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# Speeding With Ned: A Personal View of the Correspondence Bias

Daniel T. Gilbert

Ned Jones was a brilliant psychologist, a wonderful human being, and a terrible driver. I discovered the last of these facts in 1982 when I was in graduate school and Ned was my advisor. One Saturday he had to make a long trip to some remote section of New Jersey and he invited me to keep him company on the drive. "We'll talk about your first-year project," he said. "Bring some jazz tapes." I didn't know any better. I thought he was serious. So I actually brought along the notes for my first-year project. I also brought along a tape of my hero, Keith Jarrett, whom Ned immediately dismissed as "too baroque." We quietly deposed my hero in favor of Ned's—the sublime Bill Evans—and listened to his music for the duration of the drive. At some point early in that drive I noticed two things. First, I noticed that Bill Evans was indeed a more subtle and articulate pianist than Keith Jarrett. Second, I noticed that we were traveling at about 90 miles per hour. Ned

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must have seen me glancing nervously at the speedometer because he proudly announced that if he got another speeding ticket, he, his wife, and his insurance carrier were all going to part ways. At which point he nudged the car to 95.

Over the next decade of our friendship I came to learn that speeding was almost as much of a passion for Ned as was Bill Evans. Everywhere Ned drove, he drove too fast. But speeding, I came to learn, was not simply about going fast. Rather, it was a competitive sport in which a player attempted to move across a paved field as rapidly as possible without being detected by the opposing team, which, on this particular spring day, was the entire New Jersey State Troopers Association. Speeding is not about cars or roads. It is not really even about speed. It is about outsmarting other people who just happen to be police.

We talked. We sped. I think "Stella by Starlight" had just come on when I first spotted the police cruiser on the shoulder just a half mile ahead, idling happily beneath an antler of whirling, colored lights, a small foreign car captive before it. One moment Ned and I were chatting about this and that, listening to music, adjusting our visors against the afternoon sun, banking and listing into a curve, and the next moment our conversation abruptly ceased and our eyes were riveted to the scene at the shoulder. And as quickly as those flashing lights appeared in our field of vision, two things happened, one of them entirely predictable and one of them entirely unexpected. Every car on the turnpike predictably slowed down, and Ned unexpectedly sped up. Perhaps it was only in contrast to the suddenly legal crawl of nearby traffic, but I felt certain that we passed the cruiser at 100.

"That's Gary Peacock on drums," Ned said, scarcely nodding in time to the music. Could he have missed the one-act drama a quarter mile—now a half mile, now more—behind us? Of course not. He was just waiting for me to say something—waiting for me to offer him my incredulity like some exotic disease for which only he had the cure. I would come to learn in the following years that Ned taught his students in the same way that Bill Evans (never Bill, never just Evans) played his piano. Between the clever remark and the penetrating question were languid patches of silence that seemed casually, almost disinterestedly



placed, but that were in fact carefully measured and impeccably timed. Conversation with Ned was a dance, a game, a jam.

"You went by that cop awfully fast," I said.

"Yep," Ned replied like a rimshot. More silence as he stared straight ahead. Perhaps there was another verse before the chorus.

"I can't believe you didn't slow down."

Ned smiled. "That's what everybody does. And it doesn't make sense." Ah. Here it was. The tempo changed, the dialogue hit the major seventh, and the melodic hook emerged from behind the drummer's fill. "The gut instinct, Danny Boy," (never Dan, never just Gilbert) "is to slow down when you see a cop giving someone a ticket. But cops aren't randomly distributed on a highway. They space themselves to cover the most territory, which means that when you see a cop giving a ticket you can be pretty sure that you have spotted the only cop on your particular section of highway, and you can be sure he's too busy to spot you back. So the odds of getting stopped just now were actually lower than they were earlier. I adjusted my speed accordingly." Ned looked satisfied. He stared out the driver's side window for a moment, and I let the pause linger. "People slow down as a reflex. They realize it's illogical, but they can't help it. They do it anyway." He shook his head. "It never ceases to amaze me."

We went on to talk about other things that afternoon—the Oakland Raiders ("No finesse"), *JPSP* ("too baroque"), and how to distinguish between good and bad gin ("One word, Danny Boy: Tanqueray"). And although I did not realize this at the time, we had also talked about my first-year project, because the thing that never ceased to amaze Ned had begun to amaze me a little bit too.

## DRIVING THROUGH JONESLAND

Somewhere between the time that Ned became seriously devoted to Bill Evans and the time he became seriously devoted to speeding, he noticed something that never ceased to amaze him. In 1965, Ned and his student Keith Davis had tried to describe how ordinary people use behavior to infer the personal dispositions of the actor. Their theory was

smart, well articulated, and not particularly counterintuitive. For example, it told us that if an actor does one thing rather than another, observers are likely to conclude that she desired the state of affairs to which her action uniquely led. It told us that when an actor's behavior is directly relevant to us as observers, we will be especially motivated to understand what makes him tick. It told us that when we are in the business of understanding what motivates people, their accidents, parapraxes, and other unintended actions don't really count. It told us that when people are forced to act as they do, we don't conclude that they were somehow predisposed toward that action. And so on. The theory was not meant to amaze. It was meant to formalize the rules that could enable the ordinary, mundane, and generally unamazing business of everyday person perception.

## Ned Charts a Course

Although the theory's predictions were clearly too obvious to warrant a test, Ned liked to do experiments. So in 1967, he and Victor Harris tested the theoretically derived hypothesis that people will not draw dispositional inferences from actions that are mandated by the social situation—in other words, when people are in the business of understanding others, their constrained behaviors won't really count. Participants in their experiment read essays that supported or opposed Fidel Castro's regime, and some participants were told that the essayist had freely chosen to write the essay while others were told that the essayist had been ordered to write the essay by a debate coach. All participants were asked to estimate just how much the essayist personally supported or opposed Castro, and the timid prediction was that participants would draw *correspondent inferences* (i.e., they would conclude that the essayist's verbal behavior corresponded to the essayist's personal attitudes) when the essayist had chosen to write the essay, but not when the essayist had been forced to do so. The prediction was timid, the results were a foregone conclusion, and the experiment didn't work. Although participants made correspondent inferences about essayists whose behavior was freely enacted, they also made correspondent inferences (albeit much weaker ones) about essayists whose behavior was situationally constrained. Participants knew perfectly well that the essay's content had been dictated by the debate coach, and yet something about the actor's behavior elicited correspondent inferences. Rather than being annoyed by this experimental anomaly, Ned celebrated it as the *observer bias,* which he later renamed the *correspondence bias* (see Gilbert & Malone, 1995, for a review). Most of us know it by another name too. As Ned wrote (Jones, 1988), "This bias has also been referred to by Lee Ross as the fundamental attribution error, but I find that designation overly provocative and somewhat misleading. Furthermore, I'm angry that I didn't think of it first."

This small difference between two means did not intrigue Ned because it taught him something new; rather, it intrigued him because it seemed to be an example of something he had thought about for a long time, namely, that in everyday life people seem all too willing to take each other at face value and all too reluctant to search for alternative explanations for each other's behavior. Ned's early work on ingratiation (Jones, 1964) had explored how people convince others to like them, and he was fascinated by how easily and routinely this was accomplished. Why, he wondered, do we believe those who tell us that we have a wonderful child, a stylish haircut, or a winning personality when (a) it is so cheap and so tempting for others to say such things even when they don't mean them and (b) we tell fibs like this all the time ourselves? Magicians surely do not fall for the same sleights of hand they perpetrate, so why are people duped by the very ingratiation tactics they employ? Shouldn't observers chalk compliments up to the requirements of polite society and reserve judgment about what the flatterer really believes? It seemed to Ned that there was something about observing behavior that virtually compelled the observer to make correspondent inferences about the actor's personality—despite the observer's knowledge that such inferences might not be logically warranted—and he wondered what that something was.

Over the next 15 years or so, Ned's wondering led to a score of experiments in which he and his collaborators toyed with the effect that he and Harris had found (see Jones, 1979, 1990, for reviews). Such toying typically took two forms. The first involved challenging the ef-

fect—creating circumstances under which it would be likely to disappear and then showing that, in fact, it didn't. So, for example, he and Art Miller and Steve Hinkle showed that the effect occurred even when participants had the experience of being an arm-twisted essayist prior to reading the arm-twisted essay of another (Miller, Jones, & Hinkle, 1981; see also Jones & Harris, 1967, Experiment 2). He and I showed that it occurred even when participants did the arm twisting themselves (Gilbert & Jones, 1986). And so on. The second form of play involved ruling out local explanations of what Ned believed to be a ubiquitous phenomenon. So, for example, he and Mel Snyder showed that the experimenter-generated essays that Jones and Harris had used were not necessary to produce correspondence bias, which occurred even when the essays were written by ordinary college students (Snyder & Jones, 1974). He and I showed that the bias did not require that participants change their own attitudes or presume that the essayists' attitudes had been changed by the essay-writing task (Gilbert & Jones, 1986). In short, Ned's empirical work on correspondence bias was largely directed toward demonstrating the robustness and generality of the effect.

Ned was a great believer in the pedagogical value of mistakes, and he liked to point out when others were making them. But he also encouraged his students to point out to him his own, and he listened graciously and agreed occasionally. It is an homage to him that we can, in hindsight, say what was wrong with this work as well as what was so very right with it. The first problem is that, in the long run, the work was paradigm bound. Ned's beloved attitude-attribution paradigm was a fine way to bottle the phenomenon, and he chose to demonstrate the generality of the correspondence bias by ruling out a variety of alternative explanations within this paradigm rather than by demonstrating the same effect in a variety of other paradigms. Ultimately this may have been a tactical error because Ned became embroiled in a host of minor debates about whether this or that aspect of the experimental paradigm was a prerequisite for the effect. Indeed, a quick glance at the literature shows that most of the relevant research of the day was about the Jones and Harris experiment and was not about the correspondence bias itself. In some ways, Ned became too busy outmaneuvering critics



to explore the more far-reaching implications of his amazing phenomenon. He should have been the one who taught us why the correspondence bias was such a vitally important effect, but he wasn't. The person who did that was Lee Ross (1977), who had the decided advantage of not being mired in the wet cement of the attitude-attribution paradigm and who was thus free to think more broadly about the phenomenon. The somewhat ironic denouement is that now, nearly 30 years later, most researchers probably agree that (a) the correspondence bias is a fairly robust and general phenomenon, and (b) the attitude-attribution paradigm is not the best way to bottle it.

The second mistake was not so much an error of commission as an error of omission. Ned showed us that the phenomenon was real, that it didn't go away quietly, and that its persistence resisted most artifactual explanations, but he did not tell us why it occurred in the first place. Of course, hindsight is dangerously sharp and we do well to remember that yesterday is not today. Ned's was not the era of process models, and the geist of his zeit called for social psychologists to invent same-level alternatives to reexplain results (e.g., the dissonance versus self-perception debate) rather than to invent lower-level models that predict them. Jones and Harris approvingly quoted Heider's (1958, p. 54) well-known maxim that behavior "tends to engulf the total field," and correctly noted that "this describes the results without really explaining them" (Jones & Harris, 1967, p. 22). Nonetheless, Ned did not explain the results himself, and privately he continued to bet on Heider's unit-formation notion as the one with the most staying power. Until the early 1980s, Ned seemed to feel that attempts to explain correspondence bias were premature, and that in the early stages of a relationship with an amazing thing one should simply play with it—tweak it, twist it, and see what pops out. This view may or may not be right, but surely it is not difficult to understand why Ned held it. After all, the amazing thing had itself just popped out of his attempt to demonstrate something much less amazing, so why shouldn't he expect that some lovely, parsimonious explanation would ultimately present itself if he kept his heart open and his eyes peeled?

As usual, Ned was right.

## George Takes the Wheel

One Friday morning in 1982, Ned and I were speeding to Trenton to meet the departmental colloquium speaker, Bob Abelson, whose train from New Haven arrived at noon. Soon the three of us had retired for lunch to a chummy, neighborhood Italian restaurant that Ned told us was renowned for its stunning version of olives in Tanqueray sauce. As we waited for our drinks, Bob said, "I read George Quattrone's article in *JPSP* last month. A beautiful piece of work."

Ned nodded in agreement, glanced over to make sure I was paying attention, and without hesitation said, "Yep, he's the smartest student I've ever had."

Whether or not George was the smartest of Ned's students, he was certainly the person who changed the direction of Ned's work and moved him beyond demonstrations of robustness and generality to descriptions of the psychological mechanisms that actually produce the bias. Social psychology was at the zenith of its love affair with cognitive psychology (after the honeymoon but before the bickering), and Dick Nisbett and Lee Ross had just immortalized that relationship in their seminal book on human inference (Nisbett & Ross, 1980). One of the book's many contributions was that it brought the decade-old work of Amos Tversky and Danny Kahneman to the attention of mainstream social psychologists. That work suggested that ordinary people solve a large number of inferential problems by using a small number of judgmental heuristics, one of which they dubbed the *anchor–adjust heuristic* (see Tversky & Kahneman, 1974).

To understand Quattrone's contribution, one must understand the state of the art at the time. What did attribution theories of the early 1980s have to say about the psychological processes that enabled attributional judgments? Not much. Next to nothing, in fact. With a few exceptions, attribution theories simply articulated the formal inferential rules that a thinking system might use to move from observations of action to inferences about the underlying qualities of the actor. As Ned and his student Daniel McGillis had noted in 1976, attribution theories were rational baseline theories and were not intended as descriptions



of the mental work that real, meaty, sweaty people did when they made inferences about each other. As geometry was to architecture, so attribution theories were to person perception. Experiments revealed that people's attributional inferences looked very much like the attributional inferences that a thinking system would generate if it was relying on formal attributional rules such as the calculus of noncommon effects, the covariation and discounting principles, and so on. But no one knew whether people were actually using those rules, and if they were, certainly no one knew how. In general, study participants seemed to understand when behavior should be taken as a fair indicator of the actor's personal dispositions and when it should be ascribed to the pressures of the social situation. The variables that should have affected judgments made by formal rules (e.g., the consistency of the behavior over time, the number of noncommon effects) seemed to affect the judgments made by ordinary people. But whether ordinary people were using those formal rules—and if so, how they were doing it—remained a mystery.

Quattrone set out to solve this mystery by borrowing an idea from Tversky and Kahneman, who had suggested that people do not typically wait until they have considered all the evidence before they generate an inference, but instead, they roughly estimate a problem's solution and then smooth out that rough estimate as they consider each new piece of information in turn. This heuristic displays nature's genius for compromise because it allows people to move toward accurate solutions while having somewhat less accurate solutions in hand as they do so, which is quite useful when one is carefully considering whether to wander away from the campfire and is suddenly interrupted by a bear. Quattrone was intrigued by this description of the process by which complex problems were solved, and he wondered whether people might solve "the attributional problem" in the same way. Whereas correspondence-inference theory suggested that people make correspondent inferences under some circumstances (e.g., when behavior is freely authored) and not under others (e.g., when behavior is constrained), Quattrone (1982) suggested that people actually make

correspondence inferences under both circumstances and that they then go on to adjust those inferences as they consider the possibility that the behavior was situationally constrained.

Quattrone reasoned that if people do indeed anchor on dispositions and then adjust for situations, then perhaps under other circumstances they could be encouraged to anchor on situations and adjust for dispositions. In other words, perhaps people could begin the attributional task by first using behavior to estimate the situational pressures that were impinging on the actor, and then continue by adjusting those estimates as they considered the actor's dispositions. In his 1982 paper, Quattrone turned the well-worn attitude-attribution paradigm inside out with two moves. First, instead of telling participants that there had or had not been strong situational pressure on the essayist, he told participants that the essayist was already known to have a pro- or anti-something attitude. Second, instead of asking participants to estimate the strength of the essayist's dispositions, he asked them to estimate the strength of the situational pressures that had impinged on the essayist. In other words, rather than manipulating what participants knew about the situational pressures and then measuring what they thought about the essayist's dispositions, he manipulated what they knew about the essayist's dispositions and measured what they thought about the situational pressures. He did this by suggesting to participants that although the essays had been freely rendered by persons who were known to support the positions they advocated, there was reason to suspect that the person who commissioned the essays had applied subtle, nonverbal pressure on the essayists to defend the commissioner's own personal position. Participants were asked to read the essay and to estimate the commissioner's (and not the essayist's) position on the issue. And in this topsy-turvy version of the attitude-attribution paradigm participants did a topsy-turvy thing: They concluded that the essay reflected the commissioner's personal position even when they knew full well that the essayist's attitude was congruent with the position advocated in the essay. In other words, participants believed that the situation (the commissioner's subtle pressure) had caused the actor's behavior even though they knew that the actor was predisposed toward it.



## Hey, Dad, Can I Drive?

It was 1984, and Ned and I were speeding through Toronto. Something about a rented car always made Ned drive faster than usual—as if some part of him believed that police officers the world over were on the lookout for his sleek, black Pontiac Bonneville 1000, and that behind the wheel of a rented rosy red Ford Fiesta he was invisible. He was wearing his trademark driving cap—the one that made him look like Mr. Toad from *The Wind in the Willows*—and when he finally became convinced that the radio would harvest nothing but "rock noise," he switched it off and began to talk to me about the correspondence bias. "It would be wonderful to show that you can really reverse the effect," he said. "George did it, but he cheated." Given Ned's dim view of cheaters and his high regard for George, I asked him to clarify. "What I mean is that he got people to attribute the essay to the situation, but in his case 'the situation' was another person's dispositions. His subjects did, in fact, attribute the behavior to dispositions—it was just the wrong person's dispositions. Somehow that doesn't seem to me like really *reversing* the correspondence bias." As the years passed, I would come to understand that Ned enjoyed pondering the mystery of the correspondence bias like he enjoyed guessing the killer's identity in a good whodunit, and as with an engrossing novel, part of him did not want the story to end. Right up until his death, he resisted the idea that anyone had actually achieved The Complete Solution.

If his resistance was understandable, his enthusiasm was contagious. His lifelong quest to understand the correspondence bias seemed to me thoroughly heroic, and as a graduate student, I never had the slightest doubt that joining that crusade was the most noble destiny a young soldier could hope for. I spent several years reading and thinking about the work that Ned, George and others had done, and in the end I found myself convinced of two things: First, I was convinced that Quattrone's theory was essentially right, and second, I was convinced that his experiment did little to show just how right his theory was. What was wrong with that experiment? In my view, the experiment was not flawed (as Ned thought) so much as irrelevant to Quattrone's beautiful idea. Quattrone had invented circumstances under which people would use

human behavior to generate inferences about the situations in which that behavior occurred. This seems remarkable if you have your head stuck inside the attitude-attribution paradigm because, on the face of it, such a finding seems to violate the maxim that people always use human behavior to generate inferences about the actor's dispositions. But who ever believed such a maxim in the first place? Indeed, social psychologists had known for quite some time that people draw situational inferences from behavior. Darley and Latane (1968), for example, had shown that bystanders use the behavior of other bystanders to determine the situational requirements of an emergency—and decades before that, Sherif (1935) had shown that people use the behavior of others to determine the physical nature of the stimuli to which they are responding. The fact that people look to the behavior of others to tell them what is happening in the world around them had been explored and exploited by everyone from Solomon Asch to Alan Funt. To my mind, it looked as though Quattrone had given participants the same essays that Ned had always used and had then given them a different task to perform (i.e., "I'll tell you about dispositions and you tell me about situations" instead of the other way around). So it did not strike me as particularly remarkable that under such circumstances subjects would . . . well, perform a different task. Moreover, I could not see (a) how participants' behavior in George's experiment demonstrated that they were anchoring and adjusting in either George's or Ned's experiments, and (b) if they were, why they were adjusting insufficiently. As far as I could see, George had demonstrated that people were capable of doing something other than drawing correspondent inferences about essayists and that they did this other something when you asked them to.

Now, the joy of having a brilliant mentor who had brilliant students before you is that you don't have to be very smart yourself to find an interesting problem and a partially assembled solution. Really, all you need is a little patience and a library card. I had both, and by 1988 my students and I had used them to develop a new theory and some new experiments that I thought would be congenial with both Ned's and George's thinking, but that might also provide The Complete Solution



we had all lusted after in our hearts. Our theorizing comprised three arguments.[1] First, we argued that George was basically right—people begin the attributional task by assuming a correspondence between the actor's behavior and one of the attributional elements, which may be either the actor's dispositions (as in Ned's studies) or the actor's situation (as in George's study). One might additionally suppose that people are inclined to make this assumption about the element they most wish to understand, and that they will most wish to understand the element that they don't already understand or the element that they have been explicitly instructed to understand. Second, we argued that people may subsequently repudiate this assumption of correspondence as they consider the element that they were not trying to understand. Third, we argued that the initial assumption of correspondence constituted a qualitatively different kind of mental activity than the subsequent repudiation of that assumption. Specifically, we suggested that the first process (which we called *characterization*) was more automatic or less effortful than the second process (which, in an alliterative frenzy, we called *correction*).

Taken together, these three axioms solve several otherwise puzzling problems. For example, why had subjects tended to anchor on dispositions in Ned's experiments? Because in Ned's experiments the experimenter typically told participants all about the situation into which the essayist had been thrust, and typically instructed participants to diagnose the essayist's dispositions. If people do indeed characterize the element that they are most eager to understand and about which they already know the least, then the experimental instructions and the experimental task provide a rather compelling explanation for participants' choice of anchors in the attitude-attribution paradigm. In everyday life, I supposed, people are sometimes like Ned's participants in that they are interested in knowing about the enduring properties of

[1] Ned once told me that readers are not interested in the writer's intellectual odyssey and that they just want to know what the writer believes and not how he or she came to believe it. To that end, I present my current view of these issues rather than an accurate historical account of how I achieved it. This will spare the reader several pages of truly dull history, and it will spare me having to recall all those embarassing mistakes.

the actor, and they are sometimes like George's participants in that they are interested in knowing about the vagaries of the situation. These motivations must surely combine with what the person already knows to determine whether the person will characterize dispositions or situations—in other words, to determine what task the person will, in a sense, instruct herself to perform. The question of why participants tended to anchor on dispositions seemed to me rather easily answered by our theory.

The more puzzling question, to my mind, was why adjustment in either case should necessarily be insufficient. Tversky and Kahneman (1974) said that adjustment was often insufficient, but they hadn't said why, and I saw no a priori reason to assume that just because people sometimes start by assuming a correspondence between dispositions and actions that they should somehow be expected to finish with that assumption intact. People change their minds all the time, and if insufficient adjustment was to explain the correspondence bias, then something had to explain insufficient adjustment. For me, that something was the relative automaticity of the characterization (anchoring) and correction (adjustment) processes. One of the hallmarks of an effortless or automatic process is that its execution is *robust*—that is, it tends not to be impaired or truncated by the simultaneous performance of other tasks. (Most of us can recite the alphabet, but not the Fibonocci series, while sorting laundry.) My students and I reasoned that if characterization was indeed a heartier, less fragile, less effortful operation than correction, then across all experimental instances one would expect it to fail less often. In fact, we assumed that if people *did* characterize and then correct, and if characterization *was* less effortful than correction, then we should be able to impair the correction process by putting people under cognitive load (i.e., by requiring them to perform an attributional task and some other task concurrently). By our reckoning, such a manipulation should exacerbate the correspondence bias when people are characterizing dispositions (i.e., when they know about the actor's situation and are instructed to diagnose the actor's dispositions, as they did in Ned's studies) and should have the opposite effect when they are characterizing situations (i.e., when they know about the actor's



dispositions and are instructed to diagnose the situation, as they did in George's studies).

Armed with a new theory, Brett Pelham, Doug Krull, and I set out to demonstrate these effects (Gilbert, Pelham, & Krull, 1988). We borrowed and modified Mel Snyder and Art Frankel's (1976) anxious-woman paradigm in which participants are shown a silent videotape of an anxious-looking woman who was ostensibly discussing with an off-camera stranger either a series of anxiety-provoking topics (such as her sexual fantasies) or a series of mundane topics (such as gardening). Our participants were instructed to diagnose the woman's dispositional anxiety and, under normal conditions, they did just what any reasonable attributer would do: They concluded that a woman who looked nervous while discussing sex was not nearly so anxious a person as was a woman who looks equally nervous while discussing gardening. In other words, one must be pretty high strung to wig over rutabagas. Another group of participants watched the same videotape while simultaneously attempting to memorize the topics that the anxious woman was discussing. We assumed that such participants would rehearse the topics ("Rutabagas, rutabagas, rutabagas") and that doing so would impose a cognitive load that would keep them from correcting their initial characterizations of the woman. In fact, loaded participants attributed the same amount of dispositional anxiety to the woman in both the anxious-topic and mundane-topic conditions—as if they had been unable to consider the attributional implications of the very information they were rehearsing. This study suggested to us that when people know about the actor's situation and are instructed to diagnose the actor's dispositions, they do indeed characterize before they correct, and that this situational correction is more easily impaired than is the dispositional characterization that precedes it.

With this encouraging result in hand, we set out to see whether George's effect behaved the same way Ned's effect did. In a follow-up to his dissertation (Krull, 1993), Doug Krull and Darin Erikson turned the anxious-woman paradigm inside-out, just as George had done with the attitude-attribution paradigm (Krull & Erikson, 1993, 1995). Their participants watched the anxious-woman videotape and were told that

the woman was known either to be dispositionally anxious or dispositionally calm, and they were asked to determine which of several topics she was likely to be discussing. Under normal conditions, participants concluded that the dispositionally calm woman must be discussing juicier topics than was the dispositionally anxious woman. In other words, it takes more than rutabagas to fluster a mellow soul. But loaded participants concluded that both the dispositionally anxious and the dispositionally calm women were discussing equally juicy topics—as if they had assumed a correspondence between actor's behavior and the actor's situation, and had subsequently questioned that assumption only when they had the cognitive resources to do so.

In short, it seemed to us that we had The Complete Solution on paper, some good evidence in the bag, and that all we had to do now was auction the movie rights and hope that Richard Gere agreed to play several of us. In fact, it turned out that we had achieved a solution, but not The Complete Solution. In fact, it turned out that there was not and never had been a complete solution to be achieved.

## ON BEYOND JONESLAND

I've told the story of the correspondence bias not from Ned's perspective (he did that quite nicely in his 1990 book) but rather, from his passenger's seat. Even so, that story is still too Jones-centric to be the whole story. Ned would have been the first to acknowledge that his was only part of a bigger picture, and he would have enjoyed having his students find out what that bigger picture was and then having them tell him about it.

### Dear Ned

I haven't spoken with you since we hooked up in Chicago the month before you died. But I've thought many times about that last evening we spent together, about that wonderful dinner we had with our friends, how you tried to get me to eat thymus glands, and how you ran up the tab with all that wine and then hid in the men's room until I paid. And then, a month later, poof, you were gone. I always suspected you



would check out when it was your turn to buy dinner. But this is not a dunning letter. Rather, I'm writing to share with you some thoughts about one of our mutual interests—the correspondence bias. I'm going to marinate you in a reasonable amount of chatter, so let me telegraph the punchline: There is no cause of correspondence bias. Now, now—let me drop the second shoe: There is no cause of correspondence bias because there are, in fact, four different kinds of correspondence biases, each with its own unique cause. The reason why people in the field don't agree about causes is that we are not, by and large, studying the same thing. Let me see if I can convince you.

#### *First, People May Be Unaware of Situational Constraints*

Think about what it would take for someone to avoid making correspondence-biased inferences. Surely the first thing they'd have to do is realize that situational forces are causing an actor's behavior, and they can only do that when they're aware that such forces exist in the first place. If they don't even know that a hostage is being threatened, a senator is being bribed, or a basketball player is being hindered, then they can't possibly do the inferential work that making an accurate attribution requires. My reading of the literature suggests that there are two discrete problems—the *invisibility problem* and the *construal problem*—that make it particularly difficult for observers to attain this basic information.

You often pointed out that the word *situation* typically refers to things that have little or no physical presence: One can't see, smell, taste, or hear audience pressure or social norms. If one can't see the situation then one may not know about the situation, and in that case one can't possibly take the situation into account when making an attribution. The best illustration of this kind of correspondence bias is the quizmaster study (Ross, Amabile, & Steinmetz, 1977). Participants were arbitrarily assigned to play the roles of contestant or quizmaster in a mock game-show. Quizmasters were allowed to generate a list of questions from their private store of arcane knowledge and, as expected, contestants typically failed to answer the questions. So what did observers conclude? They concluded that the quizmasters were genuinely brighter

than the contestants. And why not? Observers couldn't hear or see a role-conferred advantage like they could hear and see a "dumb answer" and a "smart question." They surely realized that the quizmaster had the good fortune of asking all the questions and that the contestant had the bad fortune of having to stumble over them, but they probably did not consider the fact that even the dimmest bulb can come up with a handful of idiosyncratic tidbits that others are unlikely to carry around with them (e.g., "How many albums did Bill Evans release in 1964?"). They could have considered that fact, but they probably didn't. So really, I'm only saying what Heider was saying when he talked about the relative salience of behavior and situations: Situations are often invisible and you can't consider the effect of something if you don't even know it's there.

But there's more to say about this first cause because there is a more subtle version of the invisibility problem that I'll call *the construal problem*. Ever since Heider (1958) first compared human behavior to the physical motion of a boat on a lake, most of us have fallen into the flabby habit of talking about situational constraints as though that term described one thing. But, as you and I have agreed on many occasions (most of them speeding on the Garden State Parkway), there are two very different kinds of situational constraints. *Behavioral constraints* directly constrain an actor's behavior and are entirely independent of the actor's understanding of them. For example, the contestants in the quizmaster experiment had no choice but to give incorrect answers on many trials. Regardless of what they may have felt, wanted, thought, hoped, wished, or believed, the objective difficulty of the quizmasters' questions guaranteed that they would perform poorly. *Psychological constraints*, on the other hand, don't change an actor's behavior by changing her behavioral options so much as they change it by changing her understanding of those options. The constraint imposed by a debate coach's instructions, for instance, is quite different than the constraint imposed by a role-conferred advantage because, unlike a role-conferred advantage, instructions don't literally force the essayist's hand or make an anti-Castro speech impossible to write. Instead, a debate coach's instructions alter the payoffs associated with the behavioral options. When a



debate coach assigns a debater to defend Castro, then saying "Yes, sir" and writing a pro-Castro speech is suddenly an easier, healthier alternative than is saying "Buzz off" and writing an anti-Castro speech, but the essayist is still technically free to do either. This is America, after all. Psychological constraints don't change behavioral options—they change the actor's motivation to enact the behavioral options.

Now here's why this distinction matters. When constraints are psychological, then the observer doesn't need to be aware of the actor's situation as it is objectively constituted, but rather, she needs to be aware of the situation as it is subjectively construed. Even if the observer can hear the debate coach's instructions, the critical question is whether the debater can hear them, and, if so, whether he hears them the same way the observer does. I mean, imagine that the coach asks for a pro-Castro essay, that the debater is a bit deaf and mistakenly believes the coach asked for an anti-Castro essay, but that the debater decides to write a pro-Castro essay anyway. He's just got to be pro-Castro, right? Surely a dispositional inference is warranted, even though the behavior is exactly what the situation-as-it-truly-was demanded because it is exactly the opposite of what the situation-as-the-essayist-sees-it demanded. Okay. So here's my point: If you think people have trouble recognizing the situation-as-it-is (the invisibility problem), then just imagine how much trouble they have recognizing the situation-as-the-actor-sees-it (the construal problem). In fact, people often adopt an egocentric point of view and assume that the situation they see is the situation that the actor sees too (Griffin & Ross, 1991; Keysar, 1994), perhaps because it is just so difficult to imagine what the situation would look like to someone who had different information about it than they do (Fischoff, 1975; Jacoby, Kelley, & Dywan, 1989).

Well, I've gone on quite a bit about this, and I really didn't mean to. It is painfully obvious that one must be aware of situational constraints if one is to avoid the correspondence bias, and it is equally obvious that attaining such awareness can at times be difficult (the invisibility problem), and at other times, very difficult (the construal problem). The first kind of correspondence bias, then, is caused by simple ignorance.

### *Second, People May Have Unrealistic Expectations for Behavior*

Even if one hears the debate coach's instructions (no invisibility problem here) and understands the essayist's take on those instructions (no construal problem either), one must still have an idea of how a debate coach's instructions generally affect a debater's essays. Are most debaters so intimidated by their coaches that they obey their every command, or do debaters tend to take such instructions as mild suggestions and ignore them whenever they please? People make dispositional inferences when the actor's behavior violates their normative expectations, so if we have unrealistic expectations about how situations normally affect behaviors (e.g., "A true American would never write a pro-Castro speech"), then those expectations are going to be violated when they shouldn't be. So how realistic are these normative expectations? Put another way: How accurately do we estimate the power of particular situations to evoke particular behaviors? Put yet another way: How accurately do we predict how the average person will behave?

Figuring out how powerful a situation is can be tricky, and people use some ingenious but fallible methods. For example, when we try to estimate a situation's power by imagining how the average person would behave, we may assume they would behave as we assume we would behave. (There's that egocentric assumption again.) That's what happened in that false consensus study in which an experimenter asked participants whether they would march around wearing a signboard that read "Eat at Joe's," and found that both consenters and refusers considered their own choices to be typical of the population (Ross, Greene, & House, 1977). Refusers personally experienced the experimenter's request as a weak situational force; they refused, they expected others to refuse, and they drew dispositional inferences about people who complied. Ditto for consenters. The problem, of course, is that other people don't always act as we think we would act. Sometimes we are unique, or at least in the minority. Furthermore, *we* don't always act as we think we would act. Jim Sherman (1980) asked college students to predict whether they would comply with an experimenter's request to write a counterattitudinal essay, and nearly three quarters said they wouldn't. You know as well as I do that in decades of cognitive



dissonance studies, college students almost *never* refuse the experimenter's request. Now imagine that Sherman's participants were taking part in the attitude-attribution paradigm. They mispredict their own behavior ("I'd say no"), which leads to unrealistic expectations about how others will behave ("Most anyone would say no"), which leads to having those expectations violated ("That guy said yes"), which leads to—*voila*!—the correspondence bias.

### *Third, People May Misidentify Behavior*

You'd think that the bias would evaporate when people are perfectly aware of the actor's situation and have perfectly realistic expectations for the actor's behavior in that situation. And you'd be wrong. In fact, rather than providing protection against correspondence bias, a perfect awareness of situational constraints can actually cause it.

As I argued above, people make dispositional inferences when an actor's behavior exceeds their expectations. Technically, of course, people don't compare their expectations to the actor's actual behavior, but rather, to their perceptions of that behavior, and perception ain't reality. Just as the sentence "I'm having a friend for dinner" means one thing when uttered by Martha Stewart and another when uttered by the head of the Donner party, so a mother's tears may appear more wrenching when shed at her daughter's funeral than at her daughter's wedding. In other words, behaviors can be ambiguous, and Yaacov Trope and his colleagues (Trope, 1986; Trope & Cohen, 1989; Trope, Cohen, & Maoz, 1988) have suggested that our perfect awareness of a situation can cause us to have an imperfect understanding of the ambiguous behaviors that unfold within it. It's interesting to imagine just how this can happen. For example, if a situational force (a debate coach's instructions) actually induces a certain kind of behavior (a pro-Castro speech), then the observer who is aware of the situation and who has a realistic estimate of its power should expect precisely that sort of behavior. However, the very awareness that enables the observer to have a realistic expectation for behavior may also cause the observer to have an unrealistic perception of behavior; in this case, the behavior may be seen as conforming more to situational demands than it actually does. The observer may be prepared to hear a pro-Castro speech, but that very

expectation may cause her to hear an incredibly pro-Castro speech. As the model I sketched earlier suggests, that observer would then be struck by the mismatch between her expectations and her perception of "reality," and will draw a dispositional inference about the essayist. The irony, of course, is that the observer's perfect knowledge of the situation has befuddled her observation of the actor's behavior, which leads her to make an unwarranted dispositional inference about an actor whose situation she understands perfectly well, but whose behavior she has misconstrued.

What's more, this actually happens. Consider that experiment in which participants watched a silent film of a woman who was ostensibly being interviewed about politics or sex (Snyder & Frankel, 1976). Some were told about the interview topic prior to seeing the film and some were only told afterwards. When participants learned about the interview topic only after seeing the film, they took the woman's situation into account and concluded that she was less dispositionally anxious when discussing sex than when discussing politics. But those who learned about the interview topic before seeing the film drew precisely the opposite conclusion. Apparently, participants who knew that the woman was talking about sex expected her to be anxious, and they then went on to see more fidgeting and shuffling in her ambiguous behavior than was actually present. So even though these participants knew all about the woman's situation, knew all about the situation's power, and took all of this into account when making attributions, they were damned from the get go. They couldn't possibly make the right attribution because they were making attributions for the wrong behavior. The third type of correspondence bias is caused by misidentification of the actor's behavior.

### *Fourth, People May Not Correct Their Dispositional Inferences*

Here's the point where you expect me to go on forever, and I'm going to surprise you. We both know what the fourth cause is because it's the one that you and George and I have been contemplating for a combined total of 60 years. In short, even if you know about situations and their power, and even if you have the behavior pegged correctly, there is still a tendency for those of us who are striving to understand dispositions



to assume a correspondence between the behavior we see and the dispositions we are striving to understand—and we seem to correct that assumption only subsequently and effortfully. If we can't exert the effort then we can't make the correction, and we end up stung by the correspondence bias. If you want to read more about this, you know where to look (Gilbert, 1989).

So that's what I've been thinking about. When we first met 15 years ago, you told me you wished someone would solve the problem of correspondence bias. It seems to me that you've gotten your wish. The problem has been more than solved—it's actually been solved four times. So why do I have a funny feeling that you aren't going to buy it? Maybe because this analysis is right but not elegant. It isn't $E = MC^2$. In fact, it is a little bit like a 600-page murder mystery in which it turns out that everybody killed the damned butler. But on the other hand, it's a reasonable framework that brings a certain degree of order to an otherwise untidy set of issues, and science is about getting it right, not making it pretty. Don't you think?

As always,

D'boy

## Much Ado About Everything

From Rousseau to Hobbes to Freud to Rogers, psychologists never seem to grow weary of talking about whether people are good or bad, smart or stupid, right or wrong. By focusing so intently on the causes of inferential error, the social psychology of the 1970s and 1980s gave rise to a minor but predictable backlash in which some critics argued that both the extent and the importance of inferential error had been vastly overplayed by laboratory science. Differing points of view often fuel discovery, but in this case, thesis and antithesis collided with a whimper instead of a bang. Rather than exploring together the meaning and consequences of inferential error, the two sides tacitly agreed to view each other as idiots. Critics often chose to vilify the workers rather than improve the work, and as a result, the criticized often dismissed the shrill chorus as incapable of serious dialogue. All of which was too bad,

because a thoughtful and friendly debate about the nature of error exercises all the important intellectual muscles. Is the correspondence bias an error? In raw form this is a lot like asking if chimps can think or if fetuses are people: The answers depend entirely on what one means by words such as *think* and *people* and *error*. Consider two ways of thinking about error.

### *Errors as Logical Violations*

If we take error to mean a violation of Aristotelian logic, or as a difference between one person's beliefs and the "objective reality" that most other sensible people subscribe to, then yes, the correspondence bias is an error. When actors are randomly assigned to be quizmasters or contestants, then the average intelligence of the actors should not differ by condition. And if observers on average believe otherwise, well then, observers on average are wrong. And we don't have to measure the intelligence of the actors to prove it. The interesting question is not whether, but why the observers are wrong. Are they wrong because the experimenter did not allow them to gather or use the information they needed to be right? If so, then their erroneous inferences merely provide support for the hypothesis that people make errors when they have been deprived of important information—not a terribly thrilling conclusion. Or are they wrong in spite of the fact that they had or had access to the information they needed, but did not recognize it as such, did not seek it, or did not use it? If so, then their erroneous inferences may tell a much more interesting story about people and how they think.

Often times when colleagues ask me if the correspondence bias is an error, they are really asking me if a particular experimenter gave a particular set of subjects a "fair chance" to do otherwise. Sometimes I say "yes" and sometimes I say "no" and never do I change anyone's mind because, as those who have pitched baseballs at milk bottles know, what the carney and the mark consider a fair chance are often worlds apart. *Fair chance* is another one of those terms that means what we want it to mean. Perhaps, then, we should not worry so much about whether participants were given a fair chance in a particular study, but rather, we should be concerned with how the chance they were given





by the experimenter compares with the chances they are ordinarily given by nature. My guess is that experimenters can be less fair than nature: For example, explicitly instructing participants to diagnose dispositions may initiate a chain of mental events that ends with inferential error when, in the real world, people might never have had such a goal in the first place. But I would guess that there are just as many instances in which nature is not nearly so fair as we are: For example, experimenters routinely tell participants about the situational constraints on an actor's behavior ("There was this debate coach, see, and . . ."), whereas in the real world, people must often dig for such information on their own.

Each of us can think of a dozen instances in which experimenters and nature are more or less fair than the other, and that sort of game could be played for years—and it should be, because it is interesting and important. But it is not new. It is the game that experimentalists have played ever since they decided to take behavior out the meadow and put it under the microscope. Anyone who believes that the world beneath the microscope is a perfect mirror of the world in the meadow ought to sit down and have a long, hard think. And anyone who believes the microscope can teach us nothing about the meadow should probably not even bother with that. Correspondence bias is a logical error, and the fact that people violate logic can be interesting and important in some cases and entirely trivial in others.

### *Errors as Trouble Makers*

There is another way to think about errors, and that is in terms of their consequences. The pragmatist philosophers (Dewey, 1908; James, 1907) were keen on this way of thinking and suggested that inferential errors are simply inferences that don't do the work that the inference-maker wanted done. If we take *error* to mean something that leads to dire consequences, then the correspondence bias is an error on some occasions, but not on others (Swann, 1984). It is clear how believing that a person is smart or honest when he's not might have unfortunate repercussions in marriage, business, or bowling, but perhaps it is not equally clear how such logically incorrect beliefs can end up having no repercussions at all. Consider two circumstances under which

correspondence-biased inferences will not be a problem for the observer, namely, the cases of *self-induced constraint* and *omnipresent constraint.*

First, many of the important situations that shape our lives are situations that we enter by choice or are drawn into by proclivity (Snyder & Ickes, 1985, p. 918). In other words, situational constraints are often self-induced, and such constraints often "push us" in the same direction as do our own dispositions. It will do the observer little harm—and even much good—to ignore the effects of self-induced constraints when making attributions. If, for example, a debater chose to serve a debate coach who was known to assign his debaters to defend the pro-Castro position, then an observer would be quite justified in ignoring the debate coach's instructions altogether and simply judging the debater on the basis of his essay. When people choose constraining situations, then those situations do not mask their dispositions so much as they provide evidence of them. A correspondence-biased inference, then, is not a trouble maker when constraints are self-induced. Second, situations may present omnipresent constraints—that is, the constraints on an actor's behavior may be enduring rather than temporary. This means that observers may never encounter circumstances under which their correspondence-biased inferences will be challenged. If an essayist were forced to write one pro-Castro essay every day for the rest of his natural life, we might be technically wrong to infer that he is personally pro-Castro, but that technically inaccurate inference would allow us to predict his future behavior quite perfectly. When the situational constraints on the behavior we observe are also the situational constraints on the behavior we wish to predict, then we might just as well ignore them. In addition, such enduring situations may create dispositions rather than merely causing behaviors (Higgins & Winter, 1993). It is not difficult to imagine that the poor debater who is doomed to write a pro-Castro essay every day for the rest of his life might eventually become pro-Castro, thus turning our inaccurate correspondence-biased inference into an accurate one. Once again, a correspondence-biased inference is not a troublemaker when constraints are omnipresent.

There is a useful way to think about the cases of self-induced con-





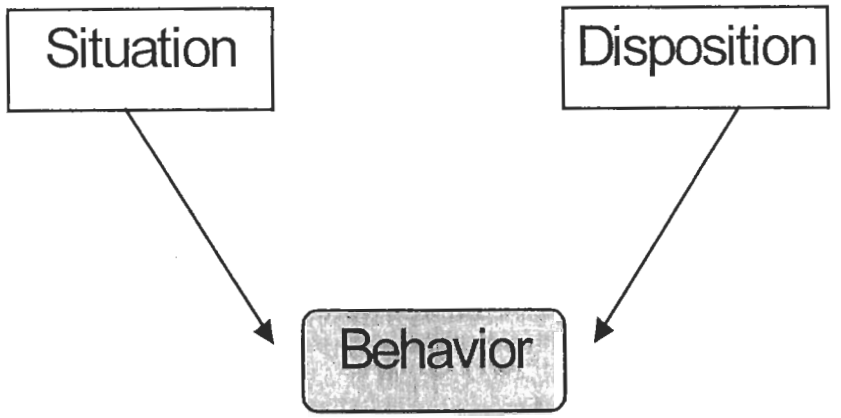


Figure 1

When discounting is a good idea.

straint and ominpresent constraint. Attribution theories tell us that if we wish to determine an actor's dispositions, we must first "subtract out" or "discount" or "remove" the effects that situations may be having on the actor's behavior. Under some circumstances this is sound advice. For example, when situations and dispositions are independent causes of behavior (see Figure 1), then one should indeed remove the effect of one cause in order to estimate properly the effect of the other. But dispositions and situations are not always independent. In fact, they often cause each other, and when this happens, then mentally removing the effect of the situation is precisely the *wrong* thing to do. As Figure 2 shows, the case of self-induced constraint is an instance in which a person's dispositions (Fred is authoritarian) exert a causal influence on the person's situation (Fred is in the military). Similarly, the case of omnipresent constraint is an instance in which a person's situation (Freda grew up in a military family) exerts a causal effect on the person's dispositions (Freda is authoritarian). In each of these cases, there is a strong correlation between the person's situation and the person's dispositions, which means that the situation does not hide the effects of dispositions so much as it provides evidence for them. In such instances, the effect of the situation should not be subtracted out of one's

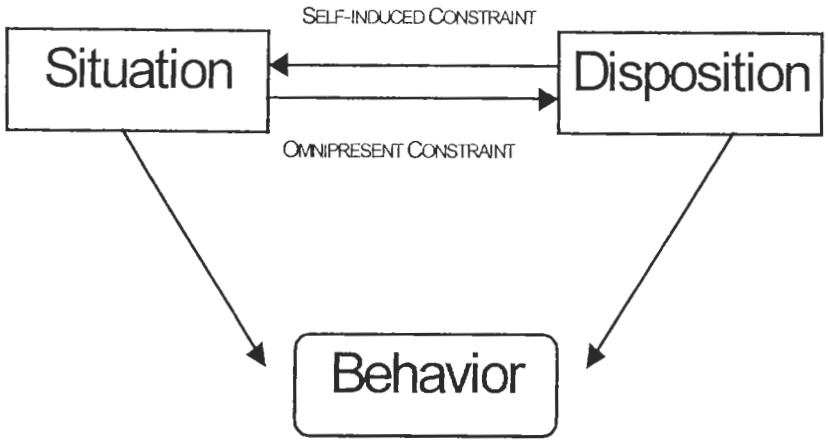


**Figure 2**

When discounting isn't such a good idea.

estimate of the actor's dispositions—rather, it should be added in! If, for example, the essayist chose to join a debate team whose coach was famous for assigning nothing but pro-Castro essays (self-induced constraint), or if the debater were forced to write such essays three times a day for the rest of his life (omnipresent constraint), then the debate coach's instructions would be good evidence of the essayist's pro-Castro attitude. The point here is simply this: Correspondence bias occurs when people fail to subtract out the effects of the situation; but there are instances in which that failure is a very good thing. The correspondence bias, then, is not always a troublemaker, and thus for the pragmatist, not always an error.

## CODA

Ned loved his codas. He was a careful writer and he used the coda as an opportunity to jump out of the bog and onto the lily—out of data and out of the detail and up to some higher place, at which he would pause, adjust his bow tie, clear his throat, and then tell us something true. Those truths were never shocking or outrageous or unbelievable. They were not revelations. Ned's truths were elegant, simple, astute



observations that were just so right—so perfectly on the money—that the moment you heard one you realized that you could have thought of it yourself , except that . . . well . . . you hadn't. When Ned gave you a birthday present it was never a card-shuffling machine or an electric corkscrew. It was a blue tie. Similarly, he did not give the world exotic intellectual gifts that will end up in the attic except when they are being trotted out for a history lesson. Instead, he gave us stuff we really needed. Ned was the master of telling us what we didn't quite know, and in a too short life, he hit that sweet note again and again and again.

There are people who change the world, there are people who don't, and then there are people like Ned who change it in such a way that you can hardly imagine what it would have been like without him. I hear people say about Aristotle, Einstein, and yes, Bill Evans, that their influence is so pervasive as to render them invisible. Ned Jones will be invisible soon too. His vision of social psychology so permeates our own that we can hardly tell we are looking through him anymore. Maybe that's inevitable and maybe that's good, but before he disappears altogether we should climb to a higher place, pause and clear our throats, and say this about him: If science is a race to get it right, then Ned drove us fast and far. We will all, for a long time to come, be speeding with Ned.

## REFERENCES

Darley, J. M., & Latane, B. (1968). Bystander intervention in emergencies: Diffusion of responsibility. *Journal of Personality and Social Psychology, 8,* 377–383.

Dewey, J. (1908). What does pragmatism mean by the practical? *Journal of Philosophy, Psychology, and Scientific Methods, 5,* 85–99.

Fischoff, B. (1975). Hindsight ≠ foresight: The effects of outcome knowledge on judgments under uncertainty. *Journal of Experimental Psychology: Human Perception and Performance, 1,* 288–299.

Gilbert, D. T. (1989). Thinking lightly about others: Automatic components of the social inference process. In J. S. Uleman & J. A. Bargh (Eds.), *Unintended thought* (pp. 189–211). New York: Guilford.

Gilbert, D. T., & Jones, E. E. (1986). Perceiver-induced constraint: Interpretations of self-generated reality. *Journal of Personality and Social Psychology, 50,* 269–280.

Gilbert, D. T., & Malone, P. S. (1995). The correspondence bias. *Psychological Bulletin, 117,* 21–38.

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.

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.

Heider, F. (1958). *The psychology of interpersonal relations.* New York: Wiley.

Higgins, E. T., & Winter, L. (1993). The "acquisition principle": How beliefs about a behavior's prolonged circumstances influence correspondent inference. *Personality and Social Psychology Bulletin, 19,* 605–619.

Jacoby, L. L., Kelley, C. M., & Dywan, J. (1989). Memory attributions. In H. L. Roediger & F. I. M. Craik (Eds.), *Varieties of memory and consciousness: Essays in honor of Endel Tulving* (pp. 391–422). Hillsdale, NJ: Erlbaum.

James, W. (1907). *Pragmatism.* New York: Longmans-Green.

Jones, E. E. (1964). *Ingratiation.* New York: Appleton-Century-Crofts.

Jones, E. E. (1979). The rocky road from acts to dispositions. *American Psychologist, 34,* 107–117.

Jones, E. E. (1988). *Attributional anomalies during social interaction.* Paper presented at the Katz-Newcomb Lecture, University of Michigan, Ann Arbor.

Jones, E. E. (1990). *Interpersonal perception.* New York: Macmillan.

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., & 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, & R. F. Kidd (Eds.), *New Directions in Attribution Research* (Vol. 1, pp. 389–420). Hillsdale, NJ: Erlbaum.



Keysar, B. (1994). The illusory transparency of intention: Perspective taking in text. *Cognitive Psychology, 26,* 165–208.

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., & Erikson, D. J. (1993). *How do people think dispositionally when they are situationally focused? The effortful processing of dispositional information.* Unpublished manuscript, University of Missouri.

Krull, D. S., & Erikson, D. J. (1995). On judging situations: The effortfull process of taking dispositional information into account. *Social Cognition, 13,* 417–438.

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.

Nisbett, R. E., & Ross, L. (1980). *Human inference: Strategies and shortcomings of social judgment.* Englewood Cliffs, NJ: Prentice-Hall.

Quattrone, G. A. (1982). Overattribution and unit formation: When behavior engulfs the person. *Journal of Personality and Social Psychology, 42,* 593–607.

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., Greene, D., & House, P. (1977). The "false consensus effect": An egocentric bias in social perception and attribution processes. *Journal of Experimental Social Psychology, 13,* 279–301.

Sherif, M. (1935). A study of some social factors in perception. *Archives of Psychology, 27,* 1–60.

Sherman, S. J. (1980). On the self-erasing nature of errors of prediction. *Journal of Personality and Social Psychology, 39,* 211–221.

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.

Snyder, M. L., & Ickes, W. (1985). Personality and social behavior. In G. Lindzey & E. Aronson (Eds.), *The Handbook of Social Psychology* (3rd ed., Vol. 2, pp. 883–947). New York: Random House.

Snyder, M. L., & Jones, E. E. (1974). Attitude attribution when behavior is constrained. *Journal of Experimental Social Psychology, 10,* 585–600.

Swann, W. B., Jr. (1984). Quest for accuracy in person perception: A matter of pragmatics. *Psychological Review, 91,* 457–477.

Trope, Y. (1986). Identification and inferential processes in dispositional attribution. *Psychological Review, 93,* 239–257.

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.

Tversky, A., & Kahneman, D. (1974). Judgments under uncertainty: Heuristics and biases. *Science, 185,* 1124–1131.



# Some Thoughts Prompted by "Speeding With Ned"

Arthur G. Miller

*It is quite clear that such bias is not always the effect of some unique cause: There is not a single, fundamental antecedent to go with the fundamental error.*

*—Jones, E. E. (1979).* The rocky road from acts to dispositions, *American Psychologist, 34,* p. 115.

*People who do crazy things are not necessarily crazy.*

—"Aronson's first law," in Aronson, E. (1995). *The Social Animal,* p. 9.

As I indicate in the quotation from the "Rocky Road" essay, (Jones, 1979), Ned anticipated that the correspondence bias would ultimately be understood in terms of more than one process or theoretical model. Thus, Gilbert's contribution in articulating four mechanisms, and showing how they embrace diverse instances of correspondence bias, is a most fitting capstone to Ned's prediction.[1]

[1] Being invited to participate as a discussant to Dan Gilbert's chapter in this volume is a very special honor for me. I count two individuals, both now sadly departed—Ned Jones and Stanley Milgram—as the most significant influences in my life as a social psychologist. I remember sending Ned, in the spring of 1986, a copy of a book I had written about the obedience experiments of Stanley

# *Attribution and Social Interaction*

## *The Legacy of Edward E. Jones*

*Edited by*

*John M. Darley*

*Joel Cooper*

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