-of-weight hypothesis (e.g., Anderson & Jacobson, 1965; Hendrick & Constantini, 1970) and against the change-of-meaning hypothesis favored by Asch (*cf.* Hamilton & Zanna, 1974). Anderson (1974) even offered to incorporate the impression formation process into a more formal and more general model of human judgment and decision making.

Despite all these experiments about experiments, something important failed to happen. Asch had hoped that his studies would uncover the *process* by which people formed impressions, but what they uncovered were the *laws* that described the relation between a stimulus (a particular string of words) and a response (a particular impression). Laws are general summaries of the way things happen ("If given half a chance, mass will move toward the center of the Earth") and are deduced from observations of specific things happening ("The blender fell off the counter, the baby fell off the blender, nothing has fallen up so far"). Processes, on the other hand, are the intermediary events that explain why things happened as they did ("The spinning planet exerts force on the wet baby's electrons, and . . ."). Asch suggested that between the stimulus and the response, subjects formed an impression that "made sense of things," but that suggestion merely begged the

question of what process enabled them to form that impression. Asch was uncharacteristically silent when it came to describing the events that took place inside the black box. Indeed, he could hardly have done otherwise, because although Asch was yearning to say how people understand people, the language in which such sentences could be spoken had not yet been invented.

The Social Cognitive Tradition

In the 1970s, social psychology began a series of raids on the new cognitive psychology that was booming just across the corridor in psychology departments the world over, and the spoils included metaphors and techniques that enabled social psychologists to describe and measure mental processes in a way they never had before. This newfound ability to talk about mental operations in turn created a mandate to do so, and the result was the birth of a new research tradition known as social cognition.¹ What distinguished this new tradition from the attributional tradition that preceded it? Although the attribution theorists had accepted Asch's contention that much could be learned about ordinary personology by experimentally varying configurations of stimulus information and then observing how these variations affected subjects' judgments, they generally rejected Asch's methods for doing so. Instead, their experiments usually involved real social interactions between actors and observers, dynamic stimuli such as videotapes and sound recordings of an actor's words and deeds, or at the very least, narratives about the behavior of an actor. The dependent measures in such experiments were generally a few Likert scales that required observers to rate the actor's behaviors or dispositions. Because the attributionists' experimental stimuli were reasonably complex and their dependent measures reasonably crude, their data said little about the mental processes that had generated them.

The social cognitivists brought a very different sensibility to the study of ordinary personology. First, because these researchers were directly—almost exclusively—concerned with precise descriptions of mental processes and the mental representations produced by these processes, they took Asch's methodology as a model for their own. Their experiments tended to examine subjects' reactions to exceptionally spare stimuli—often the very strings of trait words used by Asch—and their studies were designed to maximize internal validity, precision, and control at the expense of external validity and mundane realism. But a lot had happened in the thirty years since Asch had interviewed his subjects, and the social cognitivists had at their disposal a variety of new and powerful laboratory techniques—from dichotic-listening and divided-attention tasks to reaction time measures and signal detection analyses of memory—that promised to reveal the psychological mechanisms underlying their subjects' judgments. Second,

the social cognitivists believed that the personological puzzle was only one of many that the human mind had evolved to solve; thus they believed that models of social cognition ought to use the theoretical language of, and be thoroughly grounded in, more general models of cognition (Wyer & Carlston, 1979; Wyer & Srull, 1986). Knowing people was an instance of knowing—period—and social psychological theory and research ought to reflect that fact.

Their song was a hit. By the late 1970s and early 1980s, the attributional models that had dominated social psychology for decades found themselves rocking on the front porch as a new set of questions engaged a new generation of researchers. By what mental operations—rather than by what logical principles—are attributions made and impressions formed? That is, what actually happens inside an observer's head when she makes a dispositional inference about Abbott or "gets a sense" of Costello? If judgments of others are the products of logical rules, then how are those rules instantiated and how are those products produced? As new questions do, these created a flurry of research activity, and students of ordinary personology rapidly moved from the business of describing logical rules to the business of describing what happens inside a black box. There were right ways and wrong ways to make such descriptions, of course, and the prevailing wisdom in cognitive psychology suggested that mental events should be described as a *sequence of operations* that had *operating characteristics*. The social cognitivists heeded the conventional wisdom and got busy making Asch's dream come true.²

Operating Sequence

People are information processors. That statement is so banal and benign that few psychologists would even bother to write it. Yet it says much of what there is to say about the approach to psychology that was the legacy of the "the cognitive revolution." That approach construes a wide range of mental phenomena in terms of the actions of a sequence of operators (or processors) that receive information (or data), transform it, and then pass it to the next operator. The notion that stimuli are sequentially sensed, transduced, encoded, represented, elaborated, integrated, rehearsed, stored, and retrieved is such a ubiquitous aspect of modern theorizing that psychologists today are apt to wonder how their predecessors ever managed to lecture about mental life without backing into a flowchart. One might go so far as to suggest that the cognitive approach is defined by the notion that one explains a phenomenon only when one specifies the sequence of mental operations that produce it (Broadbent, 1958; Miller, Galanter, & Pribram, 1960; Neisser, 1967; Newell & Simon, 1972; see also Lycan, 1989). By what sequence of operations, then, are observers' inferences about actors produced? Social psychologists have explored three general classes of opera-

tions (see Figure 4): *identification* (the process by which observers identify acts), *attribution* (the process by which observers draw dispositional inferences from the acts they have identified), and *integration* (the process by which observers form impressions from the dispositional inferences they have drawn).

Identification Attribution models specify the logic by which observers make the inferential journey from acts to dispositions. But before that logic can be implemented, observers must somehow get to acts. How do people move from the observation of overt *actions* to the identification of meaningful *acts*—from the physical motions of hands and feet and eyes and lips to the psychological notions of *murdering* and *stealing* and *coveting thy neighbor*? The physical structure of action helps by providing discernible beginnings and endings that enable observers to extract these actions as a unit from the ongoing stream of bodily movements (Newtson, 1973; Newtson et al., 1987). But once extracted, actions may still have numerous identities, such as *contracting facial muscles*, *smiling at the boss*, and *sucking up* (Borkenau, 1986; Vallacher & Wegner, 1985). Each of these is a proper identity, and research on the identification process has largely been concerned with determining *which* of these proper identities the ordinary personologist will ordinarily choose.

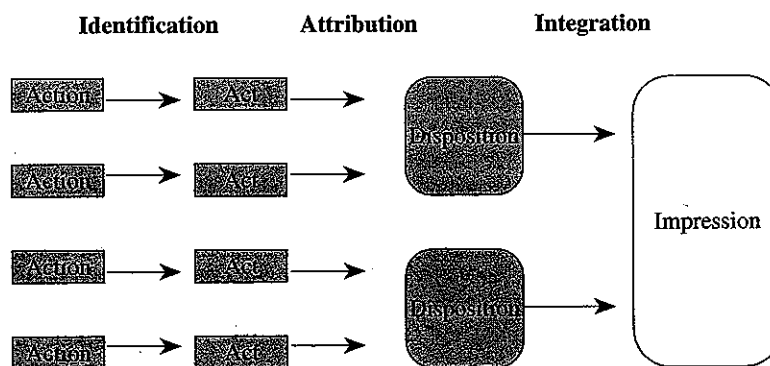
Observers Prefer to Identify Actions in Terms of the Actor's Intentions Many philosophers and psychologists have suggested that of all the ways in which actions can be correctly identified, human beings prefer to identify actions in terms of the constellation of beliefs, desires, plans, and goals known as the actor's intentions (Dennett, 1987; Dretske, 1988; Goldman, 1970; Wegner & Vallacher, 1987). One feels fairly well informed on learning that Frank crossed the room and turned on the television, but merely puzzled on hearing that Frank crushed carpet fibers before changing the balance of ions in the atmosphere. If the two descriptions are equally accurate, then why does

the former feel so much more apt? The reason is that when one asks what Frank is doing, one wants to hear about the changes in the world that Frank was trying to bring about (the activation of the television set) and not about changes that were merely incidental (the flattening of the carpet) or enabling (the moving of feet). In a sense, an act is what actions strive to accomplish.

Jones and Davis (1965) made the concept of intentionality the centerpiece of their theory, which can be (but is not often) thought of as a model of both the identification and attribution processes. They suggested that the process of dispositional inference begins with an identification stage in which the observer isolates the action's noncommon effects ("Both *crossing the room and turning on the television* and *crossing the room without turning on the television* would have allowed Frank to crush carpet fibers, but only the former allowed him to watch the news and use electricity"), weighs their social desirability ("Some people like to watch the news, but nobody is dying to use electricity"), and then uses this information to infer the actor's intentions ("Frank must have crossed the room and turned on the television because he wanted to watch the news"). This inference of intention is nothing more or less than an identification of the action ("What did Frank do? Why, he turned on the news!"). Once the behavior has been so identified, the observer may move to the attribution stage, in which she uses the logical rules already reviewed to determine whether the behavior reveals an ordinary disposition ("Frank is a curious person") or an extraordinary disposition ("Frank is aroused by the NBC peacock"). Jones and Davis were among the first social psychologists to try their hand at a flowchart. Figure 5 illustrates their two-stage model: the noncommon effects and social desirability of an action point to the actor's intention, which identifies the action and prepares it for attribution.

The Actor's Intentions Are Often Ambiguous, but Observers Resolve That Ambiguity This model does not solve the problem of action identification completely, be-

FIGURE 4 The General Sequence of Operations in Ordinary Personology.



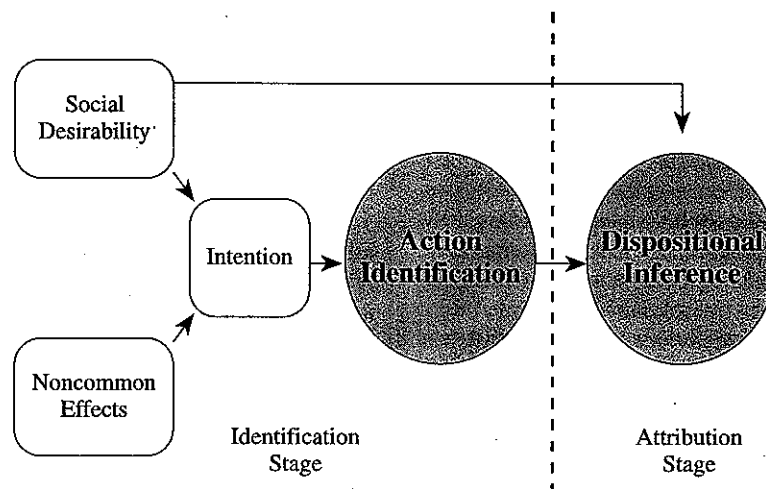


FIGURE 5 Jones and Davis's Two-Stage Model.

cause some actions have more than one unique consequence and thus implicate more than one intention. Is the student who slips his friend an answer during an exam intending to help his pal or intending to break the rules? Surely both of these intentional descriptions are in some way accurate. How, then, does the observer resolve this ambiguity so that she may identify the student's action and proceed to questions about the dispositions that may have caused it?

Jones and Davis (1965) suggested that observers resolve the ambiguity in action identification by using rules—for example, by assuming that desirable consequences (helping a friend) are intended and that undesirable consequences (breaking the rules) are incidental. Although few modern researchers would quibble with that suggestion, work in the social cognitive tradition has taken a rather different approach to the problem of ambiguity resolution in action identification. Rather than attempting to spell out the inferential rules that allow observers to reason their way out of the dilemma that ambiguity produces, social cognitivists have investigated the cognitive and contextual factors that dictate how such ambiguities are resolved before they ever enter the observer's awareness. For example, work on construct accessibility has shown that observers tend to identify behaviors in terms of the identities that were most recently or frequently used (Bargh et al., 1986; Higgins & Bargh, 1987; Higgins, Bargh, & Lombardi, 1985; Srull & Wyer, 1979, 1989). If an observer has just been thinking about honesty, or if an observer frequently thinks about honesty, then she is more likely to identify the student's behavior as cheating than helping (Higgins, Rholes, & Jones, 1979). Similarly, research has shown that an observer's goals may influence how she will

resolve ambiguity in action identification: the student who received the illicit aid and the teacher are likely to identify the passing of answers rather differently (Bargh, 1990; Jones & Thibaut, 1958; Srull & Wyer, 1989). Whereas attributionists looked for inferential rules that, when applied correctly, would allow observers to locate the proper identity of an action, social cognitivists looked into the observers' past and at the observer's current circumstances for factors that might predispose her to embrace one identity over another. This focus on unconscious, passive processes that "happen to" the person rather than on conscious, active operations that the person "does" was one of the subtle but important theoretical shifts fostered by the social cognitive tradition. It will shortly become the focus of our discussion.

Observers Often Identify Actions with Trait Terms One of the marvelous complexities of the English language is that it allows speakers to describe acts and people with the adverbial and adjectival forms of a single word, but to mean very different things by those descriptions. The student's mother may agree with the principal that the student *behaved* dishonestly, but she would undoubtedly claim that such behavior does not mean that her son *is* dishonest. She could, as it were, love the sinner while hating the sin—especially if her son had been forced to sin by an external agent (e.g., the threat of retaliation, the cheating pills that were unknowingly dropped in his cocoa). The fact that trait terms can be used to describe the actions of people who may or may not have those traits has frustrated social psychologists' attempts to theorize clearly in English, but it is also a fact of some psychological note. Ordinary people are sometimes content to terminate the identification process

with unambiguous intentional descriptions such as "Fred wanted to watch the news," but most often they seem to prepare for the upcoming attribution stage by labeling the intentional behavior with the trait word to which it might correspond ("Fred is feeling *bored*" or "Fred acted *responsibly*"). These are *not* dispositional inferences. They do not mean that Fred is generally more bored than other people or that he is extraordinarily responsible across time and circumstance. Rather, they suggest that Fred's interest in watching the news *points toward* or *hints at* such traits without confirming their existence (see Bassili, 1989; Newman & Uleman, 1993; Trope, 1989; Uleman et al., 1993). Of all the many psychological phenomena that research in the social cognitive tradition has uncovered, perhaps the most robust is the tendency for observers to identify actions in terms of the traits to which they may or may not correspond (e.g., Srull & Wyer, 1989). The cheating student, most people seem prepared to say, behaved dishonestly or loyally.

Attribution

The Attribution Process Consists of Two Subprocesses

Once an act has been identified in terms of the actor's intentions or potential traits, attribution theories suggest that observers use inferential rules to move from that identification to either a dispositional attribution (a correspondent inference of extraordinary disposition) or a situational attribution (an inference of ordinary dispositions). In a sense, attributional logic serves as a vehicle that shuttles observers from an inferential starting position ("Opal is behaving anxiously") to one of two destinations: the dispositional ("Opal's a pathologically anxious person") or the situational ("Opal's a normal person who just happens to be waiting for a dentist to torture her"). Quattrone was an attribution theorist who came of age in the social cognitive era, and so was uniquely positioned to drag attributional models kicking and screaming into the present by sketching the sequence of mental operations that implemented their logic. Like his attributional forebearers, Quattrone assumed that observers ultimately arrive at either situational or dispositional conclusions. But unlike his forebearers, Quattrone suggested that observers come to these conclusions not by different routes, but aboard an inferential bus that makes one or two stops.

Quattrone's theorizing was predicated on the work of Tversky and Kahneman (1974), who had noted that because people do not always have the luxury of lingering over the details of logical problems, they often solve such problems by quickly approximating a solution and then refining that approximation as time and circumstances allow. For example, when asked to estimate how many airplanes fly from New York to Los Angeles on a given Monday, a person might quickly reason, "Probably about one an hour

all day long, so I'd say around twenty-five," and only then begin to think more critically about exceptions ("Of course, the number of flights probably decreases drastically at night, so . . ."). If circumstances require an immediate response, the person can offer the initial rough estimate, and if the person does have the luxury of lingering, she can use her further musings to revise that estimate ("More like seventeen, now that I think about it"). The beauty of this anchoring-adjustment mechanism is that the person can take the time to solve a complicated problem well while always having a reasonable (if imprecise) answer at the ready.

Quattrone's (1982) insight was that this anchoring-adjustment mechanism could also be used to describe the sequence of mental operations by which people make attributions for behavior. He argued that rather than lingering over the choice of dispositional and situational explanations, observers approach the personological puzzle by first *assuming* a correspondence between the action they have identified and the actor's dispositions, and then correcting or adjusting this assumption if further meditation on the matter dictates they do so. For example, Quattrone's model suggested that on identifying Opal's behavior as nervous, an observer would naturally assume that Opal is an unusually anxious person and only then consider the fact that Opal is waiting to see the dentist. People may ultimately arrive at situational attributions, but Quattrone suggested that they do so by first passing through dispositional ones. The dispositional attribution has priority for many reasons, not the least of which is the marvelous coincidence that allows dispositions and actions to be described by the same English word. The impact of Quattrone's theorizing cannot be fully appreciated until we explore the problem it was specifically meant to solve (which we will do soon) and until we examine the models it ultimately spawned (which we will do even sooner). For the moment it is enough to note that Quattrone opened up the black box labeled "attribution," peeked inside, and found a pair of sequential processes: dispositional inference followed by situational adjustment.

Identification and Attribution Are Interactive Identification enables the observer to move from actions to acts, and attribution enables the observer to move from acts to dispositions. Although social psychologists had described each of these processes in isolation, Trope (1986) suggested that the two could be described in a common language that allowed their interactions to be examined. Trope noticed that two factors enable the observer to identify the actor's behavior, and that these same two factors also enable the observer to attribute that behavior to the actor's situation or dispositions. First, observers of a behavior ("Opal is biting her fingernails") often have knowledge of the actor's *prior behaviors* ("Opal has often behaved ner-

vously before"). Second, observers often have knowledge of the actor's *current situation* ("Opal is waiting to see the dentist"). Trope argued that each of these factors has an *additive effect* on action identification; that is, each factor encourages the observer to assimilate the action's identity to the factors themselves. For example, if an observer knows that Opal behaved nervously last month (nervous prior behaviors) or knows that she is now seated in her dentist's waiting room (nerve-racking current situation), then that observer is particularly likely to identify Opal's nail biting as *extreme nervousness* rather than as *grooming* or *snacking* or *mild anxiety*.

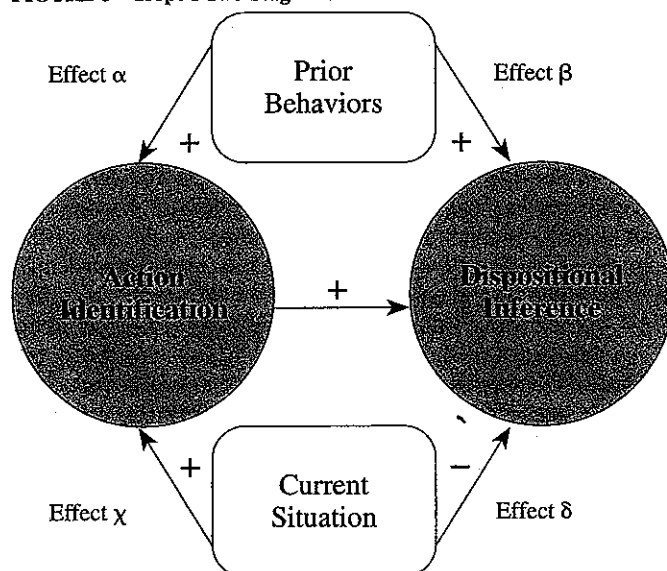
Although these factors both have additive effects at the identification stage, Trope's model suggested that they have very different effects at the attribution stage (see Figure 6). Specifically, although the observer's knowledge of the actor's prior behavior exerts an additive effect at both the identification stage (effect α is positive) and the attribution stage (effect β is positive), the observer's knowledge of the actor's current situation exerts an additive effect at the identification stage (effect χ is positive) and a *subtractive effect* at the attribution stage (effect δ is negative). For example, knowing that Opal behaved nervously in the past should encourage the observer to identify Opal's nail biting as nervousness (effect α) and to attribute that nervous behavior to her dispositions (effect β) because such behavior is, evidently, typical of Opal, and dispositions are supposed to be enduring tendencies to behave in certain ways. On the other hand, knowing that Opal is waiting for the dentist should encourage the observer to identify Opal's nail biting as nervousness (effect χ), but should *discourage* the observer from attributing Opal's nervous behavior to her enduring

dispositions (effect δ), because the upcoming dental visit provides a plausible situational explanation for her behavior.

Note how Trope's model incorporated the classic rules of attributional logic. For example, the β effect was a simple way of describing the results of Kelley's consistency and distinctiveness tests; that is, if Opal behaves nervously over time and across situations, then this current instance of nail biting probably should be taken as further evidence that she is a dispositionally nervous person. Similarly, the δ effect was a simple way of describing the results of Kelley's discounting principle or Jones and Davis's social desirability principle; that is, if an upcoming dental visit would cause most people to bite their nails, then this current instance of nail biting probably should not be taken as evidence of Opal's dispositional anxiety. Trope's model, then, suggested that the factors that determine how actions will be identified also determine how they will be attributed, and that one of these factors—namely, knowledge of the actor's current situation—affects these processes in opposite ways. As with Quattrone's model, the utility of this model will become even more apparent when we later explore one of the nagging problems it helped solve.

Integration In 1980, Kelley and Michela counted more than 900 attributional studies that had been published in the previous decade, and almost fifteen years later, Smith (1994) suggested that this number had tripled, quadrupled, or more. Wyer and Lambert (1994) argued that although these many studies had been informative, it was not clear that "the overall gain in our understanding of social inference phenomena has been commensurate with the enormous amount of research performed," in part because

FIGURE 6 Trope's Two-Stage Model.



"very little attempt was made to . . . specify the role of attribution phenomena within a broader framework of person perception" (p. 114). Because attribution theories were agnostic about process, they made no attempt to ground themselves in more general models of human cognition. In this sense, Wyer and Lambert were right. If social psychologists knew all there was to know about attribution, there would still be much they did not know about ordinary personology because, as Figure 4 shows, ordinary personology does not end with the inference of dispositions from behavior. An observer may notice an actor on a park bench looking sullen despite glorious weather and the presence of several amusing geese, and may conclude that the actor is taciturn. Later, the observer may notice the same actor in a dark corner of the library making notes on a dusty tome and conclude that she is scholarly. As Asch (1946) argued, however, these two dispositional inferences do not reside as separate items on some sort of mental list that describes the actor, but rather are integrated into a single impression that may be more than the sum of its parts ("She's one of those brooding intellectuals who wears black turtlenecks and fancies German opera"). Attribution theories (particularly Kelley's) explain quite nicely how observers draw inferences about an actor's dispositions from multiple behaviors, but they say nothing about how multiple inferences of multiple dispositions give rise to a general impression of the actor.

How are dispositional inferences integrated? This was Asch's burning question, and it has continued to be a burning question for the social cognitive tradition that Asch begot. Studies of the integration process have generally used one of two methods. Some have required subjects to form impressions from descriptions of traits ("Jack is mean and dishonest"), and others have required subjects to form impressions from descriptions of behavior ("Jack kicked the dog and then denied doing so"). The former method clearly requires that the observer perform integration, but what about the latter? One might assume that asking subjects to move from behavioral descriptions to impressions invokes all three operations—identification, attribution, and integration—but it probably does not. First, behavioral descriptions do not allow subjects to perform the identification operation. Such descriptions do not describe actions ("Jack's foot moved back toward his buttocks and . . ."). Rather they describe the unique ends toward which the actor was aiming and thus offer subjects "pre-identified" acts ("puppy bashing") rather than allowing subjects to generate their own alternative action identities ("exercising" or "teaching Sparky how to jump" or "clumsy hunting"). Second, the behavioral descriptions used in these experiments rarely mention situational antecedents that might have facilitated or inhibited the act ("When Jack entered the room, the ever-rabid Sparky roared and lunged"). Instead, they strongly imply dispositional causes ("Jack's a meanie"). In short, experiments

that use behavioral descriptions as stimuli have tended to bypass the identification operation and minimize the attribution operations, and thus their results bear directly on the integration operation.

The sheer volume of work on integration means that any simple answer to Asch's burning question must average across daunting complexities. Furthermore, research on the process by which dispositions are integrated into impressions has been closely linked with research on the representation of impressions in memory—a topic that has received so much attention in the last few decades that separate, detailed treatments can be found elsewhere in this *Handbook* (see Smith, 1998; see also Wegner & Bargh, 1998). Although the present chapter will not attempt to duplicate the coverage of others, it may be worthwhile to note briefly three features of the integration process and of the impressions it produces (see Hamilton & Sherman, 1996, for a related analysis).

Impressions Are Coherent As Asch surmised, impressions are enormously coherent. Observers do not think of actors as collections of unrelated dispositions; rather they use the dispositions they have inferred from behavioral evidence ("taciturn and scholarly") to construct unifying explanations ("brooding intellectual type") that allow them to infer new dispositions for which they have no evidence ("opera lover"). To construct these explanations, observers must have extensive practical knowledge about the ways in which various dispositions are likely to interrelate, and researchers have referred to that system of knowledge as the observer's *implicit personality theory* (Schneider, 1973; Wishner, 1960). Whether these theories take the form of abstract axioms to which observers consciously subscribe (e.g., "Kind people tend to be honest, but not necessarily impulsive"), or whether they are implicit in observers' memories of individuals whom they've encountered (e.g., "He's got Bubba's sense of humor. I bet he's a cheapskate too"), they allow observers to use the dispositions they have extracted from an actor's behavior to predict the presence or absence of dispositions about which they have no direct knowledge. (For more about how such theories may be represented, see Smith, 1998, in this *Handbook*; see also Cantor & Mischel, 1977; Carlston, 1992; Sherman & Klein, 1994; Smith & Zarate, 1992; Srull & Wyer, 1989). In short, the average person may have frighteningly little knowledge of geography, world politics, or history, but she has a remarkably nuanced understanding of the ways in which dispositions go together. Asch once suggested that the most important finding to emerge from his studies was that of his many subjects, not *one* was *ever* at a loss to explain how the most incongruous traits might cohere.

Impressions Are Structured Functionally If people have implicit personality theories that tell them how to integrate dispositions to predict new ones, then what are the princi-

ples and axioms that constitute these theories? Mathematical theories provide axioms that tell us how to integrate 3 and 8 to obtain 11. By what axioms do people integrate "powerful" and "porcine" to obtain "Speaker of the House"? Though not for lack of trying, psychologists have never discovered a simple trait calculus that would allow them to predict complex impressions from knowledge of their components.³ The structure of complex impressions does, however, seem to reflect two organizing principles. First, impressions of personalities tend to be organized in roughly the same way that personalities are (Peabody & Goldberg, 1989; Williams et al., 1995). An actor's extroversion suggests sociability but not helpfulness to an observer, because extroverted people do tend to be sociable but are not exceptionally helpful. Second, impressions are inevitably structured around an evaluative dimension (Os-good, Suci, & Tannenbaum, 1957; Rosenberg & Sedlak, 1972); that is, implicit personality theories suggest to their owners that an actor's traits can most easily be used to predict the presence of other traits of the same valence. If an actor is known to be honest, for instance, then observers are quite willing to assume the presence of other positive traits, such as kindness and generosity, that may or may not actually be associated with honesty (Cooper, 1981). This *halo effect* is especially pronounced in the case of negative traits, which are readily used to predict other negative traits and thus tend to dominate impressions (Kanouse & Hanson, 1971; Skowronski & Carlston, 1989).

Why should impressions be organized realistically and evaluatively? The reason is that impression formation is not an abstract intellectual exercise that observers do in lieu of the Sunday crossword. It is a functional process whose products enable observers to navigate their social worlds, and as such, the structure of such an impression is substantially dependent on the navigator's purposes. Impressions that are generally realistic but that emphasize the hedonic value of an actor's dispositions for the observer are just the kinds of impressions that ought to enable observers to approach or avoid actors with maximally beneficial results. Because most observers inhabit reality and wish to continue doing so, their impressions tend to emphasize the actual and relevant aspects of the actor's personality. This functional argument is not strictly post hoc. Jones and Thibaut (1958) described several different goals that observers might have when forming impressions and suggested that these goals have broad effects on the operations of ordinary personology. Research has shown that manipulating observers' goals does change the structure of the impression they form (see Srull & Wyer, 1986; Vorauer & Ross, 1993). In short, one can navigate a river in a rowboat, a tugboat, or a raft, and the structure of one's craft depends on whether one is on the river to fish, tow, or ditch Aunt Polly. Impressions also serve a navigatory purpose, and thus the implicit personality theories that determine

their structures are themselves determined by the ends that the observer hopes to achieve.

Impressions Function Autonomously It is a mistake to assume that if something can be named (memory, trait) then there must be something in the observer's head to which that name corresponds (see Ramsey, Stich, & Garon, 1991; Smith, 1998, in this *Handbook*; Stich, 1983). To what, then, if anything, does the word "impression" correspond? Do observers have mental representations of actors that are distinct from their representations of the actor's behavior, or is the word "impression" merely a psychologist's shorthand that acknowledges the fact that observers use observations of past behavior to predict future behavior? In fact, impressions have a good claim to being representationally distinct from behaviors, because they so often *behave* as though they are independent of the behavioral evidence from which they were drawn. For example, people are able to articulate their impressions of an actor long after the behaviors that gave rise to the impression are forgotten (Carlston, 1980), and even when those behaviors are available, observers do not seem to retrieve them when considering or describing their impressions of an actor (Klein & Loftus, 1993; Sherman & Klein, 1994). In short, once we decide that Meg is the turtle-neck-wearing, opera-loving type, it is this impression—and not our memory for her behavior (such as note taking in the library) nor even our inference of her disposition (she is taciturn)—that guides our further judgment and behavior. One consequence of this autonomy is that impressions tend to resist disconfirmation, even when the behavioral evidence on which they were based is found to be flawed (Anderson, Lepper, & Ross, 1980; Ross, Lepper, & Hubbard, 1975).

Operating Characteristics

Despite intermittent periods of amnesia, psychologists have long known that any reasonably complete human psychology must make explicit reference to the fact that people perform complex mental operations of which they are unaware. Before the turn of the twentieth century, Helmholtz argued for the theoretical necessity of such unconscious processes, but Freud's reification of the unconscious as a purposive, willful entity led William James (1890a) to dismiss the notion as "the sovereign means for believing what one likes in psychology, and for turning what might become a science into a tumbling-ground for whimsies" (p. 163). Having no desire to see their discipline become a whimsical tumbling ground, the new American psychologists decided they could manage quite nicely without the unconscious. Just to prove it, they invented behaviorism and got along without the conscious as well. But by the middle of the twentieth century, experimental psychologists had been awakened from their behaviorist slum-

bers and were once again using the word "unconscious," albeit gingerly, and usually as an adjective rather than as a noun. This resurrection was made possible in part by that assemblage of transistors and punch cards known as the computer, which served as a respectably materialist metaphor for the mind—a machine that appeared to perform something very much like thinking but that was not, its builders swore, inhabited by ghosts.

Some of the operations that occur inside a computer are neither apparent nor accessible to the computer's user, and psychologists reasoned that some of the operations of the mind might be similarly impervious to conscious inspection without implying that people were composed of anything spookier than neurons (see Erdelyi, 1974, 1985). Once unconscious processes were no longer seen as the handiwork of a hidden entity that had its own intentions, motives, and wily ways, the next step was to demote them even further. The psychoanalytic unconscious had been a trickster and baby-sitter by turns, but the new cognitive unconscious was merely the dim servant of the more powerful conscious mind, laboring patiently behind the scenes, transforming routine patterns of information into inferential products that it could then deliver to consciousness for inspection (see Greenwald, 1992; Kihlstrom, 1987). In this way, cognitive psychology rejected both Skinner's eliminativism ("There is no unconscious mind because there is no mind") and Freud's homunculism ("There is so an unconscious and he's cleverer than you are"), and instead took the middle road between those positions—a road that brought cognitive psychology right back to the kind of unconscious inferential processes for which Helmholtz had pleaded a century before.

Helmholtz (1910/1925, pp. 26–28) had described four properties of these unconscious inferences: (1) they are made *without awareness* (the inferential process is "going on in the dark background of our memory" and "can never once be elevated to the plane of conscious judgments"); (2) they are *unintentional* (they are "the result of an unconscious and involuntary activity of the memory"); (3) they are relatively *effortless* because they result from the routine transformation of familiar patterns of information ("These inductive conclusions . . . lack the purifying and scrutinizing work of conscious thinking"); and (4) they are *uncontrollable* (they are "urged on our consciousness, so to speak, as if an external power had constrained us, over which our will has no control"). Thus, about a hundred years after Helmholtz articulated them, these four attributes were generally accepted as the critical features of what psychologists now prefer to call automatic processes (Bargh, 1989; see also Hasher & Zacks, 1979; Posner & Snyder, 1975; Schneider & Shiffrin, 1977; Wegner & Bargh, 1998, in this *Handbook*).⁴

The reemergence of automaticity as a respectable concept had a profound impact on research in ordinary person-

ology. Heider (1958) had specifically suggested that attributional rules are often used unthinkingly, yet the attributional models to which Heider's system gave rise were widely taken to imply the opposite. This implication was not made by the attributional models per se so much as it was conveyed by some of the experimental work that those models inspired. For example, in her trend-setting experiment, McArthur (1972) provided subjects with the major premises of a syllogism ("Oli liked the pizza, no one else liked the pizza, and Oli likes pizza every time he eats it") and then asked them to reason their way toward a response ("Was it the pizza or Oli that caused the liking?"). This procedure was a perfectly reasonable way to determine whether human beings could reason perfectly—that is, whether they could reason by the logic of Kelley's theory. But such techniques also seemed to equate the process of dispositional inference with the process of conscious causal reasoning. Because attribution theorists were not particularly interested in the nature of mental processes, no one explicitly championed the claim that attributions were the result of conscious reasoning. But no one expressly denied it either. Perhaps inadvertently, then, *dispositional inference* and *causal reasoning* became synonyms in the minds of most social psychologists, investigations of conscious causal reasoning were taken as investigations of dispositional inference, and few questioned the conflation of the two (cf. Hamilton, 1988; Hilton, Smith, & Kim, 1995; Langer, 1978; Zuckerman, 1989). But when the spirit of Helmholtz returned from its exile, all of that changed in a heartbeat.

Spontaneity and Awareness Students of language led the revolt against behaviorism, so is it not surprising that students of reading were among the first to invoke the newly legitimated concept of automaticity. When people read, they draw inferences that go well beyond the information given on the page. What's more, they do so unintentionally, uncontrollably, effortlessly, and without awareness (e.g., Bransford & Franks, 1971; Kintsch & van Dijk, 1978; LaBerge & Samuels, 1974; Shank & Abelson, 1977). In the late 1970s and early 1980s, several social psychologists (e.g., Pryor & Kriss, 1977; Smith & Miller, 1979, 1983; Winter & Uleman, 1984) imported this perspective into the study of ordinary personology by suggesting that when people read about human actions, they spontaneously draw inferences about characteristics of the actors. Uleman and his colleagues set out to demonstrate this hypothesis by taking advantage of Tulving and Thomson's (1973) encoding specificity principle, which suggests that events that are encoded together can serve to retrieve each other. For example, if one thinks, "Jacques-Philippe's new beret is the handsomest I've ever seen" at precisely the same moment that a warm baguette emerges from the oven, then the smell of new bread may later summon the memory of the

new hat and vice versa. Similarly, if people draw inferences about an actor's dispositions ("Opal is an uptight person") as they are reading about the actor's behavior ("Opal chewed her fingernails as she waited for the dentist"), then the trait terms that describe these dispositions ("uptight") should serve as potent retrieval cues for the sentences themselves.

Winter, Uleman, and Cunniff (1985) asked subjects to complete a series of math problems between which were sandwiched "distractor sentences," each of which happened to imply a trait. Although the trait terms were never actually mentioned in the sentences, and although the subjects had been led to believe that these sentences were irrelevant linguistic fodder meant merely to segregate the math problems from one another, the trait terms later proved to be excellent retrieval cues for the sentences—as good, in fact, as close semantic associates ("toothbrush") and far better than no cue at all. It appeared that when subjects read about a case of fingernail chewing in a dentist's office, they spontaneously generated the concept of uptightness. Furthermore, they appeared to be unaware of having done so.

This study was inventive, important, and imperfect, thus guaranteeing a slew of dissections, extensions, replications, and rejoinders. The more mundane objections (e.g., that the effect occurred at retrieval and not at encoding) were quickly dismissed (Newman & Uleman, 1990; Uleman, Newman, & Winter, 1992), and conceptual debate centered on two aspects of these spontaneous trait inferences. First, were they really spontaneous? And second, were they really inferences about traits? Social cognitivists pointed their most powerful experimental artillery at this pair of questions and, after a dozen years of heavy shelling, concluded that the answers depended on what one meant by *spontaneous* and what one meant by *trait* (Carlston & Skowronski, 1994; Carlston, Skowronski, & Sparks, 1995; Uleman, Newman, & Moskowitz, 1996). First, spontaneous trait inferences are not spontaneous if one takes the word to mean that such inferences are drawn by observers who lack even the *slightest* desire to understand the word. On the other hand, they are spontaneous if one takes the word to mean that such inferences are often drawn by observers who have no *explicit* desire to understand the actors about whom they are reading (see Bassili & Smith, 1986; Uleman & Moskowitz, 1994; Whitney, Waring, & Zingmark, 1992). The second question proved a bit stickier than the first. Although subjects apparently did spontaneously generate the word "uptight" as they read about Opal, it was not clear that this word referred to Opal's disposition rather than to the identification of her act, which was itself uptight even if she wasn't. And if it did refer to Opal's disposition, then it was not clear in what sense it did so. Many more clever experiments sought answers to this question (e.g., Carlston & Skowronski, 1994; Carlston, Skowronski,

& Sparks, 1995; Newman & Uleman, 1993; Uleman et al., 1993; Whitney, Davis, & Waring, 1994), and the findings seemed to converge on a compromise: spontaneous trait inferences are not mere identifications of action, but neither are they the sort of full-blown dispositional inferences that Jones and Davis, Kelley, or Heider had in mind. Opal and uptightness were associated in subjects' memories, but subjects did not necessarily consider Opal to be more uptight than her peers across time and circumstance.

Although the critical questions about the spontaneity of trait inferences are still being answered, the work made two notable contributions to the study of ordinary personology. First, it provoked more thoughtful and cogent debate about the meaning of the phrase "dispositional inference" than social psychology had seen for some time. It is imperative, but apparently painful, for social psychologists to say just what constitutes a dispositional inference (internality? uniqueness? breadth? durability?) and how best to test for it (ratings? ascriptions? predictions?). The work on spontaneous trait inferences did not answer these questions, but it did get the right people scratching their heads. Second, and much more importantly, it suggested that whatever dispositional inferences were, they were not (as the classic attributional work on causal reasoning seemed to imply) necessarily the result of thoughtful deliberation. Rather than being the logical product of an elaborate system of inferential rules, dispositional inferences appeared to be unintentional, instantaneous, and unconscious responses to the behavior of others—more reflex than reflection.

Ease By the end of the 1980s, social psychologists had laid out all the pieces of a jigsaw and it remained only for someone to assemble them, which Gilbert and his colleagues (Gilbert, Pelham, & Krull, 1988; Gilbert, Krull, & Pelham, 1988) did. They took Uleman's claims about the spontaneity of dispositional inferences, mated them with Trope's conceptualization of the identification and attribution links, integrated those with Quattrone's vision of the sequence of attributional subprocesses, added some novel bits, and offered social psychology a full schematic of the attributional engine (see Figure 7). Their model suggested that attribution comprises three sequential operations: a behavioral identification stage that they called *categorization*, a dispositional inference stage that they called *characterization*, and a situational adjustment stage that they called *correction*. Furthermore, they claimed that the first two of these stages were relatively automatic, whereas the last stage required conscious, controlled, effortful deliberation. According to their model, when an observer witnesses an anxious behavior, she automatically categorizes the behavior ("Opal feels anxious") and characterizes the actor ("Opal is anxious"), and then laboriously corrects the dispositional inference by considering the actor's circumstances ("But everyone feels anxious in the dentist's office,

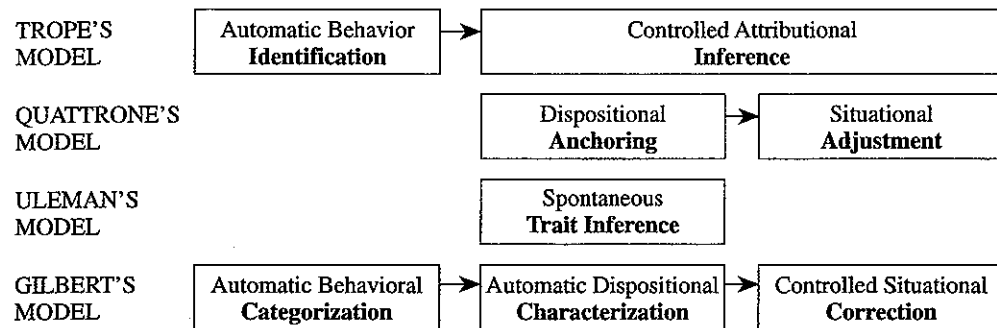


FIGURE 7 The Relations of Some Modern Stage Models.

so Opal isn't an anxious person after all"). This model put attribution theory in its place: observers did follow logical rules (such as the discounting principle), but only *after* they had automatically characterized the actor in dispositional terms. Attributional principles, it seemed, were used to repudiate dispositional inferences rather than to construct them.

The evidence that Gilbert and his colleagues offered for their model was grounded in a popular conceptualization of conscious attention as a scant resource. According to this view, people's attempts to, say, solve a crossword puzzle and recite the alphabet backward do not successfully co-occur, because each task requires the same limited resource—namely, conscious attention, size small (see Kahneman, 1973; Norman & Bobrow, 1975). On the other hand, two simple or well-practiced tasks, such as driving one's car from home to office and whistling "Oh Susannah," could be performed simultaneously because neither required much of this attentional resource (Hasher & Zacks, 1979; Posner & Snyder, 1975; Schneider & Shiffrin, 1977; Wegner & Bargh, 1998, in this *Handbook*). Gilbert and colleagues suggested that categorization and characterization were more automatic than correction, and thus their model predicted that people whose attentional resources had been temporarily commandeered by an extraneous task (such as an experimenter's request that they recite the alphabet backward) should be perfectly capable of performing the first two of these operations but should have considerable trouble performing the last. Such people should draw dispositional inferences about actors whose behaviors should have been discounted as the clear products of coercion.

In an early experiment (Gilbert, Pelham, & Krull, 1988), subjects watched a series of video clips of a female actor behaving anxiously as she ostensibly discussed several experimenter-assigned topics with a stranger. Some subjects were led to believe that the topics were anxiety-provoking ("Discuss your most intimate sexual fantasies"), others were led to believe that the topics were mundane

("Discuss your view on home gardening"), and all were asked to estimate the actor's level of dispositional anxiety. If subjects used the discounting principle, then those who believed that a fidgety woman was being forced to reveal her sexual fantasies should have considered that woman less dispositionally anxious than should those who believed that the same fidgety woman was discussing gardening (Snyder & Frankel, 1976). And this is what subjects in the control condition did. However, another group of subjects watched these videos while silently rehearsing a set of word strings (which happened to be descriptions of the very topics that the woman on the video had ostensibly been assigned to discuss). Rehearsal tasks of this sort are known to usurp attentional resources which, according to the Gilbert et al. model, should have rendered the correction operation impossible. Indeed, subjects who rehearsed word strings considered the actor to be equally dispositionally anxious regardless of the topics she was ostensibly discussing. The irony of this result was that those subjects who were mentally rehearsing the discussion topics were least likely to use them to correct their dispositional characterizations of the anxious actor.

Inevitability This so-called undercorrection effect proved to be robust across a range of behaviors and manipulations. In study after study, observers who were denied the opportunity to think deeply about an actor's behavior tended to draw dispositional inferences about the actor, even though these observers had been informed and could remember that the actor's behavior was coerced. Perhaps because the model was such a Frankenstein monster—a patchwork of previous models, each of which had proven reliable before being incorporated—its basic claim about the sequence of operations was accepted without much fanfare. Its claims about the characteristics of these operations received somewhat closer scrutiny. At about the same time that the model debuted, researchers who had built their theories (and their reputations) on the concept of automaticity had begun to notice a disturbing noise beneath the floor-

boards: the four attributes of automatic processes—intentionality, effortfulness, awareness, and controllability—did not always hang together. That is, processes that were spontaneous were sometimes controllable, those that were effortless might still be amenable to conscious inspection, and so on. Automaticity was not a monolithic attribute of mental operations, but rather an amalgam of attributes that could be distilled into elements that did not imply each other (Bargh, 1989). Gilbert and colleagues had claimed that dispositional characterization was “relatively automatic,” but they had examined only one of the features of automaticity: effortlessness. Taken together, Gilbert’s and Uleman’s work suggested that dispositional inference participated, at least to some extent, in three of the four characteristics of an automatic process. But what of the remaining characteristic? Were dispositional inferences inevitable consequences of behavioral observations?

From Asch’s (1951) beleaguered conformists to Darley and Latané’s (1968) reluctant bystanders, social psychology was rife with examples of people taking the behavior of others (“No one is helping”) as a measure of the situation (“I guess I’m not supposed to help”) rather than as a measure of the actor (“Everyone here is a coward”). These demonstrations led Krull to suggest that behavior may spontaneously, unconsciously, and effortlessly be taken as an index of either an actor’s dispositions or an actor’s situation, and furthermore, that the observer’s epistemic goals would determine which of these characterizations the behavioral observation would evoke. Gilbert and colleagues had suggested that when observers have some interest in understanding actors, they naturally categorize the actor’s behavior (“The fat man is grinning from ear to ear”), characterize the actor in dispositional terms (“He’s a jolly fellow”), and then, if they are willing and able, correct that characterization by considering the actor’s situation (“But, of course, he’s leaving the theater after seeing a comedy, so perhaps he isn’t a particularly jolly fellow after all”). Krull reasoned that when people are more interested in understanding a situation than an actor (e.g., when they are about to enter the theater rather than meet the grinning fat man), they may characterize the stimulus (“The movie must be wonderful”) and then, if they are willing and able, correct this characterization with information about the actor’s dispositions (“But, of course, fat people are always jolly, so maybe the movie isn’t so funny after all”). In other words, people first take behavior as a manifestation of the element—the actor’s disposition or the stimulus to which the actor is responding—about which they most want to know, and only then consider the role that the other element may have played in causing the behavior. Observers are more flexible than Gilbert and his colleagues assumed, inasmuch as the sequence of operations they execute depends on epistemic goals that may themselves be determined by any number of transient factors. For example, observers may be

more inclined to understand the actor’s dispositions than the actor’s situation when they expect to interact with the actor later, or when the experimenter has specifically asked them to diagnose the actor’s dispositions, or when they have prior knowledge of the actor’s situation and thus have nothing other than the actor’s dispositions left to discover.

Krull’s studies generally bore out these suggestions (Krull, 1993; Krull & Dill, in press). For example, in a clever mating of Gilbert, Pelham, and Krull’s (1988) methodology with Quattrone’s (1982), Krull and Erickson (1995) showed subjects a series of video clips of a female actor behaving anxiously while she was ostensibly discussing several experimenter-assigned topics with a stranger. Some subjects were led to believe that the actor was dispositionally anxious, others were led to believe that she was dispositionally calm, and all were asked to determine how anxiety-provoking the topics must be. Those who believed the woman to be dispositionally anxious concluded that the topics must be less anxiety-provoking than did those who believed the woman to be dispositionally calm. However, subjects whose cognitive resources were depleted by a tone detection task considered the topics to be equally anxiety-provoking regardless of the actors’ dispositions. These results suggest that observers are quite capable of using the behaviors they have categorized to characterize unknown situations (“She looks so nervous, I guess that must be a dentist’s office”) and then using information about the actor’s dispositions to correct that characterization (“Of course, Opal is so uptight that she may just be waiting for her dry cleaning”). The lesson for students of ordinary personology is that dispositional inferences may be easily launched and easily completed, but they are not inevitable.

Content Matters

Research on the sequence and the characteristics of mental operations suggested that ordinary personology is not a puzzle-solving enterprise in which people thoughtfully apply abstract rules to syllogistic conundrums, but a parade of qualitatively distinct mental operations that produce judgments, beliefs, and impressions. The conscious observer trails the parade, cleaning up after the elephants, following, fixing, and occasionally stepping in the conclusions that his mind seems so naturally to produce. Attributionists described the rules by which observers make these inferences, and social cognitivists described the sequencing and characteristics of the mental operations by which those rules are implemented. For all their differences, however, attributional and social cognitive models tended to share one feature: emptiness. Both classes of model called on the concept of *behavior* as though that word described a collection of perfectly interchangeable events. Voicing an opinion, kicking a nun, weeping uncon-

trollably, and scoring a touchdown were considered only superficially different by attribution theories, which promised that each could be penetrated by the same logic, and by social cognitive theories, which promised that each could be processed by the same mental operations. Like algebraic equations into which any real number could be substituted, both sorts of theories eschewed content, treating the differences among moral lapses, attitudinal expressions, emotional displays, and athletic performances as a matter of mere symbol substitution, vaguely suggesting that these actions had more in common than not.

Domain Specificity But when attribution theorists tried to apply their general theories to specific instances, they found, as general theorists generally do, that content mattered. For example, Weiner et al. (1971) investigated how observers make attributions for an actor's achievements rather than for an actor's statements of opinion or emotional expressions, and found they had to nip, tuck, and tailor the general attributional principles to fit the bulges and lumps of this special domain. There were features of striving, scoring, beating, yearning, competing, losing, and winning that were simply unique to the attribution of performance. Similarly, Reeder and Brewer (1979) noticed that behaviors in different domains mapped onto their respective dispositions in different ways. For example, the magnitude of an actor's laughter may have a linear correspondence to the magnitude of her dispositional mirth such that a complete lack of laughter indicates a dyspeptic demeanor, an occasional, well-timed giggle indicates a moderately sunny disposition, and incessant howling indicates pathologically good cheer. But the same linear mapping does not seem to hold in, say, the moral domain. Dishonest behavior clearly indicates a dishonest disposition, but honest behavior can indicate either an honest or a dishonest disposition, which is to say that both the saint and the sinner generally fail to rob most of the banks they pass and thus bank non-robbery is not a particularly revealing behavior. Extroverts can behave introvertedly, but not vice versa, and winners can lose more easily than losers can win. Reeder and Brewer described a variety of behavior-to-disposition mappings, and suggested that observers learn through experience which of these mappings should be applied when drawing inferences from behavior in different domains.

These attempts to address the matter of content were prescient. They were not, however, offered as challenges so much as supplements to the more general attributional models. Attributionists recognized that high-level models always sacrifice precision for parsimony, and that the classic attribution theories were meant to describe the inferential rules that were common to different domains without denying the possibility of consequential idiosyncrasies. Let there be general attribution theories and let there be domain-specific attribution theories; let the former imply the

latter, let the latter detail the former; and let the whole lot coexist peacefully, amen. Alas, the problem refused to stay fixed, because if observers made attributions somewhat differently in different behavioral domains, a troubling question arose: Which rules, the general or the specific, did the observer know and use and follow? That question proved to be much more annoying than anyone could have guessed.

Tacit Representation More than one social psychologist has lectured on attribution theory, only to have a puzzled student raise his hand and ask, "Excuse me, Professor, but if ordinary people use the law of noncommon effects, then how come I don't understand it?" The answer to that innocent question was not always forthcoming, because attribution theorists had not said what it means for a person to *know* or *use* or *follow* a rule. Heider had noted that people could not always articulate the attributional rules they used, and subsequent attribution theorists skirted the issue by making no claims about how those rules were implemented. But the fact of domain specificity brought the puzzled student's question to center stage, and some social cognitivists answered it boldly by suggesting that people may not use general attributional rules at all, but may instead use "domain-specific inference rules" or "real-world knowledge structures" or "event scripts" or "exemplar-based reasoning" (e.g., Abelson & Lalljee, 1988; Hilton & Knibbs, 1988; Read, 1987; Smith & Miller, 1979). Observers, they claimed, may not consult a general discounting principle, but may simply apply their knowledge of debaters or job applicants or moral transgressors when they try to solve personological problems in each of these domains. How can we tell whether an attributional inference ("That debater isn't any more pro-Castro than I am") was achieved by using a domain-specific rule ("Debaters are like lawyers and novelists—they don't believe a word that comes out of their own mouths") or a domain-general rule ("Do not attribute behavior to dispositions when there exists a plausible situational cause")?

The generalists were tempted to point out that observers' inferences are similarly structured across a wide variety of domains, but such similarity does not necessarily demonstrate that the observer is calling on a general, abstract, content-free rule. For example, an observer may use her experience in one domain ("Guys have said all sorts of nice things to get a date with me in the past") to reason about behavior in that same domain ("I bet this one doesn't *really* think my poems are all that terrific"), and then use her experience in a second domain ("The fastballs always seem faster when the wind blows from right field") to reason about behavior in that second domain ("I bet this pitcher doesn't have such a hot arm after all"). These inferences may have similar structures (i.e., both may, from the psychologist's point of view, involve discounting the situational causes of behavior), but the similarity may result be-

cause the problems they are meant to solve are themselves structured similarly—and *not* because the two inferences relied on a single, general inferential rule. Just as people who memorize the multiplication tables may answer arithmetical questions correctly without knowing or using general rules of arithmetic, so might an observer make appropriate inferences about the dispositions of suitors and pitchers without knowing or using general rules of attribution. So, if similar inferences across varied domains do not necessarily indicate use of a general rule, then what does?

An answer to this question requires that we first say what we mean when we claim that a person *uses a rule*. At one time such a statement meant that somewhere in the representational medium of the mind, the rule was explicitly written in a mental language, and thus questions about whether it was written in general language (“When two causal candidates, Φ and Ω , are equally likely causes of effect Ψ , then . . .”) or in particular language (“When a guy says he likes your poem, check and see if he’s lonely”) made good sense. Questions like these no longer make such good sense, because in the last decade our understanding of mental representation itself has undergone some important changes (see Smith, 1998, in this *Handbook*). When social psychologists say that a person knows or uses or follows a rule, they have intended to assume that the rule is represented *explicitly* such that “there actually exists in the functionally relevant place in the system a physically structured object, a *formula* or *string*, or *tokening*” that represents the rule (Dennett, 1987, p. 216), or that the rule is represented *implicitly* such that the rule is “implied logically by something that is stored explicitly” (Dennett, 1987, p. 216). But at least since Ryle (1949), philosophers have recognized a third alternative—namely, that rules can be represented *tacitly*. As Dennett (1987) explains:

A tiny hand calculator *gives one access to* a virtual infinity of arithmetical facts, but in what sense are *any* arithmetical facts “stored” in it? If one looks closely at the hardware, one finds no numerical propositions written in code in its interior . . . Its inner machinery is so arranged that it has the fancy dispositional property of *answering arithmetical questions correctly*. It does this without ever *looking up* any arithmetical facts or rules of operation stored in it. (p. 221)

This idea has begun to make better sense to psychologists because in the last few years psychologists have begun to build models of “inner machinery” that is “arranged” by experience (McClelland & Rumelhart, 1986; Rumelhart & McClelland, 1986; for examples in social psychology see Kunda & Thagard, 1996; Read & Marcus-Newhall, 1993; Shultz & Lepper, 1996). In a connectionist network, for example, particular experiences create new configurations of weighted connections, and these new configurations *are* dispositional tendencies to deal with

later experiences in particular ways. Over time, such a network will “relax” into rule-following behavior, but nowhere in the network will the rule be noted, programmed, printed, inscribed, etched, or implied. Nonetheless, the network will *abide* by the rule and *obey* the rule and *follow* the rule and a human being who happens to be loafing by the water cooler and observing the network will tend to say to other human loafers that the network *knows* or *uses* the rule, because that’s the way human loafers like to talk (see Dennett, 1987; Gilbert, 1992; Ramsey, Stich, & Garon, 1991; Rumelhart & Norman, 1985; Smith, in press).

The notion of tacit representation has important implications for ordinary personology and its contentless theories, particularly for the question of whether people use general or domain-specific rules. People sometimes solve problems by consciously consulting and applying rules that they have memorized (“Subtract the rightmost numbers and carry the remainder to the left” or “Red next to yellow can hurt a fellow”; see, for example, Larrick, Morgan, & Nisbett, 1990), and in such instances, it probably makes sense to say that the person *knows* the rule (inasmuch as she can recite it) and that the person *uses* the rule (inasmuch as he explicitly considered the rule when deriving a solution to a particular problem). In such instances it also makes sense to ask whether the rule that the person knows and uses is general (inasmuch as it covers a variety of mathematical problems) or domain-specific (inasmuch as it covers certain striped snakes). But these sorts of sensible questions make no sense when rules are represented tacitly, because tacit rules are merely descriptions of the person’s behavior, much as the law of gravity is a description of the behavior of every massive object on Earth but is neither written nor represented anywhere within those objects. In the case of tacitly represented rules, it is perhaps best to say that the rule is *followed* rather than *known* or *used*.

If we allow the words *know* and *use* and *follow* to take these special meanings, we can safely draw three conclusions about the rules of ordinary personology. First, people *know* and *use* a variety of domain-specific inference rules in their everyday interactions. Customs officials are explicitly taught to look for the signs of dress and demeanor that will betray a smuggler, and waiters have no trouble explaining how they spot the big tippers. Second, people *know* and, on some occasions, *use* domain-general inference rules that they have been taught in the classroom or that they have extracted by reflecting on their own experiences. But third, people can *follow* rules that they neither *know* nor *use*, and thus their allegiance to a rule does not tell us how that rule is represented. At present, social psychologists know almost nothing about how personological rules such as the discounting principle are represented, and this pocket of ignorance promises to attract much attention in the next decade. Are attributional principles learned and considered and applied, or is attributional logic merely something that a human brain, by dint of its architecture,

does—much as a ball “does” rolling? Does the discounting principle *describe* the logic that people use, or *is* it the logic that people use? For now, we can safely say that attributional rules can be thought of as prescriptions for how people *ought to think* or as descriptions of how people might *actually behave*, but not as examples of *what* people think *with* or *about*. Attributional rules are not crib notes, either general or specific, that are written in the observer’s cortex and consulted, either consciously or unconsciously, when an inference is required.

The Next Tradition

Attribution theory dominated social psychology’s research agenda for nearly two decades, but by the end of the 1970s it had clearly plateaued and was headed for decline. The attributionists’ static, logical models simply could not compete with the social cognitivists’ dynamic, process-oriented models for the attention of young researchers, who had no intention of staying indoors and watching through the living-room window as the cognitive revolution went by. Although the story of the attributional tradition is quite naturally told in the past tense, it is not clear how best to talk about the social cognitive tradition. Some would say it is now mature but vital; others would say that news of its passing has been slow to reach some quarters. Perhaps the truth is in the middle—or as Frank Zappa might have said, social cognition isn’t dead, but it sure smells funny.

Whether hale, defunct, or malodorous, the social cognitive tradition has clearly lived up to its promise, and its core contributions, like the core contributions of the attributional tradition, will be part of social psychology’s legacy for a long time. Indeed, this edition of the *Handbook* does not offer a chapter on the cognitive approach, because that approach has so thoroughly infused every nook and cranny of social psychology as to render a single treatment moot. In the previous edition of the *Handbook*, Markus and Zajonc (1985) presaged this development: “This adoption of the cognitive view among social psychologists has been so complete that it is extremely difficult for most of the workers in the field to conceive of a viable alternative” (p. 137). Nonetheless, like all movements and schools and approaches, the social cognitive tradition has not been without the kinds of disappointments that ultimately provide the impetus for change, and that change is clearly on the horizon. Whether the social cognitive tradition has been or will be transformed by evolution or deposed by revolution, the tradition that replaces it will face at least three challenges: methods, models, and mergers.

Methods One of the social cognitive tradition’s clearest disappointments has been its unrelenting passion for the tidy experimental paradigms bequeathed to it by Asch and

improved by modern cognitive techniques, but about which the intellectual children of Heider and Brunswick have never been able to muster much enthusiasm. The trait adjective paradigm, critics have contended, is problematic both in terms of what it says about ordinary personology and in terms of what it does not say. Zajonc (1980a) articulated the first of these concerns:

Because we cannot assume a one-to-one correspondence between language and reality, we may not take it for granted that the same principles of social perception will be generated by studying words as by studying the actual social objects for which these words stand. (p. 192)

The standard social cognitive experiment has stayed remarkably faithful to the Asch paradigm inasmuch as it requires observers to do nothing but read a list of traits and form an impression of the actor whom those traits describe. Although reading is a distinctly important human skill, it may be importantly distinct from other mental abilities (Barr et al., 1991; Pearson et al., 1984; Rayner & Pollatsek, 1989), such as those that enable people (even illiterate people) to understand each other in their daily lives (see also McArthur & Baron, 1983). Some social psychological phenomena that occur when words are used as stimuli (Devine, 1989) may disappear altogether when actual behaviors are substituted for those words (Gilbert & Hixon, 1991), and some protracted debates (e.g., the origin of the primacy effect) may have more to do with how readers read word lists than with how observers make sense of an actor’s behaviors on different dimensions over time. Although some of the intellectual leaders of the social cognitive movement once dismissed such concerns with a hand wave (Ostrom, 1984), others have recently come to share the attributionists’ concerns about the adequacy of their methods. As Wyer and Gruenfeld (1995) note:

Much of our theoretical and empirical knowledge about social information processing has been obtained under laboratory conditions that only faintly resemble the social situations in which information is usually acquired in everyday life. In particular, highly controlled experimental situations have typically been constructed in which the influence of social contextual factors of the sort that surround the acquisition and use of information outside the laboratory is minimized Unfortunately, contextual features are likely to interact with the literal meaning of the information in determining the implications that people derive from it. Consequently, a consideration of the semantic features of information that people receive in a social context is often insufficient to understand how the information is interpreted and how it is likely to influence judgments and behavior. (p. 48)

In short, some social psychologists have wondered just how tall a theoretical structure one can build on this sort of experimental foundation without fear of collapse, and that concern seems well justified. The trait adjective paradigm may or may not tell us much about how ordinary personologists unravel the mystery that is the other, but in the coming years the burden of proof will undoubtedly shift onto those who would use such paradigms exclusively and away from those who would question them.

If some critics have worried about what the trait adjective paradigm shows, even more have worried about what it ignores. Asch's (1946) experimental method came with this clear warning:

A far richer field for the observation of the processes here considered would be the impressions formed of actual people. Concrete experience with persons possesses a substantial quality and produces a host of effects which had no room for growth in the ephemeral impressions of this investigation . . . [For example] it was a constant feature of our procedure to provide the subject with traits of a person; but in actual observation the discovery of the traits in a person is a vital part of the process of establishing an impression. (pp. 288-289)

Clearly, Asch worried that his paradigm eliminated the very noise that social psychologists ought to find joyous, and though the social cognitivists improved the precision of that paradigm with techniques borrowed from cognitive psychology, they did little to heed Asch's warning. Thus, the turbo version of Asch's paradigm suffered from much the same defect as the original. As Markus and Zajonc (1985) noted:

[T]he appropriation of theoretical and methodological sophistication from information-processing science by social psychologists enables them only to study those phenomena of social cognition that are basically and fundamentally the same as those that are studied by experimental psychologists. And for the most part, experimental psychologists study cognitive phenomena that from the social psychological perspective appear quite restricted Thus the study of social cognition must reach beyond the simple experimental paradigms of information processing. (p. 213)

It is so easy to think of pressing social psychological problems that defy investigation by such paradigms that there is hardly a need to list them. But rather than developing methods that would shed light on the problems that pressed, social cognitivists tended to develop problems that were naturally illuminated by the techniques already at their disposal, thereby allowing their shoes to determine the size of their feet. For example, the social cognitivists

paid especially close attention to the structure of "person memory" while worrying less often about behavior, emotion, motivation, social interaction, and relationships. They did not consider these problems unimportant so much as experimentally intractable—which, given Asch's methods, they were. Many of the features of ordinary personology were not amenable to the sort of rigorous analysis that the social cognitive tradition required, and thus these features received less attention than their significance suggested they should.

These two shortcomings of the social cognitive method converge on a single question: when are the costs of scientific precision worth paying and when do they bankrupt the discipline? Social psychologists are just now beginning to do those calculations, and the results will shape the next tradition. The strengths of the social cognitive tradition have been its concern with mental process and mental representation, and its quest to find a place for ordinary personology within a more general model of human cognition. Its weakness has been in its failure to move beyond paradigms that provide exact answers to small questions. Social cognitivists do not all wear matching hats, of course, and in recent years some have attempted to blend the richer elements of the attributional tradition (e.g., social interactions, dynamic behavioral stimuli, manipulations of motivational states) with the more precise elements of the social cognitive tradition (e.g., divided-attention tasks, reaction time measures, implicit memory measures). Such hybrids are promising and may provide a template for the methodology of the next tradition. In addition, recent advances in the analysis of real social interactions (e.g., Ickes et al., 1986; Kenny, 1994) suggest that some precise answers can be extracted from naturalistic data, and this, too, is a healthy innovation. The methodological pendulum is always in motion, and today it seems to be swinging away from the hard line. Whether it completes the full sweep of its arc or stops somewhere around the middle, the next tradition must cultivate methods that allow students of ordinary personology to speak with confidence about matters of substance.

Models The rallying cry of the social cognitive movement was that models of ordinary personology must be grounded in more general models of human cognition, and this declaration of dependence seemed so sensible, so obviously right, that few stopped to consider its potential dangers. By tying their models of ordinary personology to the more general information-processing models of the day, the social cognitivists ironically ensured that their brand of social psychology would become archaic at precisely the same time that the cognitive psychology on which it was parasitic did. That time may have arrived. It is notoriously difficult to take the pulse of the moment, but it seems clear that in the last few years cognitive psychology has begun to

shed its dry skin—to lose interest in what Fodor (1983) has called “relatively banal boxology” and to transform itself into the decidedly wetter discipline of cognitive neuroscience. Anderson (1976) argued some years ago that “behavioral data cannot uniquely determine the internal structure of the black box that generated it” (p. 10), and many cognitive psychologists are now beginning to wonder whether the development of biologically unconstrained models of that box might not be a fool’s errand—especially when technology has opened the lid of the box for more direct inspection. Thus, social psychologists who joined the cognitive revolution as modelsmiths may soon find themselves wearing the uniform of an army whose troops have gone AWOL, leaving behind nothing but their unspent ration of boxes and arrows.

If in the next decade or so cognitive psychology does bug out to Broca’s area, then social psychologists will deal with their abandonment issues in one of three ways: They will tape up the boxes, polish the arrows, and stay where they are; they will follow the leaders ever deeper into the brain; or they will return to higher, more traditional levels of analysis. The first option is worrisome. Social psychology *could* decide to be the science that describes psychological phenomena at the operational level without regard for biological constraints on those descriptions, but those models would suffer from the very malady that plagues cognitive psychology today. If making up stories about nodes and bins and operators and activation levels proves to be an unprofitable theoretical enterprise for those who majored in it, it is hard to see why social psychology would want to expand its minor. The second option is more promising but less likely. There are lines over which a discipline cannot cross without surrendering its identity, and should social psychology marry the neurosciences, it should probably be prepared to give up its maiden name. The facts that sociality emerges at higher levels of analysis and that social psychologists are a proud race make it difficult to imagine most social psychologists venturing very far along the neurobiological road. Indeed, it is not clear that science itself would benefit if every discipline were to move in the same direction at the same time until everyone met in a heap at the quark.

The third option may seem on its face like an admission of defeat, but all generals know that retreat can be a brilliant military maneuver. Consider attribution theories, for instance, which were entirely algorithmic inasmuch as they described rules without concerning themselves with the mental processes by which those rules were implemented. Although this approach was ostensibly the ailment for which only social cognition could provide a cure, it is ironic to note now that the changing face of cognitive psychology threatens to neutralize social cognition but poses no threat to attribution theories, which continue to do the work for which they were intended regardless of how the

brain turns out. In other words, a theory’s failure to commit itself to a particular implementational scheme can be a prudent rather than an irresolute move, and the fact that few theorists have ever made that move explains why we still admire the psychology, but merely chuckle at the biology, of geniuses from Aristotle to James. The typical theoretical analysis offered by an article in the *Journal of Personality and Social Psychology* has, over the last twenty years, been driven down one level and then one more, and when one hits bedrock, one ought at least to *consider* packing up the drills and going home.

This may seem like a rather unambitious direction for the next tradition to take. Can our discipline do nothing fancier than the backstroke? Of course it can, but a return to higher levels of analysis need not be thought of as a rerun or a recapitulation, but rather as part of the slow process of zeroing in on our adult identity. That process is not a linear march in which we aim for the horizon and never look back, nor a circular stroll in which we continuously cycle between a few familiar trends. It is a helical journey. We rediscover old truths, resurrect old ideas, and refurbish old approaches—but we do so with fresh insights and new techniques that make both us and the ideas better with each pass. As Jones (1983) noted:

Perhaps it is inevitable, somehow built into the nature of our field, that our understanding evolves in a series of circular loops. We sometimes appear to be always touching the same bases as we circle—but perhaps they are a little bit further up in some vertical insight dimension and the spiral is a better metaphor than the retraced circle. (p. 7)

Like sightseers climbing a spiral staircase inside a glass tower, we see the same scenery again and again, but with each return we view it from a higher place. As the next tradition struggles to find the level of analysis at which social psychology can make its most profound contribution, it should at least think about going where it has been. The beauty of climbing a spiral staircase inside a glass tower is that sightseers like us can go forward, backward, and upward, all at the same time.

Mergers Over beer, students of ordinary personology can be heard to lament the fact that there is not a unified science of ordinary personology that uses a single language and a standard set of techniques to explore topics such as nonverbal communication, categorization and stereotyping, attribution, impression formation, person memory, and interpersonal attraction. That’s not just the alcohol talking. Each of these topics has spun an independent research tradition around itself, and work in each tradition naturally bears the individuating marks of the scientists who create it. Research on impression formation has been shaped by

the cognitive and verbal learning traditions, stereotyping was for some time the province of sociologists and European social psychologists, the study of nonverbal communication cannot hide its ethological roots any more than attribution theory can ignore its Gestalt parentage—and so on. Those who try to straddle traditions soon learn that each of these topics is so complicated that few can hope to master the different theories and methods necessary to work in more than one. Because of their inherent difficulty and divergent origins, these traditions rarely agree about what constitutes a proper question, much less a proper answer. And so, on the cusp of a new century, the *Handbook of Social Psychology* offers a host of separate chapters on a host of separate problems to a host of bewildered readers who naively thought that stereotyping and person memory had something to do with causal attribution.

How might these different topics be connected? Attribution theories attempt to explain how a stimulus (an action) engenders a pair of responses (an identification and then a dispositional inference), and social cognitive theories attempt to explain how those responses engender others (an impression). But ordinary personology includes many other kinds of stimuli and many other kinds of responses (see Figure 8). For example, an actor who is in a deep coma, and hence performing no observable behaviors save breathing, still has an appearance (a crucifix or a mohawk) that allows him to be identified as a member of a category (a priest or a punk), which then allows observers to draw inferences about the actor's dispositions (he is religious or rebellious) without witnessing a single action. Research on how people draw dispositional inferences from appearance cues and from category membership is vast enough to warrant separate treatment in this *Handbook* (see Crocker, Major, & Steele, 1998; Fiske, 1998). Similarly, just as observers may use stimuli that are not behaviors, so may they have responses that are not dispositional inferences. For example, observers may have affective or evaluative reactions to behavior (e.g., "God yes!" or "Ouch!"), and in some instances these reactions are uninformed by—and

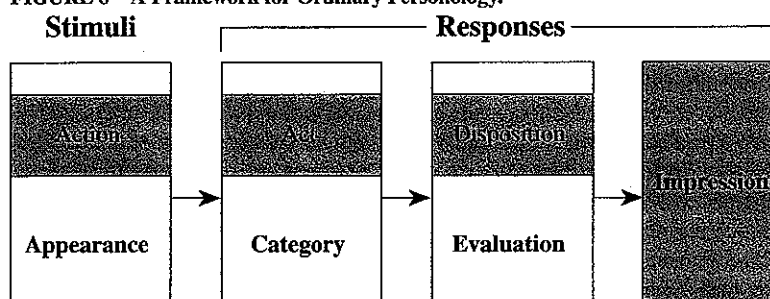
may even precede—dispositional inferences (Zajonc, 1980b). The processes that determine whether observers will like or dislike actors have also been so thoroughly investigated that they warrant separate treatment in this *Handbook* (see Berscheid & Reis, 1998; Petty & Wegener, 1998; Zajonc, 1998). The point is that understanding others involves much more than the actions-to-impressions sequence on which this chapter has concentrated, and the fact that these other processes are relegated to other chapters should not obscure the fact that they are an integral part of ordinary personology.

Perhaps, as many Americans believe, diversity is strength, and the conceptual jambalaya created by history and circumstance is to be savored. Perhaps, but probably not. It seems much more likely that an understanding of the components of ordinary personology would be significantly enhanced by an understanding of their interplay, and that many of the disagreements that divide traditions would turn out to be based on semantics, parochialism, and ancestral feuds. The next tradition, like the traditions before it, must answer questions about how *intelligent* and *impulsive* go together, about how smiles are sent and winks are received, about how Hispanic Republicans judge female lumberjacks. But if all of this is to be more than babble, the next tradition must also provide a way for us to ask and answer these questions in a single tongue.

ICHHEISER'S CHILDREN: ERRORS IN ORDINARY PERSONOLOGY

The objective approach was singularly concerned with the question of how accurately people judge dispositions, and although the logical approach abandoned the old standard of accuracy, it retained a vestige of the old concern in its quest to determine how scrupulously people follow inferential rules. Models from the attribution era formulated these rules, and models from the social cognition era put these rules in context by suggesting that they are invoked rather

FIGURE 8 A Framework for Ordinary Personology.



late in the process of making personological judgments. But all of these models agreed that, at some time, rules are invoked. Does this mean, then, that people always "get it right" when they seek to understand their neighbors, lovers, foes, and friends? One need not have spent too much time on the planet to know that human beings make mistakes about one another and that the consequences of these mistakes are sometimes amusing, often trivial, and occasionally tragic (see Gilovich, 1991). This fact has not been lost on social psychologists, who, almost from the time of the discipline's inception, have sought to understand the form and function of errors in ordinary personology.

Sources of Error in Ordinary Personology

Perhaps the logical approach's emphasis on rational baseline models could not help but culminate in the programmatic comparison of those models with ordinary inference. Perhaps by using experiments to expose the shortcomings of ordinary inference, social psychologists demonstrated the crucial role that scientific methodology plays in uncovering truth and thereby justified its practice. Perhaps the study of error was fueled by an altruistic desire to cure inferential ills and promote more harmonious personal and international relations. Perhaps it was just fun to watch people do something silly. For whatever reason, the logical approach triggered an explosion of research on inferential error, and the list of illusions, foibles, flops, and bloopers to which ordinary people were apparently prone became rather long. The work of Tversky and Kahneman (1974) and Nisbett and Ross (1980) gave the study of error a firm theoretical foundation, and soon, if there was a mistake to be made, someone was making it in the presence of a social psychologist.

What kinds of mistakes do people make? As Allport (1961) noted, "Almost every conceivable way of committing an error in thinking is at the same time a way of misjudging people. Superficial observation, faulty memory, erroneous premises, mistaken inferences, superstitions, prejudice, rationalization, projection—the number of possible missteps is too great to classify" (p. 512). If the number of missteps was too great to classify thirty-five years ago when little was known about the steps or about what qualified as a miss, then today a complete taxonomy would require pages and patience well beyond the present supply. Nonetheless, a long view of the literature suggests that the mistakes people make when they attempt to understand others tend to involve four general phenomena, which we might call *idealism* (people see things as they expect them to be), *egotism* (people see things as they want them to be), *realism* (people think they see things as they are), and *circumstantialism* (people think about only the things they

see). Each of these phenomena has its own short story (see Gilovich, 1991, for a similar analysis).

Idealism Timid philosophy professors make unlikely revolutionaries. But in 1781, Immanuel Kant (1781/1965) described a theory of mind that was such a radical departure from what had gone before that several centuries of epistemological thought were instantly neutered by its arrival. At the heart of Kant's theory was the claim that perception is not merely a physiological process by which the world is faithfully projected on the brain as if by a series of mirrors. Rather, perception is a psychological process that uses the perceiver's knowledge of the world to re-present the world to the perceiver. Kant (1781/1965) argued that it could be no other way: "Intuitions without concepts are blind The understanding can intuit nothing, the senses can think nothing. Only through their union can knowledge arise" (p. 93). Kant believed that perception was structured by innate knowledge of time, space, object, causality, and the like, and Hegel (1807/1977) extended this claim by suggesting that perception could also be influenced by acquired knowledge, such as a person's momentary beliefs and attitudes. These versions of idealism underwrite almost all schools of modern psychology, which are unabashedly constructivist (Hundert, 1989). The constructivist perspective suggests that perception is a generative process in which old knowledge (preexisting beliefs) and new knowledge (incoming sensory data) are combined into a sensible whole, which is then experienced by the person as reality.

Research has shown that people's perceptions—and to an even greater extent, their judgments and inferences—are guided by their internal representations of previous experience (which psychologists variously call schemata, theories, beliefs, hypotheses, or, during an occasional fit of clarity, expectancies), and these representations seem to influence experience in two basic ways. First, they may promote *assimilation*; that is, people may see the world as their previous experience suggests the world should be rather than as it is. For example, knowing that the apples in a bowl were red this morning may cause a person in a dimly lit room to see the apples as red in the evening, even though physical analysis proves that the evening light does not allow a human being with normal vision to discriminate red from brown. Such expectancies not only shade and shape experience, but actually create experience *ex nihilo*: the person who glances at the apple may see a stem where a stem ought to be but isn't. The second way in which expectancies affect experience is that they may promote *contrast* instead of assimilation and may lead people to exaggerate the differences between what they see and what they expected to see. For example, a ten-pound apple may look like a fifteen-pound apple to a person who glances appleward expecting to see an ordinary Rome Beauty. Experi-

ments suggest that assimilation tends to occur when the world is roughly as one expects, and contrast tends to occur when it is obviously, patently, and grossly different (Parducci, 1965; Sherif & Hovland, 1961). Because the expectations of creatures that have survived a few million years of evolution are typically right, assimilation tends to be the more typical of the two effects.

Common sense suggests that accuracy depends on seeing the world as it is and not as one thinks it should be; thus a common and sensible reaction to the constructivist position is to fret about assimilation and rush to devise programs to cure it. Psychologists have generally resisted that impulse and have instead argued that assimilation is a valuable tool without which perception would be at least difficult and perhaps impossible (Allport, 1954; Bruner, 1957). A world in which one could not see red apples in the evening and could not tell whether they were stemmed without close inspection would be an exasperatingly slow world indeed. Nonetheless, for all its pragmatic value, in some circumstances the assimilative tendency can lead the observer to err, and the documentation of such circumstances has been a mainstay of research in ordinary personology. Lorge (1936) showed how the perceived veracity of a communication could be enhanced by the prestige of the communicator, Kelley (1950) showed how labeling a communicator as "cold" could cause students to dislike his lecture, and several hundred—perhaps even several thousand—studies have appeared in the last fifty years that offer variations around the same point (see Fiske & Taylor, 1991; Taylor & Crocker, 1981). Although the corpus of this work is large and nuanced, together it seems to suggest that assimilation leads to error when the observers' expectancies are irrelevant to the judgment or just plain wrong.

For example, research on construct activation has shown that when irrelevant information is activated, it can serve as a kind of unconscious expectancy that distorts the observer's perception of the actor's behavior. Brief exposure to sentences that convey hostility can lead people to feel negatively about an actor (Srull & Wyer, 1979), just as brief exposure to the word "brave" can cause people to think of an actor's behavior as adventurous instead of reckless (Higgins, Rholes, & Jones, 1977). Although the tendency to use recently activated constructs to interpret ongoing experience is generally adaptive (Anderson, 1990), social psychologists have concentrated on those instances in which irrelevant expectancies undermine rather than promote accurate personological inferences (see Higgins, in press-b; Higgins & Bargh, 1987; Smith, 1997, in this *Handbook*; Wegner & Bargh, 1997, in this *Handbook*; Wilson & Brekke, 1994). Expectancies may also undermine accuracy when they are relevant but wrong—just wrong enough to cause assimilation but not quite wrong enough to produce contrast. Stereotypes are good examples of such "almost wrong" expectancies because they are rarely so

absurd as to be easily and obviously disconfirmed (e.g., most whites do not believe that most blacks are profoundly retarded) but they often suffer from inaccuracies born of ignorance or antipathy (e.g., many whites do believe that blacks are unintelligent; see Devine & Elliott, 1995). When the owner of an inaccurate expectancy (a prejudiced white teacher) observes an actor's behavior (the failure of a black student to answer a question quickly), then that behavior may be assimilated to the inaccurate expectancy ("He was very slow") and an inaccurate conclusion about the actor may result ("He is very stupid"). The process by which expectancies based on race, age, and gender shape the observer's personological inferences has been so well studied, and its consequences have been deemed so vitally important to society, that it warrants its own detailed treatment in this *Handbook* (see Brewer & Brown, 1998; Deaux & LaFrance, 1998; Fiske, 1998).

What is the bottom line here? In one of the earliest analyses of the fallibility of human inference, Francis Bacon (1620/1994) warned that "the human understanding, once it has adopted opinions . . . draws everything else to support and agree with them" and that "an opinion once adopted infects and brings under control all the rest, though the latter may be much firmer and better" (pp. 57–58). Social psychological research has, in large measure, supported these conclusions. To some extent people see what they expect to see, and because such expectations can be wrong or irrelevant, the assimilative tendency they produce can be a source of error in ordinary personology.

Egotism In the early 1950s, perceptual psychology began to acquire a New Look that had as its primary mission the demonstration of constructivist phenomena. Demonstrations of perceptual assimilation were not fundamentally incompatible with the prevailing view of perception, and if they stretched psychologists' understanding, then they surely did not violate it. The New Look's more controversial claim was that perception could also be influenced by warm constructs, such as values, needs, and desires. Philosophers such as Francis Bacon, Niccolò Machiavelli, and Blaise Pascal had long ago described the ways in which passion perverts reason, but they had not claimed that passion perverts vision as well. Yet New Look experiments seemed to suggest just that. People who valued an object perceived it as physically larger than it was, people who feared a stimulus were particularly likely to miss it, and so on (Postman, Bruner, & McGinnies, 1948). Motivation and desire, the New Lookers claimed, did not merely encourage people to think badly or well about what they had seen, but actually determined what people saw in the first place. This was, of course, a natural outgrowth of the constructivist perspective: everyone knew that passion perverts reason, and if (as Kant argued) perception relied on reason, why then, perception might just be perverted as well.

One can understand why such claims would have been unpalatable to most experimental psychologists, for they not only appeared to anoint psychoanalytic notions such as wish fulfillment with scientific respectability, but they also suggested that perception could not be fully understood without reference to the soft and squishy constructs that most players of brass instruments had been trained to ignore. Hoping and wishing and wanting were not the sorts of things that students of optical physiology were eager to include in their theories ("Then, after the signal leaves the optic nerve, it makes a quick stop at the need for approval . . ."), and the New Lookers' attempt to dismantle the wall between cognition and perception was about as welcome as a prison break. Furthermore, some of the New Look's most important phenomena—such as perceptual vigilance and perceptual defense—struck many critics as non sequiturs (Goldiamond, 1958). How could a person know that a stimulus was too threatening to be seen unless she had already seen it? And if she had already seen it, then in what way had she perceptually defended against it? Because the psychology of that era lacked the kinds of modular information-processing models in which "seeing before seeing" is a simple trick (e.g., Fodor, 1983; Zajonc, 1980b), the notions of perceptual vigilance and perceptual defense never attained the wide acceptance that perceptual assimilation and perceptual contrast seemed to enjoy (see Erdelyi, 1974, 1985).

In social psychology, however, the soft and the warm were welcomed with open arms—partly because social psychologists tended to be less concerned with lower-level processes (such as vision and audition) and more concerned with the higher-level processes (such as judgment and belief) in which passion played a noncontroversial role, and partly because passion was an undeniable and undeniably important part of everything that mattered to them. In particular, it was an important part of ordinary personology. What could be more obvious than the fact that people's conclusions about each other are strongly influenced by their hopes, fears, and dreams? As every grandmother knows, love is blind, justice is not, and flattery will get you everywhere. So it surprised few psychologists (and no grandmothers) when social psychology experiments showed that observers sometimes offer more charitable explanations of their own actions than of the identical actions of others, that an observer's need for control may lead her to think of an actor as controllable, that an observer's attributions may be influenced by an actor's physical attractiveness, by similarity to the observer, by expressed liking for the observer, and so on (see Ross & Fletcher, 1985). Students of human behavior had always known that people have a collection of throbbing needs, and it was not particularly difficult to show in the laboratory that personological inferences often satisfy those needs.

It was quite difficult, however, to show that these infer-

ences satisfied the observer's needs as a matter of purpose rather than as a matter of course. As the 1950s gave way to the 1960s, the New Look gave rise to cognitive psychology and cognitive psychology's most significant influence on social psychology lay not in the techniques it offered, but in its definition of a good explanation. Cognitive psychologists *explained* a phenomenon by describing the sequence and operating characteristics of the mental operations that gave rise to it, and not the burning wants it might serve to cool. This preference for explanations that did not invoke preferences had its roots in early American psychology's wholesale rejection of psychoanalytic thinking, but it finally caught up with social psychology and was made an explicit part of its research agenda by Jones and Nisbett (1971), and then later raised to an art form by Nisbett and Ross (1980). These theorists argued that motivations, though quite real and quite important, were facile explanations of erroneous inferences, and that before one invoked them as explanations one ought first to determine whether the particular error might instead be the unmotivated, accidental by-product of a normally functioning system. Most people would agree, for example, that the FAA ought to check the remnants of an airplane's wing flaps for structural defects before rushing to blame the crash on sabotage, and similarly, good explanatory form suggested that social psychologists should look to design before desire when attempting to explain inferential errors.

And so they did. For example, many experiments demonstrated that actors tend to make more situational attributions for their failures ("I didn't get any sleep the night before the test") than for their successes ("I'm awfully bright"), and at first blush this pattern of attributions looked very much like the kind of self-enhancing, egocentric, or self-serving bias that would have made Freud smile and his nana yawn. But social psychologists who adopted the new explanatory style argued that people generally strive to succeed rather than to fail, and people might look to the situation to explain their failures simply because they look to the situation *whenever* they are surprised by an unexpected outcome. A test taker plans to pass a test and naturally explains her expected success in terms of that plan ("I outlined every chapter") and her unexpected failure in terms of the events that thwarted her plan ("The neighbor's dog barked all night long!"). Thus, what looks like excuse making and face saving may be an incidental by-product of a reasonable tendency to attribute unexpected outcomes to unexpected events (see Miller & Ross, 1975). As Jones and Nisbett (1971) noted, "The term 'egocentric attribution' . . . makes the process sound willful and motivated, or at best, the result of self-satisfied laziness. We hold that the individual comes by 'egocentric attribution' honestly for the most part" (p. 87).

This one word—"honestly"—is probably the key to understanding why the change of explanatory style engen-

dered such fierce debate in social psychology (see Tetlock & Levi, 1982). Social psychology's traditional view of inferential error was an essentially psychoanalytic view, which suggested that people are brilliantly self-deceptive and that in order to perpetrate gratifying frauds on themselves they must first outfox their own able intellects. The new cognitive view seemed to imply that people were neither particularly self-deceptive nor particularly brilliant, and that their errors were not the result of plotting strategies so much as plodding strategies. Mistakes were mistakes and everybody made them—so whose fault was it if some of these mistakes happened to stroke the observer's ego? A mistake might satisfy a need, but that certainly did not mean that the mistake had been made *in order to* satisfy the need. So which was it? Wing flaps or sabotage? Were people ingenious deceivers or ingenuous boobies? Different theorists tended toward different positions. Some believed, for example, that people forgive themselves more readily than their neighbors because they *like* themselves more than they like their neighbors, whereas other theorists attributed the same effect to the orientation of people's visual receptors (Storms, 1973; Taylor & Fiske, 1975). Some theorists believed that people selfishly take a disproportionate share of the credit for collaborative ventures because they *like* themselves more than they like their collaborators, whereas other theorists believed that the nature of memory and attention naturally causes people to underestimate their partners' contributions (Ross & Sicoly, 1979). And so on. Some experiments minimized motivating conditions and showed that egocentric effects persisted, others exacerbated motivating conditions and showed that egocentric effects were enhanced, and both sides took these results as evidence for their own positions.

Like so many debates in psychology, the motivation-cognition debate was never resolved in anyone's favor because no one was able to produce the crucial experiment that would instantly bring the other side to its knees. So after a while, the game was called on account of boredom—as well it should have been, because this was less a contest of facts than of definitions. Do people make mistakes about others *because* those mistakes make them feel good or *because* they use flawed judgmental strategies? It depends on what one means by "because." Inferential errors are often mediated by flawed inferential strategies and in this sense are the consequences of those strategies. But one could also argue that people use flawed inferential strategies (rather than the superior strategies that enable social psychologists to point out their flaws) because the result of doing so is appealing, self-enhancing, and ultimately reinforcing. Self-enhancing strategies may conceivably have a selective advantage at the species level (Taylor & Brown, 1988; see also Buss & Kenrick, 1998, in this *Handbook*), and thus even if a particular individual cannot avoid using them, their self-enhancing effects still provide an ex-

planation for their existence. Social psychology's brief attempt to exclude motivation from its explanations of inferential error was surely as ill-fated a project as Skinner's attempt to exclude psyche from psychology, because motivational and mechanistic explanations are not at odds, but rather serve as complementary levels of explanation.

Bacon (1620/1994) noted, "The human understanding is not a dry light, but is infused by desire and emotion which give rise to wishful science. For man prefers to believe what he wants to be true . . . In short, emotion in numerous, often imperceptible ways pervades and infects the understanding" (pp. 59–60). Social psychologists have once again come back to Bacon. People may actively or accidentally achieve gratifying conclusions; they may be blinded by lint or lust; they may be inexcusably dishonest, forgivably forgetful, neither, or both. To be sure, even if it is hard to tell when, needs, wants, wishes, and desires can be a potent source of error in personological judgment.

Realism Descartes drove a wedge between the physical presentation and the mental re-presentation of objects—between things and one's view of things—and ever since, philosophers have worried about how and how well thoughts represent the world. But if that distinction has proved a source of anxiety for philosophers, it has not caused widespread panic among the masses. While Descartes wondered whether he could truly know anything, his butcher chopped chickens and his banker counted coins. While Kant argued that he could truly know nothing, his barber gave haircuts and his baker made muffins. Although psychologists have tended to accept one or another form of idealism, ordinary people behave as though they accept some form of realism.⁵ In caricature, realism suggests that the mind is a blank tablet, that the eye is merely a socket through which experience pokes its pencil, and that the representations that exist in the brain are therefore the authentic signatures of objects as they exist in the world. As the original realist, John Locke (1690/1959), argued:

When our senses do actually convey into our understandings any idea, we cannot but be satisfied that there doth something *at that time* really exist without us, which doth affect our senses, and by them give notice of itself to our apprehensive faculties, and actually produce that idea which we then perceive. (p. 333)

It is in the nature of perception, the realist argues, that the world should appear as it really is, and hence there is not much point in fussing over the distinction between appearances and realities. The Scottish philosopher Thomas Reid (1795/1983) noted that even those philosophers who insist that appearance does not reflect reality nonetheless behave in their everyday lives as though it does: "I never heard that

any skeptic run his head against a post, or stepped into a kennel, because he did not believe his eyes" (p. 158).

Research suggests that ordinary people do believe their eyes. That is, they tend to be realists inasmuch as they may fail to acknowledge, consider, or otherwise take into account their mind's role in the construction of their subjective experience. For example, Gilbert and Osborne (1989) led observers to believe mistakenly that an actor was shy, and then allowed some of the observers to hear a brief, uninformative interview with the actor. Next, observers were disabused of the original mistaken notion. Those who had not heard the uninformative interview readily relinquished their mistaken beliefs about the actor, but those who had heard the interview did not. These observers defended their continued belief in the actor's shyness by explaining that although they now recognized that the original information had been erroneous, the actor's shy personality had clearly "come through" in the subsequent interview and that this new information constituted a reasonable basis for their continued belief. In other words, observers did not recognize that their original mistaken belief about the actor had served as an expectancy that assimilated the information later presented in the interview. Rather, they argued that the impressions they formed on the basis of the interview were realistic. Because these observers failed to recognize that their own expectancies had played a role in constructing their perceptions, they drew inaccurate inferences about an actor.

Realism, then, is simply the failure to acknowledge that idealism and egotism can be sources of error. It is the failure to recognize the inherent mutability—and hence the fallibility—of one's own perceptions and judgments. As Ayer (1956) noted, the realist "denies, or overlooks, the existence of the gap between what things seem to be . . . and what they really are. His mistake, if it is one, is therefore just that he oversimplifies the situation; he denies the possibility of questions which can in fact be asked" (p. 113). This tendency seems to underwrite a host of phenomena that have been documented by social psychologists (see Dunning et al., 1990; Griffin, Dunning, & Ross, 1990; Griffin & Ross, 1991; Jacoby, Lindsay, & Toth, 1992). But if the failure to acknowledge that one's beliefs and desires can color one's perceptions of the world is not uncommon, neither is it ubiquitous. Folk wisdom is replete with aphorisms that remind us that different people often have different views of the same thing, and even the smuggest observer is willing to admit that the moon merely *looks* bigger on the horizon than it does at its zenith. People may underestimate the *extent* to which their own minds shape and mold their experiences, but few would insist that everything they see is always and precisely as they see it. To start and stop by saying that people are realists, then, is to miss a more subtle and interesting fact: people may tend toward realism, but they may also be pushed, prodded, and bullied in the direction of acknowledging idealism and egotism.

Consider how a shift from realism to the acknowledgment of idealism and egotism characterizes Piaget's view of cognitive development. Young children tend to act as though their perceptions of objects are determined entirely by the object's properties, which is why they expect others to perceive objects the same way as they do. But with maturity, children seem to transcend this form of realism (which Piaget called egocentrism), and almost all of the developmental changes outlined in Piaget's theories "either are or could be interpreted in terms of a gradual replacement of egocentric thought by socialized thought" (Flavell, 1963, p. 270). What initiates this replacement? "It is social interaction which gives the ultimate *coup de grâce* to childish egocentrism" (Flavell, 1963, p. 157) because the socialized child "increasingly finds himself forced to reexamine his own percepts and concepts in the light of those of others, and by so doing, gradually rids himself of cognitive egocentrism" (Flavell, 1963, p. 279). The attributionist would say that increased exposure to consensus information enables the realist child to begin attributing her perceptions to her own dispositions rather than attributing those perceptions to the object. "Before the analysis of variance cube begins to fill in, however, the child believes that clowns are funny in the same way that balls are round" (Jones & Nisbett, 1971, p. 86).

If realism goes away as children develop, where does it go? Developmentalists have generally assumed that realist thinking is transcended or eliminated in the course of cognitive development, but the sequential operations models described earlier suggest that people may not "replace" or "rid themselves of" realism, but rather may learn to correct their realist conclusions *after* those conclusions have been generated. Gilbert and Gill (1996) have shown that people may take their own reactions to a stimulus ("Ha ha ha") as an accurate characterization of the objective properties of that stimulus ("The clown is funny"), and only subsequently recognize that this reaction is idiosyncratic ("But I always laugh at big people in tiny cars, so maybe no else would find it funny"). The world is taken to be isomorphic with one's experience of the world, and then, in a second mental act, the isomorphism is questioned, interrogated, and sometimes repudiated (see Gilbert, 1991). Adults who appear to have moved beyond realism may merely be adept at subverting it. As Jones and Nisbett (1971) note, "It seems clear that the distinction between evaluations and primary qualities is never fully made. We never quite get over our initial belief that funniness is a property of the clown and beauty is in the object" (p. 86). In short, thoughts may misrepresent things, and when people fail to acknowledge this fact they may give themselves over to error.

Circumstantialism The truth and the whole truth are rarely the same. Even when people see the world as it is—

that is, even when they manage to avoid the untoward effects of idealism, egotism, and realism—they may fail to recognize that what they see is not necessarily everything that is seeable. Scientists routinely draw inferences about large populations from observations of small samples, and this practice is legitimated by strict adherence to sampling rules, one of which is that a sample must be sought (i.e., randomly drawn or specifically selected) rather than stumbled upon. Just as everyone knows that a healthy diet is composed of foods chosen for their nutritional value and not those that happen to be in the refrigerator, everyone knows that healthy inferences about others should be based on a diet of information that logic or experience suggests will be useful rather than on information that simply happens to be at hand.

Yet one theme that runs through much of the work on errors in ordinary personology is that personological judgments tend to emphasize information that just happens to present itself to the observer, and thus, these judgments are subject to the vagaries of circumstance. Consider three examples. First, an observer's seating position may influence his inferences such that the actor who dominated his visual field may be thought to have dominated the social interaction, presumably because that actor's behavior is particularly noticeable and hence particularly well represented in memory (see Taylor & Fiske, 1978). Second, when an observer is in a bad mood ("I've been waiting for an hour and I'm getting really ticked off"), she may draw inferences about an actor ("This guy is a real jerk") on the basis of information that comes naturally or easily to mind ("Now that I think of it, he was late the day we went sailing too") rather than on the basis of information that is also in memory but that, because of her foul mood, resists retrieval ("Of course, he also saved me from drowning"; see Forgas, 1995). Third, an observer may conclude that an actor is especially extroverted, conservative, likable, or depressed when the actor is placed in circumstances that are specifically designed to evoke extroverted, conservative, likable, or depressed responses from an average person (see Gilbert & Malone, 1995).

What do these superficially disparate demonstrations have in common? In each of these instances an observer uses the information that most vigorously presents itself and fails to use information that is (1) present but pallid (the salience bias), (2) stored in memory but not active in mind (the accessibility bias), or (3) absent but obtainable (the availability bias). In each of these instances the observer draws inferences about an actor based on information that happens to find its way into her awareness, and fails to consider that from another vantage point, in a different mood, or under other circumstances, she might have noticed, remembered, or encountered very different information. People go beyond the information given inasmuch

as they willingly draw inferences from that which they see, but they *fail* to go beyond the information given inasmuch as they often do not search their environments or memories for that which is not readily apparent.

Scores of studies in social psychology capitalize on some aspect of people's tendency to be overly influenced by information that circumstances happen to afford or highlight. Salience biases, availability biases, accessibility biases, base-rate fallacies, representativeness biases, correspondence biases, vividness effects, conjunction fallacies—the list goes on (see Gilovich, 1991). The picture that emerges from these studies is that of a passive judge who renders verdicts about others on the basis of the information received by the court, rather than of an active detective who makes inferences on the basis of information he first deems informative and then sets out to gather. When given the opportunity to play detective and examine whatever information they wish, people often do seek the most appropriate kind (Trope & Bassock, 1982), thereby revealing that they recognize, in principle, what sort of information constitutes an appropriate basis for judgment. However, principle and practice are not the same, and in everyday life people rarely have the opportunity to see or select only the evidence they wish to examine. Students parade their talents and hide their mistakes, colleagues trumpet their agreement and mumble their objections, and situations bring out the best or the worst in friends, but rarely both at once. The information that life serves is not necessarily the information that one would order from the menu, but like polite dinner guests and other victims of circumstance, people generally seem to accept what is offered rather than banging their flatware and demanding carrots. Bacon (1620/1994) considered this the most troubling of all inferential errors:

By far the greatest impediment and aberration of the human understanding arises from [the fact that] . . . those things which strike the sense outweigh things which, although they may be more important, do not strike it directly. Hence, contemplation usually ceases with seeing, so much so that little or no attention is paid to things invisible. (p. 60)

Bacon illustrated this point with a story (which he "borrowed" from Cicero, who told it seventeen centuries earlier). A visitor to a Roman temple was shown a portrait of several pious men who had miraculously escaped a shipwreck and, when pressed to take this miracle as evidence for the power of the gods, astutely inquired, "But where are the pictures of those who perished after taking their vows?" (Bacon, 1620/1994, p. 57). Social psychology experiments suggest that many of people's mistaken conclusions about others are due to the fact that they

rarely ask to see the missing sailors. The observer's tendency to rely exclusively or heavily on the information that she happens to find—or that happens to find her—is one of the fundamental sources of error in ordinary personology.

Molecular and Atomic Errors The list of errors in ordinary personology is long, and those who did not see their most favored or most feared in the foregoing discussion should not be disappointed just yet. Although there may well be errors that befuddle this fourfold analysis, many of the missing can probably be constructed from a combination of the four general tendencies just described. For example, many psychologists (e.g., Fiske & Taylor, 1991; Greenwald, 1980; Kruglanski & Webster, in press) have suggested that resistance to new information is a potent source of inferential error. Once we decide that someone is cunning, curious, capable, or cruel, even compelling new evidence to the contrary may be unable to dislodge that initial belief. Why? Each of the four phenomena—idealism, egotism, realism, and circumstantialism—may play a role. For example, an observer may be motivated to retain those beliefs in which she is personally invested, not only because changing her mind is hard work, but also because it is an admission of failure that may leave her feeling stupid, regretful, powerless, or uncertain (egotism). In addition, because the observer surrounds herself with likable and like-minded others, the new evidence that happens to present itself may be evidence that tends to confirm her current beliefs, and she may not conduct a full-scale search for the disconfirming evidence that failed to find her (circumstantialism). When disconfirming evidence does find the observer, her preexisting beliefs may act as expectations that lead her to see the new evidence as less of a challenge than it actually is (idealism). Finally, the observer may not be open to the possibility that she has made a mistake, because she may believe that her conclusions were caused by behaviors “out there” in the actor rather than by expectations and motivations “in here” (realism). Most researchers agree that complex errors are composed of more basic errors (e.g., Higgins, in press-a; Vorauer & Ross, 1993), and it remains to be seen whether many, most, or all of the mistakes people make about people can be construed in terms of these particular building blocks.

Of course, social psychologists have rarely begun with atomic elements and then worked their way toward intriguing and impactful molecular phenomena. Quite the opposite. The study of error in ordinary personology did not begin with the delineation of four, five, or seventy-two elemental tendencies; rather, it began with a shrewd observation by a sad, prophetic man who described one of social psychology's most important phenomena about twenty years before anyone thought to investigate it. With a gen-

eral framework for understanding errors in hand, we are now prepared to hear what he had to say.

THE FUNDAMENTAL ERROR OF ORDINARY PERSONOLOGY

Gustav Ichheiser was a Polish refugee who came to the United States in 1940 as a displaced person, repeatedly failed to attain a suitable academic position, and died thirty years later alone, in poverty, and under conditions that hinted at suicide (Boski & Rudmin, 1983; Rudmin et al., 1987). He considered himself a victim of difficult circumstances—from the war that made him an expatriate in his youth to the mental institution that made him a prisoner in his later years—but moreover, he considered himself a victim of those who judged him by his professional failure and neglected to take the circumstances of that failure into account. Ichheiser's circumstances were difficult indeed, and they led him to develop a poignant and penetrating analysis of his own suffering. We do not know whether for Ichheiser, as for Van Gogh and Nietzsche and Rimbaud, the tortured life was an essential ingredient for the great work he produced. We do know that he produced great work. When one reads Ichheiser's writings—many of which predate the earliest developments of attribution theory by nearly two decades—one has the sense of reading a thoroughly modern analysis of errors in ordinary personology. Ichheiser should have been one of social psychology's leading men, but his acerbic personality and calamitous history cast him instead in the role of fifth business. As a result, social psychology waited several decades to discover by experiment what Ichheiser had gleaned from experience.

Ichheiser's Core Ideas

One Error Is Fundamental Fifteen years before Heider described the extraction of dispositional invariance as the attributional system's primary task, Ichheiser argued in much plainer language that the ordinary actor has dispositions that may or may not manifest as behavior (the process of *expression*), and that the ordinary observer's attempts to infer those dispositions from those behaviors (the process of *impression*) generally meet with mixed success. Whenever people attempt to infer realities from appearances they are bound to make mistakes, and Ichheiser (1943) suspected that many of those mistakes were instances of a single, fundamental error:

Instead of saying, for instance, the individual X acted (or did not act) in a certain way because he was (or was not) in a certain situation, we are prone to believe that he behaved (or did not behave) in a certain way because

he possessed (or did not possess) certain specific personal qualities. (p. 152)

Ichheiser, like Lewin, conceived of behavior as a joint product of dispositions and situations. But he was struck by the fact that when making attributions, both professional psychologists and ordinary people displayed a "tendency to interpret and evaluate the behavior of other people in terms of specific personality characteristics rather than in terms of the specific social situations in which those people are placed." (Ichheiser, 1949, p. 47). People attribute failure to laziness and stupidity, success to persistence and cunning, and generally neglect the fact that these outcomes are often engineered by tricks of fortune and accidents of fate. "The persisting pattern which permeates everyday life of interpreting individual behavior in light of personal factors (traits) rather than in the light of situational factors must be considered one of the fundamental sources of misunderstanding personality in our time" (Ichheiser, 1943, p. 152). In attributional language, Ichheiser suggested that people often fail to use the discounting principle, and thus they draw dispositional inferences from behaviors that are situationally produced. Why would people make such a mistake? Ichheiser offered a two-part answer.

The Fundamental Error Originates in Ideology Ichheiser argued that the average person has "misinterpretative mechanisms . . . at work within himself, distorting and falsifying his perception of other people" (Ichheiser, 1949, p. 6). This description may sound vaguely psychoanalytic, but Ichheiser was staunchly anti-Freudian, and he wrote of *misinterpretative* mechanisms rather than *defense* mechanisms because he believed that the fundamental error was not a rationalizing maneuver designed to pamper a frail ego, but a stubborn cultural myth:

These misinterpretations are not personal errors committed by ignorant individuals. They are, rather, a consistent and inevitable consequence of the social system and of the ideology of the nineteenth century, which led us to believe that our fate in social space depended exclusively, or at least predominantly, on our individual qualities—that we, as individuals, and not the prevailing social conditions, shape our lives. (Ichheiser, 1943, p. 152)

The fundamental error was like the belief in a flat Earth, and if there was blame to be laid for such ignorance, it was properly laid on the times and not on the misguided folk who stumbled through them. Ichheiser (1943) argued that "the working of the whole social order in our society depends upon such misinterpretations . . . (which) perform the same function of maintaining the appearance that the 'individual gets what he deserves' " (p. 154, cf. Lerner, 1980). In a society that rewarded some with wealth and

others with hardship, the tendency to attribute people's outcomes to dispositions served to justify the status quo. Classist society could only perpetuate itself by brainwashing its members to think of people as the authors of their actions, and thereby deserving of their fates (see Nisbett, 1987; Weber, 1930).

The Fundamental Error Is Maintained by Invisibility Society, then, planted the seed of the fundamental error by teaching the individual that the situational antecedents of others' behavior need not be considered. But even if the individual thought to consider them on her own, Ichheiser believed that by their very nature, situational antecedents would be difficult to detect. For example, observers rarely have the opportunity to see how actors behave when the observer is not present, and because observers lack information about how the actor's behavior covaries with the observer's presence, observers may fail to realize that they are themselves the cause of the actor's behavior.

Many disagreements about interpreting and evaluating other people could be resolved if we would realize that those disagreements are simply the result of our not being aware that we ourselves are "switching on" different aspects of other people's personalities by the mere fact of our presence. (Ichheiser, 1949, p. 29)

Furthermore, many of the other factors that we think of as situational causes of behavior (e.g., bribes, threats, peer pressure) are not physically manifested during the behavioral episodes they control, and thus they may escape even the most mindful observer's attention. One sees the battered wife rushing to heed her husband's commands, but one does not actually *see* the history of abuse that causes her fearful acquiescence.

In perceiving and observing other people we do see the spatial situation in which they act, but, as a rule, we are not in the position to see and to evaluate correctly the dynamic meaning of the social, invisible factors in the total situation controlling the behavior of those people. . . . The complete tragic blindness of the privileged concerning the life-situation of the underprivileged is the result of just this kind of not seeing the invisible factors in the situations of others. (Ichheiser, 1949, p. 47)

Ichheiser returned again and again to the problem of invisibility, often speaking of situational forces such as poverty, racism, and unemployment as "invisible social chains" or "invisible jails" or even "invisible concentration camps" that observers could barely help but overlook (Ichheiser, 1949, p. 47). There is no doubt that he considered the invisibility of situational forces to be one of the great impediments to an accurate ordinary personology—and to an accurate scientific personology as well.

According to Ichheiser, then, people often fail to use the discounting principle because they are indoctrinated with false beliefs and because the evidence that would refute such beliefs is hard to come by. In Ichheiser's view, the individual was an innocent, and the brute facts of social life and physical reality conspired to prevent her from seeking or noticing information about the situational factors that shape the behaviors of others. Not only was Ichheiser's a remarkably forgiving analysis of those whom he believed had misjudged, exiled, and institutionalized him, but it also presaged the highly cognitive, nonmotivational spin that social psychologists would put on their own analysis of errors in ordinary personology several decades later. People, it seemed to Ichheiser, came by the fundamental error honestly.

The Fundamental Error Occurs Automatically Ichheiser believed that thoughtful people generally acknowledge the validity of the discounting principle—in principle—but that they often cannot apply this principle because their dispositional inferences are “automatized interpretations” and thus “even though we often know ‘in principle’ the conditions under which success is obtained . . . our explicit critical knowledge remains pale and ineffective” (Ichheiser, 1943, p. 154). In other words, the realization that dispositional inferences are logically unwarranted does not prevent them from being drawn. Ichheiser suggested a mechanism by which these “automatized interpretations” and one’s “explicit critical knowledge” might interact. “Very frequently the conscious interpretations operate on the basis of an image of personality which was already performed by the unconscious mechanisms” (Ichheiser, 1949, p. 19). Ichheiser shared Heider’s sense that attribution was largely implicit, but he went further by suggesting that implicit attributions were dispositional attributions, that they occurred in spite of one’s better insights into the matter, and that if one’s better insights came into play at all, then they did so by operating on the implicit dispositional inferences that were previously and automatically produced. The fact that the attributional process was a combination of automatic and controlled operations was one of the reasons why the fundamental error so often escaped extermination. In short, Ichheiser outlined the concepts and models that would, forty years later, be used to explain the fundamental error.

The Observer Bias The study of ordinary personology developed in the respectable academic settings of which Heider and Asch were, and Ichheiser was not, a part. Thus, it took an accident for Heider’s children and Asch’s children to rediscover the phenomenon that Ichheiser—who was the first and last of his line—had spent a lifetime experiencing and describing.

Jones and Davis (1965) suggested that when behavior is evoked by situational norms, the actor’s intentions are am-

biguous, and thus her behavior should not be attributed to her extraordinary dispositions. Kelley (1976) construed this notion more broadly as the discounting principle. In an attempt to demonstrate that people follow this attributional prescription, Jones and Harris (1967) showed subjects an essay that either supported or opposed Castro’s regime, and told subjects either that the essay had been freely rendered by a college student or that it had been solicited from a college student by an authority figure. Subjects were then invited to draw inferences about the essayist. Not surprisingly, subjects concluded that essayists privately endorsed the position they had volunteered to defend—particularly when the essayist’s position was unusual for an American college student. What was surprising was that subjects also concluded that (to a much smaller degree) the essayists privately endorsed their positions when they had been coerced to defend them. Although the attributional rules clearly stated that coercion (a strong situational force) renders correspondent inferences illegitimate, subjects seemed willing to defy the canons of logic and draw such bastardly conclusions.

What could this unexpected result have meant? It could have meant any number of dull things. For example, it could have meant that subjects had misunderstood the instructions (“I didn’t realize he was coerced”), that the essays had been so powerful that subjects had themselves been persuaded (“Now I see why Castro is such a great leader. I suppose the essayist must have undergone the same eye-opening experience”), or that the essays contained specialized arguments that no one but a true advocate would advance (“If he isn’t a Red, then how come he knows so damned much about Havana cigars?”). Or it could have meant something much less dull. Subjects’ judgments about the coerced essayist might—just might—have been an instance of a general tendency to attribute behavior to extraordinary dispositions (“He’s a commie”) even when the rules of attribution theory suggest that they should have attributed the behavior to ordinary dispositions (“He likes to please authority figures”), or, in Kelly’s language, to situations (“The debate coach forced him to defend Castro”). In other words that should have a familiar ring, people may display a fundamental tendency to evaluate the behavior of other people in terms of specific personality characteristics rather than in terms of the specific social situations in which those people are placed.

It is important to note what this finding did *not* suggest. It did not suggest that observers do not use attributional rules. Jones and Harris’s subjects clearly did follow the prescriptions of the discounting principle inasmuch as their inferences about the dispositions of coerced essayists were much less correspondent than were their inferences about the dispositions of volunteers. The volunteers were seen as *absolutely* and *fervently* pro- or anti-Castro, the coerced essayists were seen as *probably* a *bit* pro- or anti-Castro, and

the differences in the strength and confidence of these inferences suggested that subjects did follow the discounting rule, albeit imperfectly. A computer with a damaged chip may use standard arithmetical rules and yet consistently arrive at solutions that are "off" by a constant; for example, it might conclude that $46 - 10 = 37$, that $7 \times 7 = 50$, that $2 + 2 = 5$, and that $9 \div 3 = 4$. It would be odd to say that such a computer did not use the rules of arithmetic. After all, 37, 50, 5, and 4 are not random numbers. They are very close to the right answers, and can be thought of as the right answers coupled with an annoying tendency for a computer with a dented chip to add 1 to the result of its calculations. Similarly, observers may follow attributional rules when they make inferences, but these inferences may reflect a certain leaning, tilt, or bias, and such a bias can be thought of as occurring *in addition to* and not *instead of* the prescribed rule-following behavior. Jones and Harris interpreted their result as evidence of such a leaning, which they called the observer bias.

Over the next fifteen years, Jones and his colleagues executed a program of research whose primary goal was to eliminate the dull explanations of this bias. That program was a success inasmuch as the dull explanations were, in the end, found wanting. For example, the phenomenon did not require the use of strong essays with unusual arguments, it did not require that subjects change their own attitudes, it did not depend on subjects' misunderstanding the instructions (see Jones, 1979, 1990). Alas, no sooner was one dull explanation put to the sword than two more arose to take its place, and within a short time the Jones and Harris experiment had become a veritable clown in a dunking booth and attempts to unseat it had become one of social psychology's favorite pastimes. These attempts usually took the form of attributing the result to some local feature of the experimental setting that presumably did not generalize to the everyday circumstances in which psychologists were properly interested. Even the most eminent psychologists paid a quarter and took a turn: "It may well be that existing evidence simply indicates that the context factors have not been manipulated as strongly or as clearly as has the behavior itself" (Kelley, 1971a, p. 18). Jones and his colleagues dodged such attacks one by one, showing that this or that feature of the experimental manipulation was not necessary for the appearance of the effect. But the attacks turned the research program into a game of ruling out rather than ruling in, and ultimately it said more about what did not cause the bias than what did.

The Fundamental Attribution Error Most social psychologists who worried about such things worried about whether the observer bias was a fact or a fiction. Ross (1977) argued that the bias was not only an established fact, but that it was *the* fact that established many of social psychology's most celebrated phenomena. For example, Festinger and Carlsmith (1959) had shown that when peo-

ple are induced to perform counterattitudinal behavior, they often rationalize that behavior by reporting attitudes consistent with it, and Bem (1967) had suggested that such reports were the products not of rationalization but of attribution. The debate between dissonance and self-perception theorists became social psychology's version of championship kick-boxing (with all the usual hooting and blood loss), and the counterattitudinal advocacy effect became the field's most intensely studied phenomenon. Ross agreed with Bem (and with Kelley) that people use attributional rules to draw inferences from their own behavior. But, he pointed out, the counterattitudinal advocacy effect would not occur if people followed these attributional rules meticulously. In fact, such effects could occur *only* if observers tended to overlook the situational cause of behavior—their own and everyone else's—and tended instead to ascribe behavior to extraordinary dispositions. The observer bias, he argued, was not a matter for the fine print. It was the engine that drove social psychology's most important empirical phenomenon.

Ross's thesis met with unbridled enthusiasm. In the wake of attacks that had shaken social psychology's self-confidence (Gergen, 1973; cf. Schlenker, 1974), researchers were particularly eager to find core axioms on which to hang both their science and their self-esteem, and Ross offered one that did both. He argued that social psychology had uncovered a first principle in its brief history—namely, that people tend to underestimate the extent to which situational forces guide behavior—and that a sizable chunk of the social psychological literature could be reconsidered in that light. For example, the classic experiments on which the discipline had been founded (such as those of Festinger, Milgram, Schachter, and Asch) had usually involved manipulating subtle situational variables and demonstrating that such manipulations had dramatic behavioral consequences. A polite request, another person's opinion, even the mere presence of others could turn ordinary participants into obedient executioners, gutless conformists, or docile bystanders. To a generation that could still hear the faint echo of goose-stepping Nazis, these demonstrations had been of tremendous interest—precisely because they violated the intuition that dispositions, and not situations, are the authors of action. People think of conformity and obedience and selfishness as traits that are woven into the fabric of personality, and these classic experiments surprised and engaged their audiences precisely because they obviated that belief. Without the fundamental attribution error, social psychology's most beloved phenomena would not have occurred and its most beloved experiments would have been mere platitudes. "Indeed, if people were more routinely aware of the situational influences that affect their behavior, social psychology would be a less important, or at least a very different, discipline" (Jones, 1993, p. 87).

But there was more. The fundamental attribution error

did not just explain why the results of classic experiments were interesting; it also explained why social psychology's experimental method was crucial—even liberating. When a social psychologist watched the Milgram film, she was in much the same position as Jones and Harris's observers had been. Both witnessed behavior that conformed to the requirements of the situation, and both were surprised by what they saw. The social psychologist, armed with information about control groups, random assignment, and the nature of the independent variable, could shake her head in awe and conclude (as Milgram wanted her to) that authority figures must indeed be more powerful forces than she had suspected. The observer, armed with none of this information, could only assume that authority figures were about as powerful as he had always thought they were and that the teacher's behavior must therefore have been propelled by a dispositional push. Social psychologists and ordinary people shared fundamentally flawed intuitions about what made actors act, but through the lens of social psychology those flaws were revealed, healed, and parlayed into wisdom. Whereas ordinary people underestimated situational forces, made erroneous dispositional inferences, and then went on to persecute Ichheiser, social psychologists learned from their experiments to embrace an invisible truth that ordinary people seemed destined to miss. The fundamental attribution error not only underwrote much of what social psychologists had discovered about human nature; it justified the science itself.

The Correspondence Bias Jones and Harris's critics asked whether the observer bias was real, and Ross answered those critics by arguing that if one did not believe in the fundamental attribution error, then one could barely believe in social psychology. But if the tendency for people to make unwarranted dispositional inferences was significant in all senses of that word, in the 1970s its causes were still poorly understood. Twenty years later, that error is recognized as a molecular rather than an atomic error, and several mechanisms are now known to cause it. Not so coincidentally, the observer bias, fundamental attribution error, or, as it later came to be called, correspondence bias, can be considered in terms of the four general sources of error in ordinary personology.

Egotism Ichheiser argued that dispositional inferences justified inherently unjust social systems by perpetrating the illusion that people's outcomes are ultimately of their own making. Heider argued that this illusion not only insulated observers against harsh political realities, but also gave them the sense that they could predict the behavior of others and hence control their own fortunes. As Heider (1958) noted, people make dispositional inferences "not because of idle curiosity, but because only if I refer this relatively insignificant offshoot event to an underlying core event will I attain a stable environment and have the possi-

bility of controlling it" (p. 80). A dispositional inference is a valuable and desirable commodity inasmuch as it "permits man to give meaning to action, to influence the actions of others as well as of himself, and to predict future actions" (Heider, 1958, p. 123). Can people be unbiased analysts in search of causal truths when they are so handsomely rewarded for embracing one truth in particular? Empirical research suggests that when an observer's need to predict an actor's behavior is piqued, her tendency to draw dispositional inferences is similarly exacerbated. For example, when observers of a bargaining game expect to play the game later with one of the actors, they are especially likely to attribute that actor's moves to the actor's dispositions, presumably because believing that a future opponent is sneaky, bold, or careless gives one the comforting sense that the opponent's behavior can be predicted, and hence its aversive consequences avoided (Miller, Norman, & Wright, 1978). According to this view, then, people display the correspondence bias because they *want to* (see Webster, 1993).

That tune sounds like an easy whistle, but there are a few high notes. At the core of the argument is the claim that dispositional inferences have a greater intrinsic payoff than do situational inferences. But consider some examples. If Walter seems particularly engaged during today's lecture, then an observer who makes a dispositional inference about Walter ("He's bright and eager") has a good sense of what Walter will do in a new situation tomorrow ("He'll blow the top off the GRE"). That's prediction and control. If, on the other hand, Walter's behavior is attributed to the situation ("Professor Mezmer is a fascinating speaker"), then the observer may be able to predict how Walter will behave in the same situation tomorrow ("I'm sure he'll be attentive at Mezmer's next lecture"), but not in other situations ("But who knows how he'll do on the GRE?"). Does this mean, then, that the latter inference leaves the observer in a weaker position than the former? Not necessarily. The observer who draws a dispositional inference may indeed be able to predict the behavior of a small number of actors (namely one: Walter) in a large number of situations, but the observer who makes a situational inference can predict the behavior of a large number of actors in a small number of situations (namely one: Mezmer's lecture). In other words, attributions are powerful to the extent that they enable observers to understand the element with which they expect to have the most frequent and enduring commerce. The dentist who attributes her patient's nervousness to the experience of the visit gains a powerful tool for predicting the behavior of all patients, just as the dentist who cannot understand why her practice seems to attract such "exceptionally nervous people" seems to miss the point entirely.

Thus, dispositional inferences are not inherently more powerful than situational inferences, and under different circumstances ordinary people can use both the situationist perspective of social psychology and the dispositionist per-

spective of personality psychology to bring order to their interactions. This fact may qualify, but it does not obviate, Heider's general point. On some occasions people will surely find dispositional inferences to be more informative, useful, and comforting than situational inferences, and on such occasions, egotism provides a ready explanation for the correspondence bias.

Circumstantialism Heider did not consider egotism the only cause, or even the most important cause, of the correspondence bias. Rather, he argued that "behavior in particular has such salient properties it tends to engulf the total field" (Heider, 1958, p. 54). Although Jones and Harris (1967) complained that "this describes the results without really explaining them" (p. 22), Heider's abstruse remark does provide an explanation, assuming that one takes it to mean that situational forces are often invisible, and hence are often unknown to observers, and hence are often not entered into the attributional calculus by which observers draw conclusions about actors. This exegesis of Heider's claim (which was also one of Ichheiser's core arguments) describes a paradigm case of circumstantialism. Circumstantialism is the tendency to consider "that which happens to be here" rather than "that which happens to be," and the problem of situational invisibility occurs because the *antecedents* of action are often removed from the circumstances of behavior. A gun to the head may be a useful metaphor for coercion, but metaphorical guns are not nearly as clear or present a danger as a .357 magnum. Situational constraints are generally not objects that can be tasted, touched, and seen, but events that begin and end—and these events tend to be over before the behaviors they invoke have even started. If situational constraints generally do not present themselves to the observer, and if observers tend not to look beyond that which presents itself, then this tendency toward circumstantialism will predictably give rise to the correspondence bias. Ross, Amabile, and Steinmetz's (1977) well-known study is a case in point. Observers who judged a game player to be especially capable failed to consider how the player's role had given him or her an unfair advantage. Although powerful, a role-conferred advantage is not a visible part of the behavioral episode, but rather an abstraction that one must achieve through analysis and reflection. It *is*, but it is not *here*, and as such, it is not likely to be considered by the circumstantialist.

Circumstantialism refers to the failure to use information that is absent but obtainable, but it also refers to the failure to use information that the observer has, but for some reason, does not have in mind. Gilbert, Pelham, and Krull's (1988) sequential operations model suggests that when observers are interested in diagnosing an actor's dispositions, they draw dispositional inferences with relative ease and then correct those inferences with relative effort. In other words, to correct their dispositional inferences and

avoid the correspondence bias, observers must actively retrieve and consider information that they possess but that is not currently active in their awareness. Gilbert, Pelham, and Krull (1988) have shown that observers are particularly unlikely to correct their dispositional inferences when they are laboring under cognitive load, and this load-induced undercorrection effect is one instance of a more general failure to use the information that one possesses and that, in more reflective moments, one acknowledges as necessary for accurate judgment (Gilbert & Osborne, 1989). The lesson to be drawn from this research is that it is more difficult to overcome circumstantialism than to surrender to it, and that when people are unable or unwilling to do what is difficult, then they may be especially prone toward the correspondence bias.

Idealism Heider (1958) noted that "the ambiguity of behavior as a local stimulus is reduced when it is seen in a situational context" (p. 38) because "what the other person actually did is not only perceived on the basis of local cues given by the movements of the person, but also on the basis of what we think we know about the situation" (p. 116). In other words, funerals prepare us to see frowns, birthday parties prepare us to see grins, and within limits, people tend to see what they are prepared to see. Kelley (1971a) was the first to recognize that the assimilation of an actor's behavior to the observer's situation-based expectancy could cause observers to draw unwarranted dispositional inferences about the actor. For example, in a well-known study by Jones, Davis, and Gergen (1961), observers saw a job applicant behave in an outgoing manner and then rated the actor's dispositional extroversion. Some observers believed that the job required extroversion (in which case both situational and dispositional factors could have been plausible causes of the actor's behavior); others believed that the job required introversion (in which case only dispositional factors could have been plausible causes of the actor's behavior). The extent to which the first group's ratings departed from the second group's ratings was taken as a measure of the first group's use of the discounting principle. If the two groups had made identical ratings, then the experimenters might have said that the first group "ignored the situational constraints on the actor" or "failed to discount" or "displayed the correspondence bias." But as Kelley (1971a) noted:

To be tested in a between-subjects design, the discounting principle requires that a subject aware of two or more plausible causes see the same effect as the subject who is cognizant of only one plausible cause. In other words, the perceived magnitude of the effect must not be affected by the information regarding the cause(s) or the application of the principle will be confounded. (p. 11)

One may conclude that the first group of observers displayed correspondence bias *only* if both groups of ob-

servers saw precisely the same behavior. And although both groups of observers were *shown* the same episode of behavior by the experimenter, "this condition would have been violated had there been . . . perceptual assimilation of the behavior to role requirements" (Kelley, 1971a, p. 11). In other words, the fact that observers are *shown* the same behavior does not mean that observers *see* the same behavior, and there is every reason to suspect that observers who know about situational demands will see an actor's behavior as more congruent with those demands than will observers who are unaware of the demands.

Trope's (1986) model shows how the assimilation of an actor's behavior to an observer's situation-based expectancy can cause the correspondence bias. As Figure 6 shows, situational information has opposite effects at the identification and inference stages, and thus all the inferential benefits of using the discounting principle (effect δ) may be offset by the assimilation of the behavior (effect χ). For example, an observer may, like a law-abiding attributer, take into account the fact that Opal is waiting for the dentist ("Waiting to have a tooth pulled would make anyone feel a bit anxious"), but if the observer's knowledge of the situation has already caused him to see more anxiety in Opal's furrowed brows than is actually there ("She looks positively pathetic"), then correspondence bias will result ("To look *that* pathetic she'd have to have been the nervous type to begin with"). The paradox is that instead of protecting him against unwarranted dispositional inferences, the observer's knowledge of the actor's situation may promote such inferences. Experimental evidence show that this comedy of errors may, in fact, unfold in precisely the way that Trope's model suggests (Snyder & Frankel, 1976; Trope & Cohen, 1989; Trope, Cohen, & Maoz, 1988). Observers who use the discounting principle may nonetheless display the correspondence bias when the actor's behavior has been assimilated to the observer's situation-based expectancy. In short, information about the situation giveth and information about the situation taketh away. When the first effect exceeds the second, correspondence bias results.

Realism Although theorists may disagree about the nature of the psychological processes by which observers come to understand actors, they generally agree that the process is transparent to the observer. As Thomas Reid (1764/1983) noted:

There are certain modifications of the human face, which are natural signs of the present disposition of the mind. Every man understands the meaning of these signs, but not one of a hundred ever attended to the signs themselves, or knows anything about them . . . (because) we pass from the sign to the thing signified with ease, and by natural impulse. (pp. 77-78)

Heider (1958) made the same point a few centuries later:

We may see that a person is displeased, without being able to say just what about his appearance or behavior gave us that impression In many cases of both thing and person perception the raw material remains phenomenally unidentifiable, the only fact that appears ready-made in our life space being the percept, the end product of the organizing process. (pp. 26-27)

In modern parlance we would say that the identification operation and part of the attribution operation (namely, the characterization stage) occur outside of awareness, and thus, observers have the experience of "seeing" rather than "inferring" what others are feeling or thinking. One consequence of this fact is that observers may disregard the psychological processes that translate realities into appearances and instead act as though the two were one, just as they disregard the physiological processes that translate light into visual images. This disregard is, of course, the familiar problem of realism, and that familiar problem can cause the correspondence bias in two ways. First, it may simply leave observers reluctant to consider the possibility that they have misconstrued the actor's behavior. For example, if the observer believes that Opal seemed to be positively pathetic because she *was* positively pathetic, and refuses to admit that his prior knowledge of Opal's situation might have made mild trepidation appear as pathos, then the observer "denies the possibility of questions which can in fact be asked" (Ayer, 1956, p. 113) and hence diminishes the possibility of achieving accurate answers. In this case, the error of realism compounds the error of idealism.

But just as realism may leave observers reluctant to consider the possibility that they have misconstrued the actor's behavior, so may it leave them reluctant to consider the possibility that they have misconstrued the actor's situation—and this second kind of reluctance can also cause the correspondence bias. The discounting principle suggests that observers should consider the actor's situation if they wish to achieve accurate inferences about the actor's dispositions. In fact, some situations directly compel or prohibit an actor's behavior (e.g., gale-force winds from center field do not allow batters to hit home runs) and these situations should be considered by the accuracy-minded observer. But many situations do not contain behavioral constraints that oblige or preclude an actor's behavior; rather they contain psychological constraints that encourage the actor to perform or not perform particular behaviors by altering the consequences associated with each performance (see Gilbert & Malone, 1995, p. 26). A debate coach's instructions do not literally prevent a debater from expressing her true opinion in the same way that a tongue removal might; rather, they make the consequences of arguing one position attractive ("You'll be the team captain next semester") and the consequences of

arguing another less so ("You'll be the towel boy"). When constraints are psychological rather than behavioral, the discounting principle requires that the observer consider the actor's situation as the actor *believes it to be* rather than *as it actually is*. If, for example, the debater is a recent transfer student from Neptune and does not know that debate coaches can inflict punishment, that instructions are meant to be followed, or that "pro-Castro" means "in favor of the hairy guy with the accent," then there is no situational cause of the debater's behavior for the observer to consider. When constraints are psychological, the observer must transcend his own understanding of the actor's situation ("The coach told him to defend Castro") and instead see the situation as the actor sees it ("But the new debater from Neptune doesn't seem to have a clue").⁶

Realism makes this sort of transcendence a challenging task. How is an observer to recognize that an actor's interpretation of the situation differs from the observer's when the observer does not even recognize that she is herself interpreting the situation rather than apprehending it directly? Putting oneself in another's shoes is precisely the problem that the "egocentric child" spends a lifetime trying to solve, and to the extent that this problem is not permanently solved so much as occasionally overcome, adult observers may find it difficult to obtain the information they need to avoid the correspondence bias (i.e., information about the actor's view of the situation; see Griffin & Ross, 1991). Fans of TV quiz shows, for instance, often fail to realize that it is one thing to generate answers to trivia questions in the comfort of one's own living room and quite another to do so in the presence of a camera, a studio audience, and hot lights. The observer's tendency toward realism may cause her to assume that the situation she experiences ("The capitol of Texas? Now *that's* an easy question") is also the situation that the actor experiences, and under some circumstances, this assumption may lead her to draw correspondence-biased inferences about the actor ("How could he miss it? That guy is a bona fide idiot"). Although decades of experimental research have shown that college students almost never refuse a request to read or write a counterattitudinal speech, Sherman (1980) asked college students to *predict* how they would respond to such a request and a whopping 47 percent claimed they would refuse. Why this massive misprediction? It seems likely that the situation these college students imagined (i.e., a polite request) is not the same situation that decades worth of college students have actually experienced (i.e., an assignment whose refusal would entail great embarrassment). But like the quiz show fan who does not appreciate how it actually feels to be in the contestant's seat, the predictor who does not appreciate how it actually feels to be in the experimenter's shoes can barely help but draw correspondence-biased inferences about the college student who agrees to "a polite request" to write a pro-Castro speech. Realism, then,

can cause the correspondence bias by causing observers to equate the actor's perception of the situation with their own.

A Critique of Pure Error

Research suggested that the correspondence bias occurs when observers fail to notice or consider the actor's situation (circumstantialism), misconstrue the actor's situation or behavior (idealism and realism), or feel a special need to predict the actor's behavior (egotism). But while some investigators were working out the causes of this error, others were beginning to question its significance. The early 1980s saw critiques such as "How Fundamental Is 'the Fundamental Attribution Error'?" (Harvey, Town, & Yarkin, 1981) followed by rejoinders such as "Let's Give the Fundamental Attribution Error Another Chance" (Reeder, 1982). Before long, a family squabble became a thriving controversy, and social psychologists were tangled in an epistemological web from which they were singularly unprepared to extricate themselves. How can one know when inferences are erroneous? How does one determine whether erroneous inferences are bad? If one claims that an inference is untrue, then mustn't one be able to say what truth is and how it can be known? Philosophers had been clarifying these issues for several thousand years (see Manktelow & Over, 1993), but with few exceptions (e.g., Kruglanski, 1989; Swann, 1984), social psychologists ignored what they might have learned from that quarter and conducted their own inquiry into the matter.

What is an inferential error? The logical approach suggested that people's inferences can properly be called errors when those inferences violate logical standards that most right-minded persons—including the inference maker—endorse in principle (see Dawes, 1998, in this *Handbook*). But during the last decade, that seemingly simple suggestion has been challenged on at least three fronts. First, some critics have claimed that logical rules are not special standards with which inferences ought to be contrasted, and that violations of logical rules are therefore not properly called errors (the critique of standards). Second, some critics have claimed that even if logical rules are special standards whose violations are properly called errors, these errors are unlikely to occur outside of the artificial contexts in which they have been most frequently demonstrated (the critique of generality). Third, some critics have claimed that even if logical violations are errors that do generalize from artificial to natural contexts, they are unlikely to have significant costs—and may even have significant benefits—for the ordinary person, and thus may not be "the wrong thing" for the ordinary person to do (the critique of consequences). Although these critiques are usually run together, it is useful to tease them apart and inspect each one separately.

Critique of Standards Reason has always had its enemies. Greek philosophy was countered by occultism, the French Enlightenment was lamented by the Romantics, and German rationalism was thoroughly denounced by the Sturm und Drang movement (see Berlin, 1980). Today's glorification of the irrational is known as postmodernism. The postmodern critique of psychology's approach to inferential error suggests that so-called errors are merely inferences that depart from some arbitrary standard and, as such, are neither bad nor wrong in any meaningful sense of those words (see Gergen, 1990; Sampson, 1991). For example, the discounting principle suggests that as the number of possible causal candidates for an effect increases, a person should become less certain about the role played by any one of them. This would seem to be little more than a self-evident application of probability theory—but wait. Why should probability theory have privileged status? Are there not other logical, or even nonlogical, ways of knowing that might be just as right or good or true? According to the postmodernist, no single view of reality is perfectly defensible, and thus all views must be equally indefensible. So-called inferential errors, then, merely reflect disparities between one person's (usually a powerful person's) view of reality and another person's (usually a vulnerable person's) view of reality, and because the former view is not irrefutably superior to the latter, such disparities do nothing to prove that the vulnerable person is wrong. One can call such disparities errors, but it barely makes sense to do so.

Pulling few punches, philosophers Solomon and Higgins (1996) suggest that "postmodernism has invited an obscurity and a pretentiousness almost unmatched in the long, often obscure and pretentious history of philosophy" and that, in fact, postmodernism "isn't a philosophy. It's at best a holding pattern, perhaps a cry of despair" (p. 301). Although such despair has conquered more than one English department, it is probably safe to say that the postmodern critique of social psychology's approach to inferential error has simply been dismissed. Is such a dismissal fair? Probably. It may be fair—even necessary—to dismiss out of hand any critique that denies the possibility that it can be disproved by denying the possibility of proof altogether. One cannot play tennis with a partner who claims the game has no rules and cannot be scored. In the end, the postmodern critique does not allow itself to be taken seriously, because for people to take each other's ideas seriously they must share a belief in the meaning of "taking seriously" that is itself not open to debate.

Nonetheless, the postmodern critique reminds us that inferential errors are departures from a standard and that even if one believes that there exists a legitimate standard with which judgments may be contrasted, the standard that a psychologist chooses in a particular instance may not be that one. It is a common belief that rational baseline theories (such as theories of probability) prescribe one and only

one correct solution for each problem, but there are many theories of probability and thus some problems have numerous solutions that experts consider equally defensible. For example, everyone acknowledges the importance of using base rates in making inferences, but not everyone agrees on how they ought to be used in a given instance (Koehler, 1996). There seem to be at least three or four reasonable ways to solve Tversky and Kahneman's (1980) well-known blue-green cab problem, and these methods lead to a variety of solutions, some of which are very much like the solutions that ordinary people generate and some of which are not. As Gigerenzer (1991) notes, "none of these statistical solutions is the only correct answer to the problem, and therefore it makes little sense to use the deviation between a subject's judgment and one of these statistical answers as the psychological explanandum" (p. 260). In other words, a logical rule may be true, but that does not mean it is the *only* true rule that one might use or that it is necessarily true in every conceivable context. Thus, even if one endorses the notion of logical standards in principle, it is often difficult to know which particular standard to endorse in practice.

This version of the critique of standards has not had serious impact on psychological research, in part because psychologists are often unaware that there are multiple correct solutions to problems, but in part because many of the problems that interest psychologists do not seem to have multiple correct solutions. This is especially true in social psychology, in which adherence to logical standards does not usually require that subjects' judgments hit a particular numerical mark (e.g., "I think the odds that the cab was blue are one in twenty-seven"), but rather requires only that judgments include some trace or no trace of a particular piece of information. For instance, when subjects know that every member of a group complied with a request to write an essay in which they presented themselves as extroverted, then the fact of their compliance is, by definition, not diagnostic of relative extroversion—and those who take compliance as evidence of relative extroversion are violating a logical standard that does not seem in this instance to have any serious competition (Miller, Jones, & Hinkle, 1981). The point, then, is that it is not always easy to say whether a particular solution to a complex problem is the one and only logically correct solution. Thus, the critique of standards provides a healthy caution for those who would exalt one solution above others.

Critique of Generality In the past fifty years the practice of social psychology has changed in many ways. One of the most significant changes has been its relocation. What was once a largely applied science that relied primarily on natural observation of ordinary people has become a largely theoretical science that relies almost exclusively on laboratory experiments involving college students. The dis-

cipline may have been founded on studies of religious cults and boy's camps, and professors in classrooms may still celebrate that work, but nothing even vaguely like it appears with any regularity in today's best journals. When Asch (1946) substituted trait adjectives for actual human behaviors, he sanctioned a method that was simple, cheap, tractable, clean, and hence instantly popular, but one that focused exclusively on judgments—and judgments of symbols at that. It is little wonder that critics of social psychology's approach to inferential error have seized on its method to challenge the evidence it produces. These criticisms have come from many sources, but three bear close consideration.

The Neo-Gibsonian Perspective James Gibson was a perceptual psychologist who considered the perceptual psychology of his day to be the science of the artificial. Gibson's own attitude toward perception was naturalistic, holistic, and functional (Gibson, 1979). His writings emphasized the fact that perceptual systems had evolved in a particular environment to respond to a particular set of problems—how to move in space, how to locate objects such as food and mates, and so on. Human brains, he argued, are not all-purpose information-processing systems that apply abstract principles of reflectance and parallax and for which the contents of visual problems are therefore largely interchangeable. Rather, they are the means by which terrestrial mammals solve problems in environments that they actively explore, to which they naturally respond, and of which they are an integral part. Thus, how people see outside of their natural environments may say nothing about how they see within them, because the visual system is merely a piece of a larger animal-environment system from which it cannot be meaningfully isolated. Just as a lamb chop on a platter says little about how sheep sleep, mate, or avoid their enemies (except, perhaps, that they are not entirely successful at the last of these), attempts to understand people by excising them from their natural environments and stuffing them into laboratory cubicles are misdirected and ill-fated.

Gibson generally confined his criticisms to the study of perceptual psychology, but because social psychologists have traditionally construed the task of understanding people as analogous to the task of seeing people, Gibson's followers found it relatively easy to export his criticisms to the study of ordinary personology. McArthur and Baron (1983), for example, argued that the stimuli used in the typical social psychology experiment are so impoverished and unrepresentative, and the information-gathering activities of subjects are so thoroughly restricted, that the errors that subjects make are, for all intents and purposes, the inferential equivalents of mutton. For example, if people reason not by general rules such as the discounting principle, but by domain-specific rules such as "Never trust a used car dealer," then experiments that require subjects to func-

tion in unfamiliar domains (e.g., judging the sincerity of a pro-Castro essayist) may underestimate the subject's capacity to operate successfully in more familiar spheres. The same may be true if observers would normally interrogate actors about whom they are uncertain ("But c'mon, what do you *really* think of Castro?"), create circumstances that would better test for the presence of dispositions ("I just whipped up some *ropa vieja*. Want a bite?"), or discuss their suspicions with confidants ("So what did *you* think of that speech?") rather than passing judgment on the basis of scant information (see Baron & Misovich, 1993). In short, the restrictive and unrealistic setting of the typical laboratory experiment may provide an unusually doleful picture of the ordinary person's inferential talents because these are not the settings for which these talents have been honed.

The Neo-Brunswickian Perspective When social psychologists embraced the logical approach, they pronounced the objective approach dead without actually checking its pulse. Attributionists and social cognitivists attempted to penetrate the mysteries of ordinary personology by developing models of the observer's inferential rules and mental processes, but some students of ordinary personology remained faithful to the Brunswickian tradition by focusing instead on the properties of the observed. For example, students of nonverbal behavior acknowledged their ethological heritage by attempting to map the relations between personological judgments and the actual grimaces, gaits, grunts, and growls that signified an actor's intentions and dispositions (see DePaulo & Friedman, 1998, in this *Handbook*). Personality psychologists, who had a long-standing interest in how personality is perceived (see Allport, 1961), also took the properties of the object as a starting point for their analysis because that object—the actor's personality—was the centerpiece of their science and thus could hardly be ignored. Neither of these research traditions attacked the problems of ordinary personology by explicating logical rules, and without logical rules there could be no logical standard for accuracy. Thus, in both traditions the object continued to be the standard against which the observer's judgments were weighed.

The objective approach, then, was dominated but not decimated by the logical approach, and in recent years it has reasserted itself with newfound vigor. For example, Kenny's (1994) social relations model offers an analytic procedure by which researchers may estimate the concordance between people's views of each other, of themselves, and of each other's views, without performing the statistical pratfalls proscribed by Cronbach (1955). Although the procedure is not limited to assessing the accuracy of an observer's beliefs about an actor, it provides a sophisticated method for doing so. Funder's (1995) realistic accuracy model is concerned primarily with the accuracy of personality judgments, and attempts to provide a framework in

which both the process and the object of judgment can be simultaneously considered. Because both of these models define accuracy as the discrepancy between a judgment and the object being judged, both naturally emphasize "the study of real people who have had a chance to observe or interact with each other" (Funder, 1994, p. 665). Neither of these neo-Brunswickian formulations suggests that laboratory experiments on inferential error are necessarily misleading, but both suggest that the generalizability of those findings is a matter that must itself be decided empirically. For the most part, researchers have studied inferential errors that "are displayed in experimental contexts carefully designed to evoke them, whereas their frequency and meaning in realistic contexts often goes unexamined" (Funder, 1995, p. 655; see also Jussim, 1991). Both of these models, then, suggest that experimenters can demonstrate inferential error until the cows come home, but until they venture into the meadow with a measuring tape, they cannot know if the results of their studies generalize a little, a lot, or at all, to the world of bovines and buttercups.

This admonition is particularly compelling when the factors that mandate the use of certain logical principles in the laboratory are known to be rare in the world outside. Consider experimental demonstrations of the correspondence bias as a case in point. If a situation (S) is known to have caused a behavior, then the role of the actor's disposition (D) ought to be discounted. This is attribution theory's most fundamental truth—but it is true only when S and D cause the behavior but do not also cause each other. If an actor's dispositions lead him into a constraining situation in the first place (i.e., if D causes S), or if an actor's situation is strong or enduring enough to bring about changes in her disposition (i.e., if S causes D), then the observer's certain knowledge of the presence of S should *increase* the observer's certainty about the presence of D rather than *decrease* it. For example, if only debaters with pro-Castro leanings tended to join the team (i.e., if D caused S), or if a year spent defending Castro tended to breed pro-Castro sympathies (i.e., if S caused D), then the mere fact that the actor was in the position to be instructed by his debate coach to make a pro-Castro speech would logically imply that the actor was dispositionally pro-Castro (see Gilbert & Malone, 1995, pp. 32–34). In short, the typical experimental conditions in which an actor's dispositions could not possibly have caused the actor's situation (because the actor was randomly assigned to the situation) and in which an actor's situation could not possibly have caused the actor's dispositions (because observers are asked to estimate the actor's attitude before the actor was randomly assigned) may not reflect familiar realities. Thus, the inferential errors demonstrated by such experiments cannot be presumed to have common counterparts in everyday life.

The Multicultural Perspective Jones (1985) correctly observed that modern social psychology is a largely North

American science, and that most of its experimental subjects have thus been white, English-speaking college students. Some researchers have suggested that the errors to which these particular subjects are prone may not occur in the same way—or at all—in different populations (see Fiske et al., 1998, in this *Handbook*; Markus & Kitayama, 1991; Newman, 1993; Smith & Bond, 1994). Miller (1984), for instance, showed that North Americans tend to emphasize dispositional causes in their descriptions of events more than do Indians. Morris and Peng (1994) have made a similar point with regard to North Americans and Chinese, and Lee, Hallahan, and Herzog (1996) found some evidence for this effect in the United States and Hong Kong. Of course, explanations that involve ordinary dispositions ("He enjoys eating") and situations ("The food was so appealing") are often two superficially distinct styles of speaking about the same thing; thus, cultural differences in free description may ultimately tell us more about sociolinguistic norms than about psychological processes (see McGill, 1989; Rholes, Newman, & Ruble, 1990). Furthermore, classic experimental demonstrations of the correspondence bias seem to replicate nicely in Japan (Toyama, 1990), as well as in Hong Kong and Taiwan (Krull et al., 1996).

Nonetheless, even if no compelling data suggested cultural differences in the process or the outcome of ordinary personology, the multicultural perspective would provide an important caveat for social psychologists, who cannot assume that inferential errors transcend the cultural contexts in which they are commonly demonstrated. It would not be surprising to find that some of the mechanisms that cause, say, the correspondence bias do transcend culture (e.g., the assimilative effects of situation-based expectancies) and that others do not (e.g., Western societies may fail to teach their members to consider situational antecedents), but such conclusions must wait for more thorough research. The multicultural perspective, then, suggests that the tale told by studies of one culture can never be more than a piece of the story.

The Value of Artifice All three of these critiques of generality make important arguments that students of ordinary personology will ignore at their peril. For example, it is undoubtedly true that modern social psychology has allowed the *in vitro* to eclipse the *in vivo*, that the consequences have not been entirely salutary, and that in response to all three sets of critiques (as well as to other societal and intellectual trends), the field is currently repositioning itself to achieve a more thoughtful balance between laboratory and field research (see Aronson, Wilson, & Brewer, 1998, in this *Handbook*; Schwarz, Groves, & Schuman, 1998, in this *Handbook*). It is also true that when psychologists move a phenomenon from the field to the cubicle or substitute a symbol for a deed, they gain control, and with it the right to make statements about cause-and-effect relations,

but they also sacrifice representativeness, and with it the right to generalize without further investigation. The laboratory experiments that have characterized the logical approach to ordinary personology have allowed investigators to isolate and interrogate numerous mechanisms that may lead to inferential error, but they have not enabled them to say how, whether, for whom, or with what result these mechanisms operate in natural settings. Whether speculations in this regard have been appropriately cautious is an aesthetic question that facts cannot resolve. In any event, the critique of generality is a healthy reminder that experimental demonstrations of inferential error are the beginning and not the end of a scientific journey.

Do these limits invalidate the evidence provided by experiments? It depends on what one asks the evidence to do. One can never know with certainty whether an error in the laboratory portends an error outside of it unless one lives in a world in which truly random sampling of subjects and stimuli is possible. Ours is rarely one of those worlds. Thus, psychologists must count on their common sense and discretion to move from descriptions of that which they observe to inferences about that which they do not. The first step in that process is to describe what has been observed with exceptional clarity, and experiments serve that function. Experiments are snapshots that capture and preserve events whose features are unmistakable and whose causes are clear. Thus, they provide a reliable springboard for speculation about how similar events might or might not occur beyond their spatial and temporal borders. Experiments do this very well, and to abandon them because they do not do everything else very well would be akin to junking one's lawnmower because it failed to peel onions or pick up Channel 10 (see Mook, 1983). In the end, the critique of generality is simply a statement about the limits of all scientific observations—even natural observations, which are not randomly made and therefore cannot determine what usually or normally or typically happens. Experiments tend to tell us *why* errors occur, field studies tend to tell us *whether* errors occur, and both of these are things we want to know.

Critique of Consequences William James worried more than most folks do about how truth can be distinguished from falsehood, and when he came upon an essay in *Popular Science Monthly* written by Charles S. Pierce (1878) and titled simply "How to Make Our Ideas Clear," he believed he had found the answer. The answer was that truth is that which works, and he quickly made this notion the centerpiece of his own epistemology, which he called pragmatism (James, 1907).⁷ James had earlier argued that "my thinking is first and last and always for the sake of my doing" (1980b, p. 333), which is to say that people have thoughts about the world because doing so helps them negotiate that world more artfully and thereby achieve their

goals more consistently (see Fiske, 1992, 1993). The transition from his functionalist perspective on cognition to his new pragmatic perspective on truth was a natural one: if thinking is for doing, then thinking must be correct when doing is successful. Just as a shovel is the right shovel when it gets the job done, so mental representations of the world are right, correct, accurate, or true when they optimize the person's ability to function in the world that is being represented. According to James, then, one may talk about truth only by talking about the consequences that the truth has for those who achieve it.

It goes without saying that many of the abstract truths that delight mathematicians, logicians, and philosophers have no consequences whatever for ordinary people. H. G. Wells (1932) noted with some amusement that "no appreciable effect has been produced upon the teaching of machine drawing by the possibility that space is curved and expanding" (p. 36), and indeed, to this day engineers do not worry about the red shift when they design bridges and tunnels. In fact, if an engineer believes that—and hence *acts* as though—space is linear and static, she can be fairly certain that none of her bridges will collapse as a result of that belief. Moreover, the calculation of a span's tensile strength is greatly simplified by ignoring the inflation of the cosmos. According to the pragmatic perspective, then, the engineer's belief in linear and static space works, and as such, is true—really, *truly* true. Such a claim has obvious consequences for the notion of inferential error. Can the cosmologist look at the engineer's calculations and say that she made a mistake? If her goal was to build a suspension bridge, and if her beliefs about space and time enabled her to do that, then in what sense can those beliefs be said to be wrong? Who would argue that she *ought* to have believed otherwise?

Several writers have considered inferential errors in ordinary personology from this pragmatic perspective and have concluded that although people violate logical standards, and although they do so both in and out of the laboratory, the consequences of these violations are often null, sometimes beneficial, and hence ought not to be called errors. For example, Swann (1984) has argued that in everyday life, persons and situations are naturally and highly correlated, and thus it may make little sense for the observer to struggle to disentangle the two (but see Kenny et al., 1996). The observer who overlooks the behavioral requirements of a bank president's high office and instead attributes the president's conservative demeanor to the president's dispositional reserve may be transgressing against the discounting principle; but if the observer is a teller who will never encounter the president outside the bank, then this logical violation will serve the observer well inasmuch as it will enable him to predict the bank president's behavior toward him. Swann (1984) notes that "perceivers are often more concerned with whether their beliefs are true for them than whether they are true in general," and that

"the accuracy of social beliefs is therefore determined by how well they serve the goals of perceivers rather than by the extent to which they are accurate in the ultimate sense" (p. 461). There are, of course, countless other instances in which observers violate logical principles and reap rich rewards, or at least fail to suffer any unfortunate consequences (Funder, 1987; McArthur & Baron, 1983; Nisbett & Ross, 1980; Swann, 1984).

The pragmatic perspective is easy to swallow in one sense and a bit of a gag in another. The easy part is that it suggests that following logical rules may not always serve a person's legitimate interests, and that logical prescriptions should therefore not be inexorably translated into moral imperatives. Few social psychologists would want to argue that people *ought* to behave logically when doing so would not serve them well. Most, however, fairly bristle when the pragmatist tells them that because beliefs that serve a person's interests are acceptable, useful, or even desirable, then they must also be accurate, right, and true. Pragmatism equates accuracy with expedience because, for pragmatists, truth is a meaningless abstraction that only the *use* of truth can solidify. But whether meaningful or not, the abstraction is surely clear enough to become the subject of discourse, and thus, it can only be confusing to assign the same word—"true"—to beliefs that are logically correct (and that may or may not be useful) and beliefs that are useful (and that may or may not be logically correct). If just for a moment we were to set aside this word entirely and agree to refer to beliefs that violate logical canons as *noncanonical* and to refer to beliefs that do not serve the person's needs as *maladaptive*, then much of the debate around the pragmatist critique would instantly evaporate. Most social psychologists would readily admit that people's inferences may be noncanonical and adaptive, that this fact helps explain why the tendency toward inferential error is not always cured by people's experience in the world, and that the question of what constitutes truth "in the ultimate sense" is probably best left to philosophers.

Errors in Perspective

Research has demonstrated that people make mistakes. Although cautious researchers have generally refrained from offering blanket assessments on the basis of such evidence, the fact that inferential errors capture the headlines in both journals and newspapers has led many to think of ordinary personologists as woefully imperfect. Indeed they are imperfect. But just how woeful is that imperfection? As Kenny (1994) noted, any batter who could consistently hit .350 in the major leagues would have a multimillion-dollar contract on his hands, and any coach who mourned that imperfection would betray a stunning ignorance of baseball. Furthermore, wise coaches know that the batting cage

is only something like the ballpark. Laboratory studies have elucidated the mechanisms that might lead to errors in the world beyond, and in so doing have captured some of life's most embarrassing moments in vivid detail. But just as often, studies in natural contexts seem to catch people with their pants up, showing that observers are capable of making accurate judgments of others on the basis of wafer-thin slices of behavior (Albright, Kenny, & Malloy, 1988; Ambady & Rosenthal, 1992, 1993; Funder & Colvin, 1988; Paunonen, 1991; Watson, 1989). Where, then, does the truth lie? The truth doesn't lie. Asking whether people are good judges of others is a bit like asking whether people are good cooks or good parents or good lovers. Some are, at some times, for someone, but it depends. Research strives to tell us what it depends *on*, and when what it depends *on happens*. At present, the scientific evidence allows neither a condemnation nor a commendation of the ordinary personologist. As Funder (1995) noted:

[H]uman judgment is sometimes right and sometimes wrong. Any further characterization of it as pathetic or admirable is a value judgment that depends more on what one expected to find and perhaps on the degree of one's dispositional optimism or pessimism than on scientific evidence. (p. 666)

Rather than looking for simple labels that aggrandize or deprecate human inference, students of ordinary personology would do well to notice what errors tell us about its nature and origin. Inferential errors are not random deviations from a standard; rather, they are often the result of habits of thought that work quite well in one context but not so well in another. Most of the errors that social psychologists study suggest that people are marvelously adapted to understand the personal microcosms they inhabit rather than the bigger picture of which their experience is but one small and idiosyncratic slice. The mind shapes its experience of the world and then uses that experience to discern the shape of the world, sometimes failing to appreciate fully the limits of its acumen, the power of its passions, and the uniqueness of its perspectives, but such habits are a natural consequence of both our evolutionary and cultural pasts. Human brains were not evolved to do philosophy or physics, but to locate food, court mates, and raise offspring in the "local now." And until quite recently, human societies were precisely the sorts of isolated, small, and stable social worlds through which modern people still seem prepared to maneuver. When the agricultural revolution brought exploding populations and shifting social structures, the ordinary person's social world was instantly made larger and more varied than ever before. Minds that were designed and trained to respond most powerfully to their personal experience were suddenly required to transcend that experience if they wished to understand their

newly extended worlds—required to move beyond the bounded points of view that had served their ancestors so well for millennia. We make errors when we judge others because the puzzle presented by their behavior has evolved more quickly than our ability to solve it. Perhaps, then, our mistakes should not be taken as indications of our imperfection so much as evidence of our expanding universe.

OUR CHILDREN: THE FUTURE OF ORDINARY PERSONOLOGY

This chapter has dwelt on history more than most, tracing a wobbly line from early research on the perception of emotional expression through the development of the attributional and social cognitive traditions, and finally, to the study of inferential error and its discontents. When a tour of the past arrives at the present, the tour guide often feels obligated to make a speech about the future before letting everyone off the bus. But those who have heard such speeches before know that, in the long run, they generally prove more charming than prophetic and usually characterize the times in which they were made better than the times at which they are aimed. If the past is a reliable guide, we should expect the future's most exciting developments to be those we do not at present expect—those for which we do not yet have a language, those for which we do not yet have an urge. What lies beyond the logical approach? How will we meet the methodological and theoretical challenges that are arising in the twilight of the social cognitive tradition? Have the children of Asch and Brunswick and Heider laid the foundation on which a future science will stand, or will the ideas described in these pages someday be relegated to a footnote that pays brief homage to the work that was done before the *real* work began? These are questions that we cannot hope to answer, just yet. But if every tour guide is obligated to make at least one prediction, then surely this is the safest: The next edition of this chapter will have news—exciting news, good news, big news—to tell us.

This chapter has dwelt on history more than most, beginning with discoveries and ending with disputes. That narrative device risks leaving the reader with the impression that the canon is in crisis, that the naysayers have successfully barbecued the flag bearers, and that what looked like triumph in the road ahead has proved to be vainglory in the rearview. A cursory glance at chapters on this topic in previous editions of the *Handbook* should dispel any such notion. They reveal that the conventional wisdom being criticized today was itself the critique of yesterday's conventions, and that rather than leaving the corpus wounded and bleeding, critiques of research on ordinary personology are shaping it, strengthening it, and ultimately

goaded it toward a more mature stage of development. Attribution theory was the critical movement that replaced the flailing objective approach, social cognition was a response to the processless models of attribution theory, and today new traditions are emerging in response to the weaknesses of social cognition. In every era, the tribe's elders lament the passing of the old as much as the youngsters delight in the inauguration of the new. The vanguard brings with it new perspectives and new practices, and the old guard bemoans new standards that would prevent classic studies from being published in modern journals. All of which is precisely what happens when the students are listening closely to, and then surpassing, their teachers. Perhaps the best sign of a healthy science is the presence of grumpy old men and women, and by that reckoning, the study of ordinary personology is the very picture of well-being.

This chapter has dwelt on history more than most, documenting the changes over the last fifty years that describe and define social psychology's approach to ordinary personology. Not all of those changes have been for the best. Perhaps because we live in a society that values low cable rates over answers to difficult and enduring questions about the human condition, social psychologists who ponder such questions for a living seem to have developed a talent for self-doubt that is somewhat sharper than that of their intellectual forebearers. That talent leaves them oddly receptive to recycled, sophomoric attacks on their discipline, and strangely disconsolate when problems that have perplexed thinkers for centuries fail to yield in a single funding cycle. But social psychology has been too successful to play a convincing rendition of those blues. The fact that a chapter of this length can merely highlight some of what we know now but could not even imagine just fifty years ago should be a powerful reminder of the long-term utility of our approach. Yes, that approach has allowed us to stumble, fall flat, and take wrong turns—but where is the science that has learned more in an equally brief moment? Surely the gloom that characterizes so much of our talk around the table is as unproductive as it is unwarranted. There is ignorance for us to vanquish and there are mistakes for us to remedy, and if our past has any lesson, it is that both these things will be done—by us, and then soon by those who will shake their heads at our naiveté and leave us grouching about the good old days.

NOTES

1. The phrase "social cognition" (which was probably first used by Bruner and Tagiuri [1954, p. 634]) describes such a wide variety of research enterprises and topics that its literal translation is almost meaningless. The phrase is used here to refer specifically to a research tradition in social psychology that came to prominence in the mid- to

- late 1970s, was described early by Hastie et al. (1980), and was reviewed successively by Higgins and Bargh (1987), Sherman, Judd, and Park (1989), and Fiske (1993). For histories of the social cognitive movement see Landman and Manis (1983), Ostrom (1984), and Fiske and Taylor (1991).
2. Ironically, Asch had no interest in this research. As late as 1983, he would invite graduate students into his Princeton University office and ask them to describe everything that had been learned about ordinary personology since 1946. He would listen politely to the student's response and, at the end of the hour, look at his watch, close his book bag, announce that nothing much had been learned, and suggest that a follow-up to his classic paper was in order. He published that paper (Asch & Zukier, 1984) a few years before his death.
 3. Models such as Anderson's (1974) do a nice job of predicting the *valence* of an impression from the *valence* of its components, but no model can predict "brooding intellectual" from "taciturn" and "scholarly."
 4. Ironically, Helmholtz was persuaded to disavow the notion of the *unbewusster schluss* or unconscious inference. Unlike Galileo, he was never heard to mutter, "Nonetheless, it infers!"
 5. Philosophers distinguish naive or direct realism from representational or indirect realism, but that distinction is not critical for the present purposes.
 6. The tendency to confuse behavioral and psychological constraints is another unfortunate consequence of the idea of *situational causation*, which equates hurricanes and bribes and thus blurs the distinction between external stimuli that directly cause behavior (hurricanes) and external events that facilitate behavior through their appeal to some internal state or trait (bribes).
 7. Ironically, Pierce did not appreciate being rescued from obscurity, and promptly changed the name of his philosophy to *pragmatism* so as to distinguish it from James's bastardizing version.
- ### REFERENCES
- Abelson, R. P., & Lalljee, M. (1988). Knowledge structures and causal explanation. In D. J. Hilton (Ed.), *Contemporary science and natural explanation: Commonsense conceptions of causality* (pp. 175-203). Brighton, England: Harvester Press.
- Albright, L., Kenny, D. A., & Malloy, T. E. (1988). Consensus in personality judgments at zero acquaintance. *Journal of Personality and Social Psychology*, 55, 387-395.
- Allport, G. W. (1954). *The nature of prejudice*. New York: Addison Wesley.
- Allport, G. W. (1961). *Pattern and growth in personality*. New York: Holt, Rinehart, & Winston.
- Ambady, N., & Rosenthal, R. (1992). Thin slices of behavior as predictors of interpersonal consequences: A meta-analysis. *Psychological Bulletin*, 111, 256-274.
- Ambady, N., & Rosenthal, R. (1993). Half a minute: Predicting teacher evaluations from thin slices of behavior and physical attractiveness. *Journal of Personality and Social Psychology*, 64, 431-441.
- Anderson, C. A., Lepper, M. R., & Ross, L. (1980). Perseverance of social theories: The role of explanation in the persistence of discredited information. *Journal of Personality and Social Psychology*, 39, 1037-1049.
- Anderson, J. R. (1976). *Language, memory, and thought*. Hillsdale, NJ: Erlbaum.
- Anderson, J. R. (1990). *The adaptive character of thought*. Hillsdale, NJ: Erlbaum.
- Anderson, N. H. (1974). Cognitive algebra: Integration theory applied to social attribution. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 7, pp. 1-101). New York: Academic Press.
- Anderson, N. H., & Jacobson, A. (1965). Effects of stimulus inconsistency and discounting instructions in personality impression formation. *Journal of Personality and Social Psychology*, 2, 531-539.
- Aronson, E., Wilson, T. D., & Brewer, M. B. (1998). Experimentation in social psychology. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 1, pp. 99-142). New York: McGraw-Hill.
- Asch, S. E. (1946). Forming impressions of personality. *Journal of Abnormal and Social Psychology*, 41, 1230-1240.
- Asch, S. E. (1951). Effects of group pressure on the modification and distortion of judgments. In H. Guetzkow (Ed.), *Groups, leadership, and men* (pp. 177-190). Pittsburgh: Carnegie Press.
- Asch, S. E., & Zukier, H. (1984). Thinking about persons. *Journal of Personality and Social Psychology*, 46, 1230-1240.
- Ayer, A. J. (1956). *The problem of knowledge*. Baltimore: Penguin.
- Bacon, F. (1994). *Novum organum*. (P. Urbach & J. Gibson, Trans. & Ed.). Chicago: Open Court. (Original work published 1620)
- Bargh, J. A. (1989). Conditional automaticity: Varieties of automatic influence in social perception and cognition. In J. S. Uleman & J. A. Bargh (Eds.), *Unintended thought* (pp. 3-51). New York: Guilford.
- Bargh, J. A. (1990). Auto-motives: Preconscious determinants of social interaction. In E. T. Higgins & R. M. Sorrentino (Eds.), *Handbook of motivation and cognition* (Vol. 2, pp. 93-130). New York: Guilford.
- Bargh, J. A., Bond, R. N., Lombardi, W., & Tota, M. E. (1986). The additive nature of chronic and temporary sources of construct accessibility. *Journal of Personality and Social Psychology*, 43, 437-449.

- Baron, R. M., & Misovich, S. J. (1993). Dispositional knowing from an ecological perspective. *Personality and Social Psychology Bulletin*, 19, 541-552.
- Barr, R., Kamil, M. L., Mosenthal, P., & Pearson, P. D. (Eds.). (1991). *Handbook of reading research* (Vol. 2). New York: Longman.
- Bassili, J. N. (1989). Trait encoding in behavior identification and dispositional inference. In J. N. Bassili (Ed.), *On-line cognition in person perception* (pp. 61-89). Hillsdale, NJ: Erlbaum.
- Bassili, J. N., & Smith, M. C. (1986). On the spontaneity of trait attribution: Converging evidence for the role of cognitive strategy. *Journal of Personality and Social Psychology*, 50, 239-245.
- Bem, D. J. (1967). Self-perception: An alternative interpretation of cognitive dissonance phenomena. *Psychological Review*, 74, 183-200.
- Berlin, I. (1980). *Against the current: Essays in the history of ideas*. New York: Viking.
- Berscheid, E., & Reis, H. T. (1998). Attraction and close relationships. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 193-281). New York: McGraw-Hill.
- Boring, E. G., & Titchener, E. B. (1923). A model for the demonstration of facial expression. *American Journal of Psychology*, 34, 471-485.
- Borkenau, P. (1986). Toward an understanding of trait interrelations: Acts as instances of several traits. *Journal of Personality and Social Psychology*, 51, 371-381.
- Boski, P., & Rudmin, F. W. (1983). Ichheiser's theories of personality and person perception: A classic that still inspires. *Journal for the Theory of Social Behaviour*, 19, 263-296.
- Bransford, J. D., & Franks, J. J. (1971). The abstraction of linguistic ideas. *Cognitive Psychology*, 2, 331-350.
- Brewer, M. B., & Brown, R. J. (1998). Intergroup relations. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 554-594). New York: McGraw-Hill.
- Broadbent, D. (1958). *Perception and communication*. London: Pergamon.
- Bruner, J. S. (1957). On perceptual readiness. *Psychological Review*, 64, 123-152.
- Bruner, J. S., & Tagiuri, R. (1954). The perception of people. In G. Lindzey (Ed.), *The handbook of social psychology* (1st ed., Vol. 2, pp. 634-654). Reading, MA: Addison-Wesley.
- Brunswick, E. (1947). *Systematic and representative design of psychological experiments*. Berkeley: University of California Press.
- Buss, D. M., & Kenrick, D. T. (1998). Evolutionary social psychology. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 982-1026). New York: McGraw-Hill.
- Cantor, N., & Mischel, W. (1977). Traits as prototypes: Effects on recognition memory. *Journal of Personality and Social Psychology*, 35, 38-48.
- Carlston, D. E. (1980). Events, inferences, and impression formation. In R. Hastie, T. M. Ostrom, E. B. Ebbesen, R. S. Wyer, D. L. Hamilton, & D. E. Carlston (Eds.), *Person memory: The cognitive basis of social perception* (pp. 89-119). Hillsdale, NJ: Erlbaum.
- Carlston, D. E. (1992). Impression formation and the modular mind: The associated systems theory. In L. Martin & A. Tesser (Eds.), *Construction of social judgment* (pp. 301-341). Hillsdale, NJ: Erlbaum.
- Carlston, D. E., & Skowronski, J. J. (1994). Savings in the relearning of trait information as evidence for spontaneous inference generation. *Journal of Personality and Social Psychology*, 66, 840-856.
- Carlston, D. E., Skowronski, J. J., & Sparks, C. (1995). Savings in the relearning: II. On the formation of behavior-based trait associations and inferences. *Journal of Personality and Social Psychology*, 69, 420-436.
- Cheng, P. W., & Novick, L. R. (1992). Covariation in natural causal induction. *Psychological Review*, 99, 365-382.
- Cooper, W. H. (1981). Ubiquitous halo. *Psychological Bulletin*, 90, 218-224.
- Crocker, J., Major, B., & Steele, C. (1998). Social stigma. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 504-553). New York: McGraw-Hill.
- Cronbach, L. J. (1955). Processes affecting score on "understanding of others" and "assumed similarity." *Psychological Bulletin*, 52, 281-302.
- Cronbach, L. J. (1958). Proposals leading to analytic treatment of social perception scores. In R. Tagiuri & L. Petrillo (Eds.), *Person perception and interpersonal behavior* (pp. 353-379). Stanford, CA: Stanford University Press.
- Darley, J. M., & Latané, B. (1968). Bystander intervention in emergencies: Diffusion of responsibility. *Journal of Personality and Social Psychology*, 8, 377-383.
- Darwin, C. (1872/1979). *The expression of the emotions in man and animals*. London: Julian Friedmann.
- Dawes, R. M. (1998). Behavioral decision making and judgment. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 1, pp. 497-548). New York: McGraw-Hill.
- Deaux, K., & LaFrance, M. (1998). Gender. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 1, pp. 788-827). New York: McGraw-Hill.
- Dennett, D. C. (1987). *The intentional stance*. Cambridge, MA: Bradford/MIT Press.
- DePaulo, B. M., & Friedman, H. S. (1998). Nonverbal communication. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 3-40). New York: McGraw-Hill.
- Devine, P. G. (1989). Stereotypes and prejudice: Their auto-

- matic and controlled components. *Journal of Personality and Social Psychology*, 56, 5-18.
- Devine, P. G., & Elliot, A. J. (1995). Are racial stereotypes really fading? The Princeton trilogy revisited. *Personality and Social Psychology Bulletin*, 21, 1139-1150.
- Dretske, F. I. (1988). *Explaining behavior: Reasons in a world of causes*. Cambridge, MA: MIT Press.
- Dunning, D., Griffin, D. W., Milojkovic, J., & Ross, L. (1990). The overconfidence effect in social prediction. *Journal of Personality and Social Psychology*, 58, 568-581.
- Erdelyi, M. H. (1974). A new look at the New Look: Perceptual defense and vigilance. *Psychological Review*, 81, 1-25.
- Erdelyi, M. H. (1985). *Psychoanalysis: Freud's cognitive psychology*. New York: Freeman.
- Felekey, A. (1914). The expression of the emotions. *Psychological Review*, 21, 33-41.
- Festinger, L., & Carlsmith, J. (1959). Cognitive consequences of forced compliance. *Journal of Abnormal and Social Psychology*, 58, 203-210.
- Fiske, A. P., Kitayama, S., Markus, H. R., & Nisbett, R. E. (1998). The cultural matrix of social psychology. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 915-981). New York: McGraw-Hill.
- Fiske, S. T. (1992). Thinking is for doing: Portraits of social cognition from daguerreotype to laserphoto. *Journal of Personality and Social Psychology*, 63, 877-889.
- Fiske, S. T. (1993). Social cognition and social perception. *Annual Review of Psychology*, 44, 155-194.
- Fiske, S. T. (1998). Stereotypes, prejudice, and discrimination. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 2, pp. 357-411). New York: McGraw-Hill.
- Fiske, S. T., & Taylor, S. E. (1991). *Social cognition* (2nd ed.). New York: McGraw-Hill.
- Flavell, J. H. (1963). *The developmental psychology of Jean Piaget*. Princeton, NJ: Van Nostrand.
- Fodor, J. A. (1983). *The modularity of mind*. Cambridge, MA: MIT Press.
- Forgas, J. P. (1995). Mood and judgment: The affect infusion model (AIM). *Psychological Bulletin*, 117, 39-66.
- Funder, D. C. (1987). Errors and mistakes: Evaluating the accuracy of social judgment. *Psychological Bulletin*, 101, 75-90.
- Funder, D. C. (1995). On the accuracy of personality judgment: A realistic approach. *Psychological Review*, 102, 652-670.
- Funder, D. C., & Colvin, C. R. (1988). Friends and strangers: Acquaintanceship, agreement, and the accuracy of personality judgment. *Journal of Personality and Social Psychology*, 55, 149-158.
- Gage, N. L., & Cronbach, L. J. (1955). Conceptual and methodological problems in interpersonal perception. *Psychological Review*, 62, 411-423.
- Gergen, K. J. (1973). Social psychology as history. *Journal of Personality and Social Psychology*, 26, 309-320.
- Gergen, K. J. (1990). Toward a postmodern psychology. *Humanistic Psychologist*, 18, 23-34.
- Gibson, J. J. (1979). *The ecological approach to visual perception*. Boston: Houghton-Mifflin.
- Gigerenzer, G. (1991). From tools to theories: A heuristic of discovery in cognitive psychology. *Psychological Review*, 98, 254-267.
- Gilbert, D. T. (1991). How mental systems believe. *American Psychologist*, 46, 107-119.
- Gilbert, D. T. (1992). The assent of man: Mental representation and the control of belief. In D. M. Wegner & J. Pennebaker (Eds.), *The handbook of mental control* (pp. 57-87). New York: Prentice-Hall.
- Gilbert, D. T., & Gill, M. (1996). *The momentary realist*. Unpublished manuscript.
- Gilbert, D. T., & Hixon, J. G. (1991). The trouble of thinking: Activation and application of stereotypic beliefs. *Journal of Personality and Social Psychology*, 60, 509-517.
- Gilbert, D. T., Krull, D. S., & Pelham, B. W. (1988). Of thoughts unspoken: Social inference and the self-regulation of behavior. *Journal of Personality and Social Psychology*, 55, 685-694.
- Gilbert, D. T., & Malone, P. S. (1995). The correspondence bias. *Psychological Bulletin*, 117, 21-38.
- Gilbert, D. T., & Osborne, R. E. (1989). Thinking backward: Some curable and incurable consequences of cognitive busyness. *Journal of Personality and Social Psychology*, 57, 940-949.
- Gilbert, D. T., Pelham, B. W., & Krull, D. S. (1988). On cognitive busyness: When person perceivers meet persons perceived. *Journal of Personality and Social Psychology*, 54, 733-740.
- Gilovich, T. (1991). *How we know what isn't so: The fallibility of human reason in everyday life*. New York: Free Press.
- Goldiamond, I. (1958). Indicators of perception: I. Subliminal perceptions, subception, unconscious perception: An analysis in terms of psychophysical indicator methodology. *Psychological Bulletin*, 55, 373-411.
- Goldman, A. I. (1970). *A theory of human action*. Princeton, NJ: Princeton University Press.
- Greenwald, A. G. (1980). The totalitarian ego: Fabrication and revision of personal history. *American Psychologist*, 35, 603-618.
- Greenwald, A. G. (1992). New Look 3: Unconscious cognition reclaimed. *American Psychologist*, 47, 766-779.
- Grice, H. P. (1975). Logic and conversation. In P. Cole & J. L. Morgan (Eds.), *Syntax and semantics: Vol. 3. Speech acts* (pp. 41-58). New York: Seminar Press.
- Griffin, D. W., Dunning, D., & Ross, L. (1990). The role of construal processes in overconfident predictions about the self and others. *Journal of Personality and Social Psychology*, 59, 1128-1139.

- Griffin, D. W., & Ross, L. (1991). Subjective construal, social inference, and human misunderstanding. In M. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 24, pp. 319-356). New York: Academic Press.
- Hamilton, D. L. (1988). Causal attribution viewed from an information processing perspective. In D. Bar-Tal & A. W. Kruglanski (Eds.), *The social psychology of knowledge* (pp. 359-385). Cambridge: Cambridge University Press.
- Hamilton, D. L., & Sherman, S. J. (1996). Perceiving persons and groups. *Psychological Review*, 103, 236-355.
- Hamilton, D. L., & Zanna, M. P. (1974). Context effects in impression formation: Changes in connotative meaning. *Journal of Personality and Social Psychology*, 29, 649-654.
- Harvey, J. H., Town, J. P., & Yarkin, K. L. (1981). How fundamental is "the fundamental attribution error"? *Journal of Personality and Social Psychology*, 40, 346-349.
- Hasher, L., & Zacks, R. T. (1979). Automatic and effortful processes in memory. *Journal of Experimental Psychology: General*, 108, 356-388.
- Hastie, R., Ostrom, T. M., Ebbesen, E. B., Wyer, R. S., Hamilton, D. L., & Carlston, D. E. (Eds.). (1980). *Person memory: The cognitive basis of social perception*. Hillsdale, NJ: Erlbaum.
- Hegel, G. W. F. (1977). *The phenomenology of spirit*. (A. V. Miller, Trans.). Oxford: Oxford University Press. (Original work published 1807)
- Heider, F. (1944). Social perception and phenomenal causality. *Psychological Review*, 51, 358-374.
- Heider, F. (1958). *The psychology of interpersonal relations*. New York: Wiley.
- Heider, F., & Simmel, M. (1944). An experimental study of apparent behavior. *American Journal of Psychology*, 57, 243-259.
- Helmholtz, H. von. (1925). *Treatise on psychological optics* (Vol. 3, 3rd ed., J. P. C. Southall, Trans.). Menasha, WI: Banta. (Original work published 1910)
- Hendrick, C., & Constantini, A. F. (1970). Effects of varying trait inconsistency and response requirements on the primary effect in impression formation. *Journal of Personality and Social Psychology*, 15, 158-164.
- Hewstone, M., & Jaspars, J. (1987). Covariation and causal attribution: A logical model of the intuitive analysis of variance. *Journal of Personality and Social Psychology*, 53, 663-672.
- Higgins, E. T. (in press-a). Biases in social cognition: "Aboutness" as a general principle. In C. McGarty & S. A. Haslam (Eds.), *The message of social psychology*. Oxford: Blackwell.
- Higgins, E. T. (in press-b). Knowledge activation: Accessibility, applicability, and salience. In E. T. Higgins & A. W. Kruglanski (Eds.), *Social psychology: Handbook of basic principles*. New York: Guilford.
- Higgins, E. T., & Bargh, J. A. (1987). Social cognition and social perception. *Annual Review of Psychology*, 38, 369-425.
- Higgins, E. T., Bargh, J. A., & Lombardi, W. (1985). The nature of priming effects on categorization. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 11, 59-89.
- Higgins, E. T., Rholes, W. S., & Jones, C. R. (1977). Category accessibility and impression formation. *Journal of Experimental Social Psychology*, 13, 141-154.
- Hilton, D. J., & Knibbs, C. S. (1988). The knowledge-structure and inductivist strategies in causal attribution: A direct comparison. *European Journal of Social Psychology*, 18, 79-92.
- Hilton, D. J., & Slugoski, B. R. (1986). Knowledge-based causal attribution: The abnormal conditions focus model. *Psychological Review*, 93, 75-88.
- Hilton, D. J., Smith, R. H., & Kim, S. H. (1995). Processes of causal explanation and dispositional attribution. *Journal of Personality and Social Psychology*, 68, 377-387.
- Hume, D. (1740/1978). *A treatise on human nature*. Oxford: Oxford University Press.
- Hundert, E. M. (1989). *Philosophy, psychiatry, and neuroscience: Three approaches to the mind*. Oxford: Oxford University Press.
- Ichheiser, G. (1943). Misinterpretations of personality in everyday life and the psychologist's frame of reference. *Character and Personality*, 12, 145-160.
- Ichheiser, G. (1949). Misunderstandings in human relations: A study in false social perception. *American Journal of Sociology*, 55, Part 2.
- Ickes, W., Robertson, E., Tooke, W., & Teng, G. (1986). Naturalistic social cognition: Methodology, assessment, and validation. *Journal of Personality and Social Psychology*, 51, 66-82.
- Jacoby, L. L., Lindsay, D. S., & Toth, J. P. (1992). Unconscious influences revealed: Attention, awareness, and control. *American Psychologist*, 47, 802-809.
- James, W. (1890a). *The principles of psychology* (Vol. 1). New York: Holt.
- James, W. (1890b). *The principles of psychology* (Vol. 2). New York: Holt.
- James, W. (1907). *Pragmatism*. New York: Longmans-Green.
- Jones, E. E. (1979). The rocky road from acts to dispositions. *American Psychologist*, 34, 107-117.
- Jones, E. E. (1983, October). *Reflections on writing a history of social psychology (or, History as social psychology)*. Paper presented at the meeting of the Society for Experimental Social Psychology, Pittsburgh, PA.
- Jones, E. E. (1985). Major developments in social psychology during the past five decades. In G. Lindzey and E. Aronson (Eds.), *The handbook of social psychology* (3rd ed., Vol. 1, pp. 47-108). New York: Random House.
- Jones, E. E. (1990). *Interpersonal perception*. New York: Macmillan.
- Jones, E. E. (1993). The social in cognition. In G. Harman (Ed.), *Conceptions of the human mind: Essays in honor of George A. Miller* (pp. 85-98). Hillsdale, NJ: Erlbaum.

- Jones, E. E., & Davis, K. E. (1965). From acts to dispositions: The attribution process in person perception. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 2, pp. 219-266). New York: Academic Press.
- Jones, E. E., Davis, K. E., & Gergen, K. J. (1961). Role playing variations and their informational value for person perception. *Journal of Abnormal and Social Psychology*, 63, 302-310.
- Jones, E. E., & Harris, V. A. (1967). The attribution of attitudes. *Journal of Experimental Social Psychology*, 3, 1-24.
- Jones, E. E., & McGillis, D. (1976). Correspondent inferences and the attribution cube: A comparative reappraisal. In J. H. Harvey, W. J. Ickes, and R. F. Kidd (Eds.), *New directions in attribution research* (Vol. 1, pp. 389-420). Hillsdale, NJ: Erlbaum.
- Jones, E. E., & Nisbett, R. E. (1971). The actor and the observer: Divergent perceptions of the causes of behavior. In E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 79-94). Morristown, NJ: General Learning Press.
- Jones, E. E., & Thibaut, J. W. (1958). Interaction goals as bases of inference in interpersonal perception. In R. Tagiuri & L. Petrillo (Eds.), *Person perception and interpersonal behavior* (pp. 151-178). Stanford, CA: Stanford University Press.
- Jussim, L. (1991). Social perception and social reality: A reflection-construction model. *Psychological Review*, 98, 54-73.
- Kahneman, D. (1973). *Attention and effort*. Englewood Cliffs, NJ: Prentice-Hall.
- Kanouse, D. E., & Hanson, R. (1971). Negativity in evaluations. In E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 47-62). Morristown, NJ: General Learning Press.
- Kant, I. (1965). *Critique of pure reason*. (N. K. Smith, Trans.). New York: St. Martin's. (Original work published 1781)
- Kelley, H. H. (1950). The warm-cold variable in first impressions of persons. *Journal of Personality*, 18, 431-439.
- Kelley, H. H. (1967). Attribution theory in social psychology. In D. Levine (Ed.), *Nebraska Symposium on Motivation* (Vol. 15, pp. 192-238). Lincoln: University of Nebraska Press.
- Kelley, H. H. (1971a). Attribution in social interaction. In E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 1-26). Morristown, NJ: General Learning Press.
- Kelley, H. H. (1971b). Causal schemata and the attribution process. In E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 151-174). Morristown, NJ: General Learning Press.
- Kelley, H. H., & Michela, J. L. (1980). Attribution theory and research. *Annual Review of Psychology*, 31, 457-501.
- Kenny, D. A. (1994). *Interpersonal perception: A social relations analysis*. New York: Guilford.
- Kenny, D. A., & Albright, L. (1987). Accuracy in interpersonal perception: A social relations analysis. *Psychological Bulletin*, 102, 390-402.
- Kenny, D. A., Kieffer, S. C., Smith, J. A., Ceplenski, D., & Kuklo, J. (1996). Circumscribed accuracy among well-acquainted individuals. *Journal of Experimental Social Psychology*, 32, 1-12.
- Kihlstrom, J. F. (1987). The cognitive unconscious. *Science*, 237, 1445-1452.
- Kintsch, W., & van Dijk, T. A. (1978). Toward a model of text comprehension and production. *Psychological Review*, 82, 363-394.
- Klein, S. B., & Loftus, J. (1993). The mental representation of trait and autobiographical knowledge about the self. In T. K. Srull and R. S. Wyer (Eds.), *Advances in social cognition* (Vol. 5, pp. 1-50). Hillsdale, NJ: Erlbaum.
- Koehler, J. J. (1996). The base rate fallacy reconsidered: Normative, descriptive, and methodological challenges. *Behavioral and Brain Sciences*, 19, pp. 1-53.
- Kruglanski, A. W. (1975). The endogenous-exogenous partition in attribution theory. *Psychology Review*, 82, 387-406.
- Kruglanski, A. W. (1989). The psychology of being "right": The problem of accuracy in social perception and cognition. *Psychological Bulletin*, 106, 395-409.
- Kruglanski, A. W., & Webster, D. M. (in press). Motivated closing of the mind: "Seizing" and "freezing." *Psychological Review*.
- Krull, D. S. (1993). Does the grist change the mill? The effect of the perceiver's inferential goal on the process of social inference. *Personality and Social Psychology Bulletin*, 19, 340-348.
- Krull, D. S., & Dill, J. C. (in press). On thinking first and responding fast: Flexibility in social inferences processes. *Personality and Social Psychology Bulletin*.
- Krull, D. S., & Erickson, D. J. (1995). On judging situations: The effortful process of taking dispositional information into account. *Social Cognition*, 13, 417-438.
- Krull, D. S., Loy, M. H.-M., Lin, J., Wang, C.-F., Chen, S., & Zhao, X. (1996). *The fundamental attribution error: Correspondence bias in independent and interdependent cultures*. Paper presented at the 13th Congress of the International Association for Cross-Cultural Psychology, Montreal, Quebec.
- Kunda, A., & Thagard, P. (1996). Forming impressions from stereotypes, traits, and behaviors: A parallel-constraint-satisfaction theory. *Psychological Review*, 103, 284-308.
- LaBerge, E. J., & Samuels, S. J. (1974). Toward a theory of automatic information processing in reading. *Cognitive Psychology*, 6, 293-323.
- Landman, J., & Manis, M. (1983). Social cognition: Some his-

- torical and theoretical perspectives. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 16, pp. 49–123). New York: Academic Press.
- Langer, E. J. (1978). Rethinking the role of thought in social interaction. In J. H. Harvey, W. J. Ickes, & R. F. Kidd (Eds.), *New directions in attribution research* (Vol. 2, pp. 35–58). Hillsdale, NJ: Erlbaum.
- Langfeld, H. S. (1918). The judgment of emotion from facial expressions. *Journal of Abnormal and Social Psychology*, 13, 172–184.
- Larrick, R. P., Morgan, J. N., & Nisbett, R. E. (1990). Teaching the use of cost-benefit reasoning in everyday life. *Psychological Science*, 1, 362–370.
- Lee, G., Hallahan, M., & Herzog, T. (1996). Explaining real-life events: How culture and domain shape attributions. *Personality and Social Psychology Bulletin*, 22, 732–741.
- Lerner, M. J. (1980). *The belief in a just world: A fundamental delusion*. New York: Plenum.
- Locke, J. (1690/1959). *An essay concerning human understanding*. (Vol. 2, A. C. Fraser, Ed.). New York: Dover.
- Lorge, I. (1936). Prestige, suggestion, and attitudes. *Journal of Social Psychology*, 7, 386–402.
- Lycan, W. G. (1989). Ideas of representation. In D. Weissbord (Ed.), *Mind, value, and culture: Essays in honor of E. M. Adams* (pp. 207–228). Atascadero, CA: Ridgeview.
- Manktelow, K. I., & Over, D. E. (Eds.). (1993). *Rationality: Psychological and philosophical perspectives*. London: Routledge.
- Markus, H. R., & Kitayama, S. (1991). Culture and the self: Implications for cognition, emotion, and motivation. *Psychological Review*, 98, 224–253.
- Markus, H., & Zajonc, R. B. (1985). The cognitive perspective in social psychology. In G. Lindzey & E. Aronson (Eds.), *The handbook of social psychology*, (3rd ed., Vol. 1, pp. 137–230). Reading, MA: Addison-Wesley.
- McArthur, L. Z. (1972). The how and what of why: Some determinants and consequences of causal attribution. *Journal of Personality and Social Psychology*, 22, 171–193.
- McArthur, L. Z., & Baron, R. M. (1983). Toward an ecological theory of social perception. *Psychological Review*, 90, 215–238.
- McClelland, J. L., & Rumelhart, D. E. (Eds.). (1986). *Parallel distributed processing: Explorations in the microstructure of cognition* (Vol. 2). Cambridge, MA: MIT Press.
- McGill, A. L. (1989). Context effects in judgments of causation. *Journal of Personality and Social Psychology*, 57, 189–200.
- Medcof, J. W. (1990). PEAT: An integrative model of attribution processes. In M. P. Zanna (Ed.), *Advances in experimental social psychology*, (Vol. 23, pp. 111–209). New York: Academic Press.
- Michotte, A. (1963). *The perception of causality*. New York: Basic Books.
- Mill, J. S. (1843/1986). *A system of logic*. Charlottesville, VA: Ibis.
- Miller, A. G., Jones, E. E., & Hinkle, S. (1981). A robust attribution error in the personality domain. *Journal of Experimental Social Psychology*, 17, 587–600.
- Miller, D. T., Norman, S. A., & Wright, E. (1978). Distortion in person perception as a consequence of the need for effective control. *Journal of Personality and Social Psychology*, 36, 598–607.
- Miller, D. T., & Ross, M. (1975). Self-serving biases in attribution of causality: Fact or fiction? *Psychological Bulletin*, 82, 213–225.
- Miller, G. A., Galanter, E., & Pribram, K. (1960). *Plans and the structure of behavior*. New York: Holt, Rinehart, & Winston.
- Miller, J. G. (1984). Culture and the development of everyday social explanation. *Journal of Personality and Social Psychology*, 46, 961–978.
- Mischel, W. (1968). *Personality and assessment*. New York: Wiley.
- Mook, D. G. (1983). In defense of external invalidity. *American Psychologist*, 38, 379–387.
- Morris, M. W., & Peng, K. (1994). Culture and cause: American and Chinese attributions for social and physical events. *Journal of Personality and Social Psychology*, 67, 949–971.
- Neisser, U. (1967). *Cognitive psychology*. New York: Appleton-Century-Crofts.
- Newell, A., & Simon, H. (1972). *Human problem solving*. Englewood Cliffs, NJ: Prentice-Hall.
- Newman, L. S. (1993). How individualists interpret behavior: Idiocentrism and spontaneous trait inference. *Social Cognition*, 11, 243–269.
- Newman, L. S., & Uleman, J. S. (1990). Assimilation and contrast effects in spontaneous trait inference. *Personality and Social Psychology Bulletin*, 16, 224–240.
- Newman, L. S., & Uleman, J. S. (1993). When are you what you did? Behavior identification and dispositional inference in person memory, attribution, and social judgment. *Personality and Social Psychology Bulletin*, 19, 513–525.
- Newton, D. (1973). Attribution and the unit of perception of ongoing behavior. *Journal of Personality and Social Psychology*, 28, 28–38.
- Newton, D., Hairfield, J., Bloomingdale, J., & Cutino, S. (1987). The structure of action and interaction. *Social Cognition*, 5, 191–237.
- Nisbett, R. E. (1987). Lay personality theory: Its nature, origin, and utility. In N. E. Grunberg, R. E. Nisbett, J. Rodin, & J. E. Singer (Eds.), *A distinctive approach to psychological research: The influence of Stanley Schachter* (pp. 87–117). Hillsdale, NJ: Erlbaum.
- Nisbett, R. E., & Ross, L. (1980). *Human inference: Strategies and shortcomings of social judgment*. Englewood Cliffs, NJ: Prentice-Hall.
- Norman, D. A., & Bobrow, D. G. (1975). On data-limited and

- resource-limited processes. *Cognitive Psychology*, 7, 44-64.
- Osgood, C., Suci, G. J., & Tannenbaum, P. H. (1957). *The measurement of meaning*. Urbana: University of Illinois Press.
- Ostrom, T. M. (1984). The sovereignty of social cognition. In R. S. Wyer & T. K. Srull (Eds.), *Handbook of social cognition* (Vol. 1, pp. 1-38). Hillsdale, NJ: Erlbaum.
- Parducci, A. (1965). Category judgments: A range-frequency model. *Psychological Review*, 72, 407-418.
- Paunonen, S. V. (1991). On the accuracy of ratings of personality by strangers. *Journal of Personality and Social Psychology*, 56, 493-505.
- Peabody, D., & Goldberg, L. R. (1989). Some determinants of factor structures from personality trait descriptors. *Journal of Personality and Social Psychology*, 57, 552-567.
- Pearson, P. D., Barr, R., Kamil, M. L., & Mosenthal, P. (Eds.). (1984). *Handbook of reading research*. New York: Longman.
- Petty, R. E., & Wegener, D. T. (1998). Attitude change: Multiple roles for persuasion variables. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 1, pp. 323-390). New York: McGraw-Hill.
- Pierce, C. S. (1878). How to make our ideas clear. *Popular Science Monthly*, 12, 286-302.
- Posner, M. I., & Snyder, C. R. R. (1975). Attention and cognitive control. In R. L. Solso (Ed.), *Information processing and cognition: The Loyola symposium* (pp. 55-85). Hillsdale, NJ: Erlbaum.
- Postman, L., Bruner, J., & McGinnies, E. (1948). Personal values as selective factors in perception. *Journal of Abnormal and Social Psychology*, 43, 142-154.
- Pryor, J. B., & Kriss, M. (1977). The cognitive dynamics of salience in the attribution process. *Journal of Personality and Social Psychology*, 35, 49-55.
- Quattrone, G. A. (1982). Overattribution and unit formation: When behavior engulfs the person. *Journal of Personality and Social Psychology*, 42, 593-607.
- Ramsey, W., Stich, S. P., & Garon, J. (1991). Connectionism, eliminativism, and the future of folk psychology. In W. Ramsey, S. P. Stich, & D. E. Rumelhart (Eds.), *Philosophy and connectionist theory* (pp. 199-228). Hillsdale, NJ: Erlbaum.
- Rayner, K., & Pollatsek, A. (1989). *The psychology of reading*. Englewood Cliffs, NJ: Prentice-Hall.
- Read, S. J. (1987). Constructing causal scenarios: A knowledge-structure approach to causal reasoning. *Journal of Personality and Social Psychology*, 52, 288-302.
- Read, S. J., & Marcus-Newhall, A. (1993). Explanatory coherence in social explanations: A parallel distributed processing account. *Journal of Personality and Social Psychology*, 65, 429-447.
- Reeder, G. D. (1982). Let's give the fundamental attribution error another chance. *Journal of Personality and Social Psychology*, 43, 341-344.
- Reeder, G. D., & Brewer, M. B. (1979). A schematic model of dispositional attribution in interpersonal perception. *Psychological Review*, 86, 61-79.
- Reid, T. (1764/1983). *An inquiry into the human mind on the principles of common sense*. (R. E. Beanblossom & K. Lehrer, Eds.). Indianapolis: Hackett.
- Reid, T. (1785/1983). *Essays on the intellectual powers of man*. (R. E. Beanblossom & K. Lehrer, Eds.). Indianapolis: Hackett.
- Rholes, W. S., Newman, L., & Ruble, D. (1990). Developmental and motivational aspects of perceiving persons in terms of invariant dispositions. In E. T. Higgins & R. M. Sorrentino (Eds.), *Handbook of motivation and cognition* (Vol. 2, pp. 369-407). New York: Guilford.
- Rosenberg, S., & Sedlak, A. (1972). Structural representations of implicit personality theory. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 6, pp. 235-297). New York: Academic Press.
- Ross, L. (1977). The intuitive psychologist and his shortcomings. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 10, pp. 173-220). New York: Academic Press.
- Ross, L., Amabile, T. M., & Steinmetz, J. L. (1977). Social roles, social control, and biases in social-perception processes. *Journal of Personality and Social Psychology*, 35, 485-494.
- Ross, L., Lepper, M. R., & Hubbard, M. (1975). Perseverance in self-perception and social perception: Biased attribution processes in the debriefing paradigm. *Journal of Personality and Social Psychology*, 32, 880-892.
- Ross, M., & Fletcher, G. J. O. (1985). Attribution and social perception. In G. Lindzey & E. Aronson (Eds.), *The handbook of social psychology*, (3rd ed., Vol. 2, pp. 73-122). Reading, MA: Addison-Wesley.
- Ross, M., & Sicoly, F. (1979). Egocentric biases in availability and attribution. *Journal of Personality and Social Psychology*, 37, 322-336.
- Rudmin, F., Trimpop, R. M., Kryl, I. P., & Boski, P. (1987). Gustav Ichheiser in the history of social psychology: An early phenomenology of social attribution. *British Journal of Social Psychology*, 26, 165-180.
- Rumelhart, D. E., & McClelland, J. L. (Eds.). (1986). *Parallel distributed processing: Explorations in the microstructure of cognition* (Vol. 1). Cambridge, MA: MIT Press.
- Rumelhart, D. E., & Norman, D. A. (1985). Representation of knowledge. In A. M. Aitkenhead & J. M. Slack (Eds.), *Issues in cognitive modeling* (pp. 15-62). Hillsdale, NJ: Erlbaum.
- Ryle, G. (1949). *The concept of mind*. New York: Penguin.
- Sampson, E. E. (1991). *Social worlds, personal lives*. New York: Harcourt Brace Jovanovich.

- Schachter, S., & Singer, J. (1962). Cognitive, social, and physiological determinants of emotional state. *Psychological Review*, 69, 379-399.
- Schlenker, B. R. (1974). Social psychology and science. *Journal of Personality and Social Psychology*, 29, 1-15.
- Schneider, D. J. (1973). Implicit personality theory: A review. *Psychological Bulletin*, 79, 294-309.
- Schneider, W., & Shiffrin, R. M. (1977). Controlled and automatic human information processing: Detection, search, and attention. *Psychological Review*, 84, 1-66.
- Schwarz, N., Groves, R. M., & Schuman, H. (1998). Survey methods. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 1, pp. 143-179). New York: McGraw-Hill.
- Shank, R., & Abelson, R. P. (1977). *Scripts, plans, goals, and understanding*. Hillsdale, NJ: Erlbaum.
- Sherif, M., & Hovland, C. W. (1961). *Social judgment: Assimilation and contrast effects in communication and attitude change*. New Haven, CT: Yale University Press.
- Sherman, J. W., & Klein, S. B. (1994). Development and representation of personality impressions. *Journal of Personality and Social Psychology*, 67, 972-983.
- Sherman, S. J. (1980). On the self-erasing nature of errors of prediction. *Journal of Personality and Social Psychology*, 39, 211-221.
- Sherman, S. J., Judd, C. M., & Park, B. (1989). Social cognition. *Annual review of psychology*, 40, 281-326.
- Shultz, T. R., & Lepper, M. R. (1996). Cognitive dissonance reduction as constraint satisfaction. *Psychological Review*, 103, 219-240.
- Skowronski, J. J., & Carlston, D. E. (1989). Negativity and extremity biases in impression formation: A review of explanations. *Psychological Bulletin*, 105, 131-142.
- Smith, E. R. (in press). What do connectionism and social psychology offer each other? *Journal of Personality and Social Psychology*.
- Smith, E. R. (1994). Social cognition contributions to attribution theory and research. In Devine, P. G., Hamilton, D. L., & Ostrom, T. M. (Eds.), *Social cognition: Impact on social psychology* (pp. 77-108). New York: Academic Press.
- Smith, E. R. (1998). Mental representation and memory. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed. Vol. 1, pp. 391-445). New York: McGraw-Hill.
- Smith, E. R., & Miller, F. D. (1979). Attributional information processing: A response time measure of causal subtraction. *Journal of Personality and Social Psychology*, 37, 1723-1731.
- Smith, E. R., & Miller, F. D. (1983). Mediation among attributional inferences and comprehension processes: Initial findings and a general method. *Journal of Personality and Social Psychology*, 44, 492-505.
- Smith, E. R., & Zarate, M. (1992). Exemplar-based model of social judgment. *Psychological Review*, 99, 3-21.
- Smith, P. B., & Bond, M. H. (1994). *Social psychology across cultures: Analysis and perspectives*. Needham Heights, MA: Allyn-Bacon.
- Snyder, M. L., & Frankel, A. (1976). Observer bias: A stringent test of behavior engulfing the field. *Journal of Personality and Social Psychology*, 34, 857-864.
- Solomon, R. C., & Higgins, K. M. (1996). *A short history of philosophy*. Oxford: Oxford University Press.
- Solomon, S. (1978). Measuring dispositional and situational attributions. *Personality and Social Psychology Bulletin*, 4, 589-594.
- Srull, T. K., & Wyer, R. S. (1979). The role of category accessibility in the interpretation of information about persons: Some determinants and implications. *Journal of Personality and Social Psychology*, 37, 1660-1672.
- Srull, T. K., & Wyer, R. S. (1986). The role of chronic and temporary goals in social information processing. In R. M. Sorrentino & E. T. Higgins (Eds.), *Handbook of motivation and cognition: Foundations of social behavior* (pp. 503-549). New York: Guilford.
- Srull, T. K., & Wyer, R. S. (1989). Person memory and judgment. *Psychological Review*, 96, 58-83.
- Stich, S. P. (1983). *From folk psychology to cognitive science: The case against belief*. Cambridge, MA: MIT Press.
- Storms, M. D. (1973). Videotape and the attribution process: Reversing actors' and observers' points of view. *Journal of Personality and Social Psychology*, 27, 165-175.
- Swann, W. B., Jr. (1984). Quest for accuracy in person perception: A matter of pragmatics. *Psychological Review*, 91, 457-477.
- Tagiuri, R. (1967). Person perception. In G. Lindzey & E. Aronson (Eds.), *The Handbook of Social Psychology* (2nd ed., Vol. 3, pp. 395-449). Reading, MA: Addison-Wesley.
- Tagiuri, R., & Petrillo, L. (Eds.). (1958). *Person perception and interpersonal behavior*. Stanford, CA: Stanford University Press.
- Taylor, S. E., & Brown, J. D. (1988). Illusion and well-being: A social-psychological perspective on mental health. *Psychological Bulletin*, 103, 193-210.
- Taylor, S. E., & Crocker, J. (1981). Schematic bases of social information processing. In E. T. Higgins, C. P. Herman, & M. P. Zanna (Eds.), *Social cognition: The Ontario symposium* (Vol. 1, pp. 89-134). Hillsdale, NJ: Erlbaum.
- Taylor, S. E., & Fiske, S. T. (1975). Point-of-view and perceptions of causality. *Journal of Personality and Social Psychology*, 32, 439-445.
- Taylor, S. E., & Fiske, S. T. (1978). Salience, attention, and attribution: Top of the head phenomena. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 11, pp. 249-288). New York: Academic Press.
- Tetlock, P. E., & Levi, A. (1982). Attribution bias: On the inconclusiveness of the cognition-motivation debate. *Journal of Experimental Social Psychology*, 18, 68-88.

- Toyama, M. (1990). *The role of causal inference in attitude attribution*. Paper presented at the International Congress of Applied Psychology, Kyoto, Japan.
- Trope, Y. (1986). Identification and inferential processes in dispositional attribution. *Psychological Review*, 93, 239-257.
- Trope, Y. (1989). Levels of inference in dispositional judgment. *Social Cognition*, 7, 296-314.
- Trope, Y., & Bassock, M. (1982). Confirmatory and diagnosing strategies in social information gathering. *Journal of Personality and Social Psychology*, 43, 22-34.
- Trope, Y., & Cohen, O. (1989). Perceptual and inferential determinants of behavior-correspondent attributions. *Journal of Experimental Social Psychology*, 25, 142-158.
- Trope, Y., Cohen, O., & Maoz, Y. (1988). The perceptual and inferential effects of situational inducements on dispositional attributions. *Journal of Personality and Social Psychology*, 55, 165-177.
- Tulving, E., & Thomson, D. M. (1973). Encoding specificity and retrieval processes in episodic memory. *Psychological Review*, 80, 352-373.
- Tversky, A., & Kahneman, D. (1974). Judgment under uncertainty: Heuristics and biases. *Science*, 185, 1124-1131.
- Tversky, A., & Kahneman, D. (1980). Causal schemas in judgment under uncertainty. In M. Fishbein (Ed.), *Progress in social psychology* (Vol. 1, pp. 49-72). Hillsdale, NJ: Erlbaum.
- Uleman, J. S., & Moskowitz, G. B. (1994). Unintended effects of goals on unintended inferences. *Journal of Personality and Social Psychology*, 66, 490-501.
- Uleman, J. S., Moskowitz, G. B., Roman, R. J., & Rhee, E. (1993). Tacit, manifest, and intentional reference: How spontaneous trait inferences refer to persons. *Social Cognition*, 11, 321-351.
- Uleman, J. S., Newman, L. S., & Moskowitz, G. B. (1996). People as flexible interpreters: Evidence and issues from spontaneous trait inference. In M. P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 29, pp. 211-279). San Diego: Academic Press.
- Uleman, J. S., Newman, L. S., & Winter, L. (1992). Can personality traits be inferred automatically? Spontaneous inferences require cognitive capacity at encoding. *Consciousness and Cognition*, 1, 77-90.
- Vallacher, R. R., & Wegner, D. M. (1985). *A theory of action identification*. Hillsdale, NJ: Erlbaum.
- Vorauer, J. D., & Ross, M. (1993). Making mountains out of molehills: An informational goals analysis of self- and social perception. *Personality and Social Psychology Bulletin*, 19, 620-632.
- Watson, D. (1989). Strangers' ratings of five robust personality factors: Evidence of a surprising convergence with self-reports. *Journal of Personality and Social Psychology*, 57, 120-128.
- Weber, M. (1930). *The Protestant ethic and the spirit of capitalism*. London: Allen and Unwin.
- Webster, D. M. (1993). Motivated augmentation and reduction of the overattribution bias. *Journal of Personality and Social Psychology*, 65, 261-271.
- Wegner, D. M., & Bargh, J. A. (1998). Control and automaticity in social life. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 1, pp. 446-496). New York: McGraw-Hill.
- Wegner, D. M., & Vallacher, R. R. (1987). The trouble with action. *Social Cognition*, 5, 179-190.
- Weiner, B., Frieze, I., Kukla, A., Reed, L., Rest, S., & Rosenbaum, R. M. (1971). Perceiving the causes of success and failure. In E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 95-120). Morristown, NJ: General Learning Press.
- Wells, H. G. (1932). *The work, wealth, and happiness of mankind*. London: William Heinemann.
- White, P. A. (1991). Ambiguity in the internal/external distinction in causal attribution. *Journal of Experimental Social Psychology*, 27, 259-270.
- Whitney, P., Davis, P. A., & Waring, D. A. (1994). Task effects on trait inference: Distinguishing categorization from characterization. *Social Cognition*, 12, 19-35.
- Whitney, P., Waring, D. A., & Zingmark, B. (1992). Task effects on the spontaneous activation of trait concepts. *Social Cognition*, 10, 377-396.
- Williams, J. E., Munick, M. C., Saiz, J. L., & FormyDuval, D. L. (1995). Psychological importance of the "Big Five": Impression formation and context effects. *Personality and Social Psychology Bulletin*, 21, 818-826.
- Wilson, T. D., & Brekke, N. (1994). Mental contamination and mental correction: Unwanted influences on judgments and evaluations. *Psychological Bulletin*, 116, 117-142.
- Winter, L., & Uleman, J. S. (1984). When are social judgments made? Evidence for the spontaneousness of trait inferences. *Journal of Personality and Social Psychology*, 47, 237-252.
- Winter, L., Uleman, J. S., & Cuniff, C. (1985). How automatic are social judgments? *Journal of Personality and Social Psychology*, 49, 904-917.
- Wishner, J. (1960). Reanalysis of "Impressions of personality." *Psychological Review*, 67, 96-112.
- Wyer, R. S., & Carlston, D. E. (1979). *Social cognition, inference, and attribution*. Hillsdale, NJ: Erlbaum.
- Wyer, R. S., & Gruenfeld, D. H. (1995). Information processing in social contexts: Implications for social memory and judgment. In M. P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 27, pp. 52-93). New York: Academic Press.
- Wyer, R. S., & Lambert, A. J. (1994). The role of trait constructs in person perception: An historical perspective. In Devine, P. G., Hamilton, D. L., & Ostrom, T. M. (Eds.), *Social cognition: Impact on social psychology* (pp. 109-142). New York: Academic Press.

- Wyer, R. S., & Srull, T. K. (1986). Human cognition in its social context. *Psychological Review*, 93, 322-359.
- Zajonc, R. B. (1980a). Cognition and social cognition: A historical perspective. In L. Festinger (Ed.), *Retrospections on social psychology* (pp. 180-204). New York: Oxford University Press.
- Zajonc, R. B. (1980b). Thinking and feeling: Preferences need no inferences. *American Psychologist*, 35, 151-175.
- Zajonc, R. B. (1998). Emotions. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The handbook of social psychology* (4th ed., Vol. 1, pp. 591-632). New York: McGraw-Hill.
- Zuckerman, M. (1989). *Inferences and explanations in attribution theory*. University of Rochester, unpublished manuscript.