

COMMENTARIES

Beyond the Fundamental Attribution Era?

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Since Jones and Harris (1967) first introduced it to the published world, the bias that Sabini, Siepmann, and Stein (this issue) refer to as the *fundamental attribution error (FAE)* has been the catalyst for a tremendous amount of research. Around the time of its 13th birthday (an age of particular significance in some cultures), Jones (1979), beaming like a proud father, characterized it as a "remarkably robust and easily replicated phenomenon" (p. 107). Almost a dozen years later, Jones (1990) called it "the most robust and ubiquitous finding in the domain of interpersonal perception" (p. 164). This important, influential concept is now in its mid-30s, and one might expect it to be cashing in its stock options and retiring to the south of France, taking the occasional *sommelier* class to keep itself entertained and making special guest appearances with cognitive dissonance theory on the celebrity lecture circuit.

But not unlike so many other similarly aged "celebrities," I fear, the FAE is not without its demons. Its discovery, if not its conception, was rather by accident, and it took a great deal of testing before many were convinced of its ability to survive long beyond its birth in the Jones and Harris laboratory. There has been almost unprecedented inconsistency in what to name it, enough to create anxiety and identity confusion in even the most robust adolescent. It took Jones about 20 years to settle on a name—the *correspondence bias* (Gilbert & Jones, 1986)—and by now it has been called, among other things, the *overattribution effect*, the *overattribution bias*, the *overattribution-to-persons tendency*, the *observer error*, the *observer bias*, *dispositionalism*, and, most dramatically, the *fundamental attribution error*.¹

More important, the cohesiveness of its identity has been rocked by the observation, made most articulately by Gilbert and Malone (1995), that there really are multiple correspondence biases, each with its own set

of causes, rather than just one all-encompassing FAE. Add to this identity confusion the questions of whether the bias occurs largely because of demand characteristics (e.g., Miller, Schmidt, Meyer, & Colella, 1984) and whether it is limited to the relatively small pockets of the world that are highly individualistic (e.g., Miller, 1984). And now Sabini et al. offer additional, important critiques of the way in which social psychologists have been conceptualizing this bias, suggesting that the really fundamental attribution error has been the field's own characterization of the bias and of the evidence supposedly supporting it. In light of these concerns, the picture that comes to mind now of this celebrated bias is less idyllic and more like one of those ubiquitous "behind-the-scenes" television biographies, depicting the loneliness, drug abuse, and myriad crises experienced by the rich and famous. If the correspondence bias is resting in the south of France, perhaps it is there to hide rather than to tan.

I believe that Sabini et al. raise some important concerns about cracks in the foundation that underlie the prototypical understanding of the correspondence bias. The work that my colleagues and I have done on the effects of suspicion on the correspondence bias complements some of these concerns. In the paragraphs that follow, I highlight some of these converging points and discuss some ways in which the target article may have gone too far in its critiques. Ultimately, I try to articulate some of the ways in which my research on suspicion and the challenges offered by the target article point to some additional, perhaps rocky, roads that future research should take in moving toward a more complete understanding of the correspondence bias.

The Internal-External Divide

Perhaps the most compelling argument that Sabini et al. make is that the distinction between internal causes and external causes has been a misleading one. As the authors note, the criticism of this distinction is

¹The term *correspondence bias* is used throughout the remainder of this article.

not new, but Sabini et al. make a particularly strong case in the target article for some of the problems with this distinction. In their example of whether someone ate some candy because he liked sweets or because the candy was sweet, they note the inherent interaction between the person and the situation, the interaction that Lewin (1935) famously championed many years ago and that so many of us emphasize to our students in our introduction to social psychology classes. But in those social psychology classes, I also emphasize two major points: (a) that so many of our thoughts, feelings, and behaviors are influenced by situational factors and (b) that people tend to fail to appreciate Point A. Because of the ambiguities of attributing causality to one or the other kind of factor, Sabini et al. suggest that this fundamental pair of points is fundamentally flawed.

Suspicion

Sabini et al. certainly are correct in noting some of the troubles with the internal-external distinction. Indeed, in some of my earliest work on suspicion as a graduate student, I recognized the awkwardness of asking participants to make this distinction and very quickly abandoned any thought of using dependent measures that required them to make this distinction. It clearly did not map onto their phenomenology, and as Sabini et al. observe, there often is no logical way to impose this distinction in interpreting perceivers' inferences. If perceivers believe that Darva's oath to love Rick until death do they part was influenced by her knowledge of Rick's status as a multimillionaire, does that reflect an external attribution? Many researchers in this area might be tempted to reach that conclusion, but Sabini et al.'s analysis suggests that it may reflect instead an attribution to Darva's internal disposition as someone who loves money.

In our work on suspicion, my colleagues and I found that contexts that raise the possibility of hidden motives underlying behavior, such as claiming love for a person when love for the person's resources is really driving the behavior, are unusually ripe for the kind of attributional thinking that helps perceivers avoid the correspondence bias. Much of this work has used the essay attribution paradigm that Jones and Harris (1967) used in the first demonstrations of the correspondence bias. Since then, this paradigm has produced numerous, and quite consistent, replications. We, too, have replicated the effect many times, but we have also found a consistent exception: When perceivers have reason to suspect that an author's decision to write the essay as he or she did may have been influenced by ulterior motives, they are much less likely to infer that the author's attitude necessarily corresponds to the opinion expressed in his or her essay (Fein, 1996; Fein, Hilton, & Miller, 1990; Hilton,

Miller, Fein, & Darley, 1990). Using this and other paradigms, we have found that contextual information implying the plausible presence of ulterior motives tends to trigger unusually sophisticated attributional thinking. In sharp contrast to the typical perceiver, suspicious perceivers, therefore, are much more likely to make inferences that reflect sufficient discounting of actors' behaviors in light of contextual information (Fein, 1996; Fein & Hilton, 1994; Fein, McCloskey, & Tomlinson, 1997; Fein, Morgan, Norton, & Sommers, 1997; Hilton, Fein, & Miller, 1993).

The power of suspicion to promote suspension of judgment and to ward off the correspondence bias may be particularly intriguing because the contextual information that arouses suspicion implies that the actor has more freedom to choose to act according to his or her disposition than does the contextual information in the high-constraint conditions that typically elicit inferences reflecting the correspondence bias. For example, participants in Fein et al. (1990, Study 1) who learned that an author wrote a persuasive essay under conditions of "no-choice" (the author was instructed what position to advocate) perceived the author's behavior to be more normative than did participants in the ulterior-motives conditions, and yet the former participants were more likely than the latter to infer that the author's true attitude corresponded to the opinion expressed in his essay.

I often have been asked if the reason suspicion has such strong effects relative to high constraint is that suspicion involves considering internal factors, such as the desire of the actor to ingratiate or self-promote. That is, whereas the alternative to a correspondent inference in a high-constraint context is purely an external factor (e.g., the strong demands of the experimenter or employer), the alternative in an ulterior-motive context is a motive internal to the actor. My response to this question is typically yes, that may be part of it, but it is important to recognize that in order for the ulterior motive to be considered, the perceivers must recognize and take into account the situational information that makes such a motive relevant. One would not suspect a motive of ingratiation without recognizing the reinforcement contingencies inherent to the situation that render ingratiation a plausible factor.

In a sense, this is the point that Sabini et al. make more generally about inferences and attributions: It may always be the case that both internal and external factors are implicated. Indeed, in the high-constraint context, there are alternative internal attributions to be made, such as to the author's motive to be a good subject or employee and to the motive to avoid the embarrassment of deviating from the script handed him or her. This distinction, therefore, cannot account for the greater discounting associated with suspicion. The more general point is that trying to separate the internal from the external may be akin to attributing someone's

vigorous applause more to one hand than to the other. Even if the actions of one hand seem more vivid than those of other, without the cooperation of the more recalcitrant hand, there would be a lot of strange waving but little applause.

The Ego-Syntonic-Ego-Dystonic Distinction

What, then, is left of the internal-external distinction? Sabini et al. offer their own conception of the distinction, focusing on the actor's values and beliefs. They propose that a "behavior is internally caused if and only if it follows from a person's values and (correct) beliefs.... The most important distinction... is one between causes of behavior people affirm as part of themselves and causes they reject, between *ego-syntonic* and *ego-dystonic* causes" (this issue). Although I appreciate the potential significance of this conception in reducing the ambiguity that perils other conceptions of internal-external causality, I am not satisfied with it, and that is not only because of my having to look up *ego-syntonic* and *ego-dystonic* in the dictionary. I see this distinction as fraught with its own fault lines of ambiguity. How are we to operationalize causes that affirm a part of the perceivers' selves versus those that don't? It may be easy to categorize participants' behavior in the Milgram (1963) paradigm as consistent or inconsistent with their consciences, but what of the more common situations consisting of more subtle social influence? And which follows more from a person's values, an honest expression of an attitude in the essay paradigm or compliance with an overworked and underpaid experimenter's request? I do not believe that the ego-syntonic-ego-dystonic distinction reduces any ambiguity here.

Moreover, how can this distinction work with ambivalent values? Which is more ego-syntonic, an aversive racist's anti-Black affect or his or her support of egalitarian values (cf. Dovidio & Gaertner, 1998)? Although we can recognize that the dieter who succumbs to eating ice cream is being driven by external forces because he rejects his cravings "as part of himself," what are we to make of the aversive racist's discomfort around African Americans? Is it caused by external factors because part of him would like to reject this part of himself, or is it internal because it reflects some of his core attitudes and values?

Covariation

Is the internal-external distinction without any utility, then? And is the introductory social psychology point about the underappreciated power of the situation truly a fundamental error? I am not ready to sen-

tence these old habits to death quite yet. Although perhaps not perfect, Kelley's (1967) covariation principle is a useful tool in understanding the distinction. In the various paradigms discussed in the target article, asking the question of whether the observed behavior seems to covary more with the person or with the situation seems a reasonable pursuit. In the Milgram (1963) paradigm, for example, although it is true that one way to explain the participants' behavior is to say that they acted according to their dispositional fear of embarrassment, it also is true that the observed behavior covaried with the situational variables more than with individual difference variables. In the essay attribution paradigm, the behavior covaries with the constraints imposed by the situation. In these cases, an attribution to the situation makes sense. Such an attribution would result in relatively accurate predictions.

Of course, this does not imply that the behavior of a participant in these situations could not have been caused by internal motives or values. One point of clarification may be to distinguish between causes of behavior and the logic of making attributions. Consider the example of Courtney writing a pro-Castro essay. Sabini et al. characterize the individual difference conception of the internal-external distinction (which is most relevant to the consensus component of the covariation principle) as suggesting that whether Courtney's behavior was internally or externally caused depends on what other people would do. As Sabini et al. imply, there may be an absurdity to the notion that whether it is correct to label the cause as internal or external depends on how other people acted in that situation. It sounds vaguely Heisenbergian. But it is not absurd to take such information into account in determining the diagnostic meaning of the behavior. If we know—or *should know*—that everyone else wrote a similar essay (and thus the behavior covaried with the situation), then our observation of Courtney's behavior would not provide us with much information. If, on the other hand, we knew that people were as likely to defy as they were to comply with the request to write the pro-Castro essay, then Courtney's behavior would provide more information about her. In the former case, the consensus information would not invalidate the notion that Courtney liked Fidel Castro, but it should have lowered perceivers' confidence in that attribution as the correct explanation. In other words, it is "right" to discount this explanation in light of the external factor. The error in the FAE is that perceivers do not do this sufficiently. An intriguing thing about suspicion is that they often do.

Inferences Versus Attributions: Another Distinction

Suspicion may work as an antidote to the correspondence bias because it triggers a greater degree of

attributional thinking than perceivers typically exhibit. We have amassed several lines of evidence to support this. One set of studies, for example, found that participants who were made suspicious about one actor subsequently made inferences about a second actor that continued to reflect appropriate discounting, even though the situational factors in this second case involved constraint rather than ulterior motives (Fein, 1996). Another line of research demonstrated that perceivers who were primed with abstract questions about suspicion were significantly more likely to avoid the correspondence bias when they subsequently made inferences about an actor under conditions of constraint than were participants who had not been primed; this priming effect of suspicion was no less effective than was priming participants specifically about attributionally complex thinking (Fein, Manning, & Parsons, 2000). A third set of studies found that, relative to nonsuspicious perceivers (such as those who learned that an actor's behavior occurred under strong constraint), perceivers given reason to suspect an actor's motives were much more likely to think spontaneously about questions of causality and issues of context (Fein, 1996). Using the essay attribution paradigm, we found this lack of spontaneous attributional deliberation—and the tendency of suspicion to activate such deliberation—not only among American samples, but also in samples from more collectivistic cultures, such as India and Sri Lanka (Fein, Fong, Lieberman, Pandey, & Bacon, 1998).

Indeed, results from this third set of studies in particular echo the findings of other researchers in suggesting that perceivers typically do not engage in much spontaneous attributional thinking. It is not the case that perceivers typically act as naive scientists trying to determine causality. Rather, they make inferences quickly and effortlessly, pausing to ask "why" questions only under special circumstances, such as when their expectations are disconfirmed or their sense of control or self-image is threatened (e.g., Clary & Tesser, 1983; Hamilton, 1988; Hastie, 1984; Liu & Steele, 1986; Pittman, 1993; Pyszczynski & Greenberg, 1981; Weary, Marsh, Gleicher, & Edwards, 1993; Weiner, 1985). The default for perceivers, therefore, may be to take actors' behavior at face value. Suspicion, like surprise or threat, may prompt perceivers to become more vigilant in processing attribution-relevant information, making them more hesitant to take behavior at face value.

Face Value and Correspondence

The notion of taking behavior at "face value" is the third conception of the internal-external distinction that Sabini et al. discussed. This concept is closest in meaning to what Jones meant by a *correspondent in-*

ference (Jones & Davis, 1965), and the idea that perceivers' default is to make such inferences quickly and effortlessly has been a key point underlying the dominant information processing models that Gilbert, Trope, and Quattrone have advanced over the years (e.g., Gilbert, 1989; Quattrone, 1982; Trope, 1986). Sabini et al.'s critique of the concept of face value, therefore, strikes near the heart of a great deal of thinking about the dispositional inference process.

In the multiple-stage models, the first step is a very automatic identification or categorization of the behavior. In the essay attribution paradigm, this might be a categorization of the behavior as "writing a pro-Castro essay." The correspondent inference that is likely to follow corresponds to initial identification or categorization of the behavior. Thus, this first stage is critically important in setting the stage for the ultimate inference. In these models, the often insufficient adjustment to this initial anchor is where the effortful, deliberative attributional thinking occurs, if it occurs at all.

Sabini et al. make the important point that what is considered the "face value" of a particular behavior may not always be clear. Which is the "face value" of the participants in the Milgram (1963) paradigm: the obedience to the experimenter, the torture of the victim, the avoidance of confrontation, or something else? In other words, how do perceivers categorize (automatically) this behavior and, soon thereafter, characterize the actor? This kind of question has intrigued me since I began to do research on suspicion. For example, how can the effects of suspicion be explained by Gilbert's (1989) model? The explanation that most easily fits with this model is that suspicion facilitates sufficient correction of the initial automatic correspondent inference. This may be true, but another possibility, which raises many more questions for this model, is that suspicion interferes with the initial categorization of the behavior. Is the behavior of an author in the ulterior-motives condition of an essay attribution study best categorized as "wrote a pro-death penalty essay," or might it be identified as "conformed with his professor's opinion"? How do we determine the most appropriate categorization?

Roads Less Traveled

The point here is that much more work needs to be done to better understand these initial stages of the dispositional inference process. The questions raised in the target article, and by my own work on suspicion, suggest that the categorization process may be more complex than has been suggested by the existing models. In addition, these questions point to a distinction beyond the internal-external distinction that needs to be addressed more carefully: the distinction between the dispositional inference process and the causal attri-

button process. Although the internal-external distinction is critical for questions of causal attribution, the questions of how behavior is categorized, what it means to draw a correspondent inference, and whether suspicion can interfere with the automatic processes that lead to correspondence inferences may be (somewhat) independent of the internal-external distinction. Hamilton (1998) also emphasized the need to distinguish between the dispositional inference process and the causal attribution process, and he has even proposed that *correspondence bias* be used to refer to a bias toward dispositional inferences and *fundamental attribution error* be used to refer to a bias toward dispositional attributions.

A provocative proposal such as Hamilton's (1998), along with the set of provocative questions presented in the target article, raise important concerns about our understanding of this bias, despite some three and a half decades of active research. Add these to the challenges described in my opening paragraphs, and it might suggest that the era of the FAE as the field has known it is ending. But although our understanding of the bias may, and probably should, change, the questions on which I focus here may be more invigorating than terminating for the field's interest in this bias, as well as in the dispositional inference and attribution processes more generally. These questions reveal that there are potentially fascinating roads on the trip from acts to dispositions that have not been traveled extensively. The appeal of such fresh and possibly rich terrain may be enough to propel this star bias forward into middle age, even if it might feel the compulsion to buy a red sports car to traverse it.

Notes

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The Really, Really Fundamental Attribution Error

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One of the most important contributions that can be boasted by the vast literature on the putative fundamental attribution error (FAE) is that it inspired the present analysis by Sabini, Siepmann, and Stein (this issue). This article is a major intellectual achievement and a milestone in our progress toward understanding the manner in which people interpret the behavior of themselves and others. In this commentary, I make three small additional points, all derived from ones made in the target article. First, the FAE in all of its forms—including the recast version attempted by Sabini et al.—is untenable, and in some of its forms it is incoherent. Second, any meaningful comparison between the power of personal variables and the power of situational variables in the determination of behavior—much less any imputation of error—must await the development of a language and technology for describing and assessing the psychologically important aspects of situations. Third, the really, really FAE—by psychologists, not by laypersons—may be that of underestimating the complexity both of situations and the conflicting goals that people try to pursue simultaneously within them.

The Demise of the FAE

Few readers could come away from Sabini et al.'s demolition of the standard forms of the FAE and maintain a belief that the FAE, as traditionally described, is an appropriate way to characterize any basic aspect of social perception. This famous error revolves around laypersons' alleged confusion about the relative importance of situational and dispositional causes of behavior. The nature of these causes has been

conceptualized in at least three ways, but the first conceptualization is incoherent, the second leads to conclusions opposite to those argued by proponents of the FAE, and the third tends to disconfirm the FAE, so far as data are available.

The first conceptualization views dispositional causes as emanating from within the skin and situational causes from emanating from without the skin (Gilbert & Malone, 1995). Sabini et al. convincingly establish that this distinction is incoherent. The original proponent of the FAE¹ (Ross, 1977) himself pointed out that, in these terms, every situational explanation for behavior implies a dispositional one, and vice versa. For example, consider the Milgram (1974) situation. A situational pressure to obey that comes from outside the skin only produces obedient behavior in a person who has a disposition to obey, inside the skin. This disposition might be surprisingly stronger than the disposition to be kind to an innocent victim, but the error people make when they predict that kindness will overcome obedience is not one of overestimating the power of dispositions in general but of underestimating the strength of one disposition relative to another.

The second, "statistical" version of the dispositional-situational dichotomy—one proposed by Ross (1977) and endorsed by Gilbert (1998)—ironically reverses the interpretation of many putative demonstrations of the FAE. If, following the statistical criterion, one considers a behavior to be

¹E. E. Jones earlier propounded an equivalent tendency called the *correspondence bias*, but the term *fundamental attribution error*; being catchier, accordingly caught on more widely (see Gilbert & Malone, 1995, for a history).

dispositionally caused when there is large interpersonal variation and situationally caused when there is small interpersonal variation, then the outcome that shows the most dispositional causation arises when 50% of your research participants do one thing and 50% do the opposite. Therefore, when, for example, laypersons and psychiatrists estimated that fewer than 1% of subjects would obey Milgram's (1974) experimenter, they were predicting that the situation would have an extremely strong effect, that of producing disobedience. They were wrong: The real proportions varied by condition, but were much closer to even. And so their error was in overestimating the power of the situation and underestimating the degree of interpersonal variation. The same basic principle applies to many other putative demonstrations of the FAE.

A third conceptualization of the person-situation dichotomy was not discussed by Sabini et al., but is no more favorable for the existence of an FAE. This conceptualization concerns the relative utility of personality and situational variables for the prediction of behavior. Proponents of the FAE often assert that research shows personality variables to be weakly related to behavior, whereas situational variables are strongly related to behavior (and people therefore err by believing in personality anyway). This argument is typically made by subtraction. If a personality variable correlates, 40 with a behavioral outcome, then it is asserted that the remaining 84% of the variance can be assigned, by default, to the situation.

This argument reveals only how little we know about situations. If there were a set of situational variables that could be correlated with behavior, then any variance left over could just as well be assigned to persons! But we don't have a well-developed set of situational variables or, really, any comprehensive set, at all. So despite the rhetoric touting the "power of the situation," we know very little about the basis of that power—or its real amount.

Some years ago, Ozer and I (Funder & Ozer, 1983) recalculated the effect sizes of a few situational variables that could be identified and that were widely acknowledged as important. For example, the size of the effect of distance of the experimenter and victim in the Milgram (1974) situation and of number of bystanders in the Darley studies (Darley & Batson, 1973; Darley & Latané, 1968; all specifically mentioned by Sabini et al.) are each equivalent to a correlation between .30 and .40. If we resist the temptation to ascribe the remaining variance to persons by subtraction, it can still be noted that many effects of personality on behavior are in this range, and measures of cross-situational consistency are often much higher (e.g., Funder, 1999; Funder & Colvin, 1991). The basic and necessary claim of proponents of the FAE, that situational variables are generally more powerful than personality variables as predictors of

behavior, therefore seems extremely doubtful on empirical grounds.

In response, it might be argued that the proximity of the experimenter, the distance of the victim, or the number of bystanders is not the real basis of the power of these situations. But that only raises the question, what is? Until psychology develops a vocabulary for describing the psychologically important aspects of situations, and a technology for manipulating or measuring them, we will never understand what aspects of situations determine their influence on behavior or how strong they really are. And as long as we lack that understanding, we are in no position to describe anybody else's estimates of the power of the situation as erroneous, whether fundamentally or otherwise.

The Really, Really FAE

Following Sabini et al.'s close analysis (and demolition) of the standard FAE, they attempt to reconstruct a variant of it by recasting the dispositional-situational dichotomy in terms of ego-syntonicity. People overestimate their capacity to choose to do things that are consistent with their images of their best selves, Sabini et al. suggest, and underestimate the degree to which they instead behave to the contrary. This is a brilliant suggestion, and certainly an improvement on the FAE, but in the end, I do not think it is successful.

The problem is that the concept of ego-syntonicity is vague (at least as vague, for example, as the concept of face value that Sabini et al. so compellingly critique). Apparently, *ego-syntonicity* refers to acting in accord with those of one's own dispositions that one is proud of, or at least not ashamed of. But the typical situation includes many forces that elicit many dispositions, few of which may be very laudable or shameful, but many of which may nonetheless be in conflict.

For example, the Milgram (1974) situation evokes various dispositions that could be construed in various ways, including the dispositions to be kind, cooperative, likable (to the victim), likable (to the experimenter), competent (as a "teacher" or research participant), intelligent, strong, scientific, decisive...the list is very long. Which of these dispositions are ego-syntonic, and which are ego-dystonic? Sabini et al. argue that the (ego-dystonic) motivation to avoid embarrassment overrules the (ego-syntonic) motivation to be kind, but one could just as well argue that the (ego-syntonic) motivation to be cooperative overrules the (equally ego-syntonic) motivation to be kind, or even the (ego-dystonic) motivation to fold under pressure. What is *really* going on? The answer is by no means obvious, but what *is* obvious is that in this situation—as in many others in life—multiple motivations are activated, and fully satisfying all of them at once is impossible.

Perhaps, then, this is the really, really FAE (by psychologists, not our participants): to believe that the causes of behavior are simple and easily dichotomized. (Our participants are not prone to this error, as evidenced by their typical, frustrated reaction to attribution questionnaires.) As Freud taught us long ago, and the modern theorists of parallel distributed processing models of cognition teach us now, many different things are going on at the same time within the typical human head (and heart). We try to serve many masters, seek many goals at the same time, and life is a continuous struggle to balance them all and find some kind of workable compromise.

Notes

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The Fundamental Attribution Error Where It Really Counts

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Sabini, Siepmann, and Stein (this issue) have written a provocative article that reminds social psychologists of two important lessons: that human behavior is not easily parsed into situational and dispositional causes, and that concerns about propriety and face are powerful and pervasive determinants of how people choose to act. It might seem odd that social psychologists, of all people, would need the latter reminder. But the fact is that although we can all throw around a few quotes from Goffman and might even give an isolated lecture on the dramaturgical approach to social psychology, issues of self-presentation are more often treated as annoying methodological artifacts than as compelling phenomena worthy of attention in their own right. Mainstream social psychology only infrequently touches the subject and even less frequently touches it for long.

Sabini et al. show us the error of our ways. It may be tempting to think of most human behavior as guided by rather broad, direct, and even noble concerns, but the reality is often less flattering. One might think, for example, that questions raised at the end of a colloquium are typically motivated by a genuine quest for knowledge, but as often as not, they are performances staged as much to dis-

play knowledge as elicit it. One might think that the decision of whether to seek a doctor's advice would be controlled solely by the prevailing medical issues, but quite often such decisions are hijacked by concerns about hurting another doctor's feelings or about being seen as a hypochondriac. And one might think that the decision of whether or not to switch one's first-grader from one classroom to another would be based primarily on the educational merits of the move, but often the merits take a back seat to fears about seeming pushy, demanding, or elitist.

Sabini et al. highlight the importance of these issues of face in everyday social interaction and in such classic experiments as Milgram's obedience studies, Darley and Latané's bystander intervention studies, and Asch's studies of conformity. They rightly point out that the fear of making one kind of scene or another is a tremendously powerful "channel factor" that lies at the heart of how people act in these studies—and why their actions seem so surprising.

But is fear of embarrassment the *only* channel factor whose influence is surprisingly powerful? Are all experimental surprises in the "situationist" tradition (indulge us for now) the result of underestimating people's concerns

about propriety? Clearly not. The percentage of U.S. citizens buying war bonds during World War II nearly doubled when a public service campaign encouraged individuals to buy "an extra bond" rather than simply to "buy bonds" (Cartwright, 1949), a result that cannot readily be attributed to concerns about propriety. Individuals have been shown to be eight times as likely to get a tetanus vaccination if given a map to the clinic and encouraged to review their schedules to identify the most convenient time for the procedure (Leventhal, Singer, & Jones, 1965), a finding that, likewise, does not fit the embarrassment account. And then there are the dramatic increases in compliance achieved through the "foot in the door" procedure of gradually escalated requests (Freedman & Eraser, 1966; Schwartz, 1970). In each of these cases, the manipulation of some seemingly minor element of the situation had a very large effect on observed behavior. And it did so without influencing the likelihood of participants experiencing or causing embarrassment. Thus, concerns of face and embarrassment are indeed surprisingly powerful determinants of people's behavior, just as Sabini et al. maintain. But they are hardly the only surprisingly potent controlling forces. Lecturers in social psychology who wish to dazzle their audiences with counterintuitive findings are not restricted to those that result from the audience's failure to appreciate the strength of people's fear of embarrassment.

The Person-Situation Distinction

The second notable contribution of Sabini et al.'s article is the reminder that the situational-dispositional distinction is no cut-and-dried dichotomy. It is not possible, clearly, for an external stimulus to influence a person's chosen actions if it does not resonate with some faculty or disposition of the person. Thus, a precise accounting of how much a given action stems from the impinging stimulus rather than from the faculty or disposition with which it makes contact is wrenchingly difficult and often impossible. Indeed, it is for precisely this reason that social psychologists have not held fast to this distinction, having backed away from it in two important respects. First, partly because attribution theorists recognized that one cannot determine whether a given attribution—or even a given pattern of attributions—is "right" or "wrong," their interest in looking at people's understandings of what happened in the past morphed into an interest in looking at how people's understandings of the past influence their thoughts about what is likely to happen in the future. They became interested, in other words, in prediction (Nisbett & Ross, 1980; Ross, 1977). This led to the very productive marriage of attribution theory with work on judgment under uncertainty and on decision making (Griffin, Gonzalez, & Varey, 2000; Kahneman, Slovic, & Tversky, 1982; Kahneman & Tversky, 2000). In these areas, the normative issues are

not always such a morass and one can often determine whether a set of responses is rational (in the case of choice) or accurate (in the case of judgment).

The second way that social psychologists have veered away from the person-situation distinction has been simply to avoid trying to precisely apportion causal responsibility to persons and situations. The fundamental attribution error (FAE), after all, is not a contention that situations are all powerful, or even that situational influences on behavior are more powerful than dispositional influences. Rather, it is the contention that the layperson's intuitions give more weight to dispositions and less weight to situational influences than what psychologists have learned—however uncertainly—is warranted. Psychologists and laypeople agree that behavior is a function of the person and the situation. But it appears that that equation is written with a capital *P* and a small *s* by the layperson, a weighting scheme at variance with a formidable body of research in both personality and social psychology (Gilbert, 1998; Gilbert & Malone, 1995; Jones, 1990; Kunda & Nisbett, 1986; Mischel, 1968; Nisbett, 1980; Ross & Nisbett, 1991).

Sabini et al., and other critics of the work on the correspondence bias, might counter with the objection that one cannot compare lay intuitions about dispositional and situational causation with what social psychologists have learned about the subject if the latter have not really learned anything at all or if what they have "learned" is wrong. But social psychologists have learned something about the surprisingly potent power of situational influence. Although personal versus situational causation can often be a conceptual thicket in which it is easy to become ensnared, there are times when the prevailing influence is quite clear. When social facilitation effects are observed in the absence of social desirability motives (Markus, 1978; Schmitt, Gilovich, Goore, & Joseph, 1986), it is clear that it is an environmental event—the mere presence of others—that is controlling behavior. Similarly, when pronounced mimicry is induced by stimuli outside the mimic's conscious awareness (Bargh, Chen, & Burrows, 1996; Chartrand & Bargh, 1999), it is true that the mimicry would not happen without the mental machinery that people have stuffed into their crania. But does that alter the fact that it was the experimenter who orchestrated the elicited behavior? To redescribe such effects as due to some disposition of the person is to fail to do justice to the terms *situational* and *dispositional*.

The broader point, however, is that it is *situations* that social psychologists manipulate in their experiments, and time after time it has been found that seemingly modest situational manipulations have a surprisingly powerful impact on behavior. It is this surprise—and the absence of any corresponding surprises over the power of personal traits—that informs us that

most people weight p too heavily and s not heavily enough in the intuitive version of $b = f(p,s)$. As Ross and Nisbett (1991) put it, "there are no famous studies in which stable personal attributes...have proved to be markedly better predictors of behavior than academicians or even laypeople had anticipated" (p. 95). But situational surprises are not hard to come by. One simply does not expect the belief that four others are present to result in a 64% decrease in a person's likelihood of responding to an unambiguous epileptic seizure (Darley & Latané, 1968).¹ Nor does one expect the arousal produced by 10 min on a stationary bicycle to increase a person's self-rated sexual arousal by 40% (Cantor, Zillman, & Bryant, 1975). And, as alluded to previously, one does not expect the provision of a map and an encouragement to review one's schedule to increase vaccination rates eightfold (Leventhal et al., 1965).

We know of no complementary demonstrations of personal traits influencing behavior to a similarly surprising degree. We do not contend, we wish to be clear, that personal traits are fundamentally incapable of exerting such surprisingly powerful effects. It is just that no demonstrations of that sort now exist.²

Syntonic, Dystonic, and the FAE in Everyday Life

Another laudable element of Sabini et al.'s article is that, although purporting to explode the person-situation distinction, it does not claim that the notion of personal versus situational causation is an alien concept foisted on the lay public by some wayward band of social psychologists. Instead, Sabini et al. provide a serious account of why the notion has great resonance not just with social psychologists, but with the layperson as well. At the core of their account is the distinction between ego-syntonic and ego-dystonic causes of behavior. Behavior is seen as externally caused if it flows from ego-dystonic elements of the self—if it flows from "the person's regrettable dispositions." Putting

the various elements of their article together, then, the argument must be that people are too quick to assume that observed behavior is ego-syntonic to the actor because they fail to take sufficient account of the actor's desire to avoid embarrassment. The distinction is an interesting one—and quite likely a significant one as well. But in the absence of any existing data pertinent to this account, only time will tell whether the distinction provides a helpful and productive framework for understanding everyday causal attribution.

We suspect, however, that their analysis of ego-syntonic and ego-dystonic motives will not be as productive as Sabini et al. hope. For one thing, there are significant elements of the vast literature on the FAE that this account simply does not address. For instance, it is hard to see how this account can explain the attributional tendencies observed in the "perceiver-induced constraint" paradigm (Gilbert & Jones, 1986). It is also hard to see how intuitions about embarrassability (or the lack thereof) can explain the frequently documented tendency for dispositionist (i.e., ego-syntonic) attributions to be enhanced when attentional resources are taxed (D'Agostino & Fincher-Kiefer, 1992; Gilbert, 1989; Gilbert, Pelham, & Krull, 1988).

But our doubts about Sabini et al.'s analysis are more profoundly influenced by what we suspect will be its inability to speak to the real-world dispositionism that experimental studies of the FAE are designed to illuminate. After all, it is real-world, and not laboratory, dispositionism that is social psychology's proper focus, however engaging some of the laboratory studies may be. And with respect to everyday dispositionism, the person-situation distinction appears far more apt than the ego-syntonic versus ego-dystonic dichotomy. When individuals make disparaging comments about impoverished single mothers ("Why doesn't she just work longer hours?"), it is clear that it is the constraints imposed by the target's situation that are not being adequately appreciated, not a readiness to confuse dystonic with syntonic motives. Even more poignantly, when individuals blame the victim for random acts of misfortune and insist that the world is just (Lerner, 1980), it is not a conflation of internal dispositions that is the problem. It is a confusion of what is patently outside the person—luck, chance, the fates, call it what you will—with some characteristic on the inside. When people maintain that "cripples, dwarfs and those born deaf or blind are the products of their own past actions" (Humphreys, 1943, p. 55; see also Amneus, 1998), it is once again a confusion between what has been visited upon a person from the outside and what that person's character has, through some perverse dispositionist logic, summoned. The dispositionist reflex is so strong, in fact, that if no defect in a person's character can be found, his or her tragic affliction is

¹To explain participants' behavior in this study, Sabini et al.'s account would have to maintain that it is more embarrassing to call the unambiguous emergency to the attention of the experimenter than it is to be seen as someone who is less concerned with the victim's fate than everyone else is. That may be, but as presently articulated, the embarrassment account can only fit such data after the fact.

²If we were to search for equally surprising effects from the personality side of the ledger, we would place our bets on the dispositional tendency toward happiness or unhappiness, which often seems to override the best and worst of life's circumstances. Indeed, if the results of Brickman's and colleague's study (Brickman, Coates, & Janoff-Bulman, 1978) of lottery winners and victims of spinal cord injury were as strong as they are often described as being, such a demonstration would already exist. At the present time, however, one searches in vain for a dispositionist counterpart to the surprising demonstrations of Asch (1956), Darley and Batson (1973), Darley and Latané (1968), Milgram (1963), and so on.

simply attributed to some flaw or transgression in a "past life." (This explains how "those born deaf or blind" could have caused their misfortunes through "their own past actions.") This logic reaches its perverse peak with the argument that children who are sexually abused are likely to have been sex offenders themselves in a past life (Woolger, 1988).

Social psychology's traditional analysis of the FAE encourages the condemnation of such beliefs. Sabini et al.'s analysis does not. The message conveyed to students through the traditional analysis is to look carefully at the details of someone's situation before drawing a dispositional inference. Sabini et al.'s analysis only encourages people to concede that troublesome behavior that looks like it might be syntonetic with a person's sense of self is actually dystonic. Thus, Sabini et al.'s analysis robs social psychology of one of its great humanizing messages: that failure, disability, and misfortune are, more often than people are willing to acknowledge, the product of real environmental causes.

Note

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Wittgenstein Was Right

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In an elegantly reasoned article, Sabini, Siepmann, and Stein (this issue) argue that the most important claim of social psychology since World War II—the fundamental attribution error (FAE)—is unsupported. Most readers will know that the FAE maintains (a) that what people do during social interaction is primarily determined by the situation they are in and (b) that naive observers erroneously believe that what people do is a function of their personalities rather than of the circumstances in which they find themselves. Sabini et al. examine six classic social psychological studies that are routinely invoked to support the FAE; they show, in each case, that the findings are better interpreted in terms of peoples' desires not to look foolish or to avoid embarrassment. They also show that the distinction between internal and external causes of behavior is hopelessly muddled. Thus, they effectively make the point that the social psychological studies most frequently used to show that situations are more important than dispositions as explanations of social behavior are better seen as making the opposite point.

I make four observations about the target article. The first is that the article is a prototypical exemplification of Wittgenstein's (1953) remark that in psychology, there are empirical methods and conceptual confusions. Our training and core practices concern research methods; the discipline is and always has been deeply skeptical of philosophy. We emphasize methods for the verification of hypotheses and minimize the analysis of the concepts entailed by the hypotheses. And that is exactly what Sabini et al. do so well. All the empiricism in the world can't salvage a bad idea.

Second, Sabini et al. let one bad idea slip by. They use the word *situation* as if there were a common referent for the term, but a closer analysis reveals that there is no common referent (cf. Hogan & Roberts, 1999). The person by situation research agenda was doomed from the outset because there is no agreed-upon meaning of the term *situation*.

Third, the target article presents extremely bad news for social psychology as it is normally practiced. The standard research agenda for social psychology has always been a search for person by situation interactions or, put differently, an examination of how "situational" factors influence individual behavior. The target article suggests that this enterprise has failed, that the most vivid and compelling studies in the modern history of social psychology can be parsimoniously reinterpreted in terms of what the people in the experiments had on their minds—and, in most cases, it was a

desire to avoid seeming foolish. Put differently, the article strongly suggests that traditional social psychology is best interpreted in terms of some simple ideas about human nature, that is, in terms of personality psychology.

Finally, as I read the article, I kept thinking, "It's déjà vu all over again." The intellectual tradition that extends from William James (1890), through George Herbert Mead (1934), to Theodore Sarbin (1954) and Erving Goffman (1955) has always maintained that people govern their behavior in terms of how they think others will evaluate them. People are sensitive to social feedback and behave in public so as to control that feedback. Drawing on this tradition, Alexander and Knight (1971) proposed that social psychological experiments in particular could be interpreted in terms of people's efforts to manage their identities; my point is that although I fully agree with Sabini et al., their point has been made several times before (e.g., Baumeister, 1982; Tedeschi, 1981). And the point is as valid now as it was then.

Note

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Living in the Minds of Others Without Knowing It

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Growing up in the 1960s, I was part of the generation of adolescent boys who fought for the right to wear our hair longer than most parents, teachers, and principals considered proper at the time. In one particularly enlightening dinnertime conversation on the topic, my dad argued that the crux of his objection was that I wore my hair and sideburns long just because I was concerned about fitting into my peer group. I didn't disagree—he was undoubtedly right—rather, I observed that he too wore his hair precisely like all of the other men where he worked, suggesting that he was also responding to social pressures. In response, he maintained that he didn't wear his hair as he did to fit in but rather because he found it most "comfortable." Yeah, right.

The central point of Sabini, Siepman, and Stein's target article (this issue) is that people in Western cultures underestimate the degree to which people's behavior is affected by their concerns with preserving face and avoiding embarrassment. Sabini et al. suggest that Americans are more concerned with preserving their public images than most people suspect, and they reinterpret a number of important findings in social psychology as reflecting people's tendency to underestimate the effects of self-presentational concerns regarding behavior. My comments focus only on their point involving self-presentational myopia, which I think is right on target, and not with their reinterpretation of the empirical data (which also seems plausible).

The observation that people underappreciate the effects of social evaluation on behavior echoes a point made one hundred years ago by Cooley (1902) in his discussion of the "looking-glass self." Cooley championed the view that people's views of themselves are largely a product of how they think others perceive them, but he noted that many people resist the idea that they are affected by others' evaluations:

Many people scarcely know that they care what others think of them, and will deny, perhaps with indignation, that such care is an important factor in what they are or do. But this is illusion. If failure or disgrace arrives, if one suddenly find that the faces of men show coldness or contempt instead of the kindness and deference that he is used to, he will perceive from the shock, fear, the sense of being outcast and helpless, that he was living in the minds of other without knowing it, just as we walk the solid ground without thinking how it bears us up. (p. 11)

Recent research has documented Cooley's (1902) observation that people do indeed live in the minds of

others without knowing it (or at least without admitting it). When Harter, Stocker, and Robinson (1996) asked adolescents whether their own self-approval and self-disapproval were affected by the degree to which other people approved and disapproved of them, only about one third of their respondents admitted that it was. My students and I recently conducted a study to determine whether people's claims that they are unfazed by social evaluations are accurate or, in Cooley's terms, "illusion." After identifying groups of college-age participants who reported that their feelings about themselves were not affected by social approval and disapproval, we subjected them to an experimental procedure in which they received bogus approval or disapproval, ostensibly from other participants. Analyses of their subsequent reactions showed absolutely no relationship between participants' claims about whether they were affected by approval and disapproval and how they actually responded to social feedback. Participants who vehemently denied that they were personally affected by social approval and disapproval reacted as strongly to the feedback as those who admitted that they were affected by others' evaluations (Leary, Hoagland, Kennedy, & Mills, 2000). Unfortunately, we have no way of knowing whether participants who claimed that they were unaffected by social approval actually believed it or whether they simply could not admit it to themselves or to others.

Interestingly, psychologists have shown the same reluctance to acknowledge the powerful influence of self-presentational motives on human behavior. Until recently, one could read most textbooks in social psychology without getting even a hunch that people are concerned with what other people think of them or that concerns with public image play any appreciable role in human behavior. Furthermore, social psychologists have historically preferred explanations based on intrapersonal motives (such as the needs for cognitive consistency, perceived control, and self-esteem) over explanations based on interpersonal motives (such as facework, impression management, or a desire for acceptance).

Even when acknowledging that people are sometimes affected by others' views of them, many writers have construed concerns about one's public image as indicating psychological maladjustment rather than as reflecting a normal, if not functional, feature of human behavior (e.g., Bednar, Wells, & Peterson, 1989; Buss & Briggs, 1984; Deci & Ryan, 1995). According to this view, mature, well-adjusted individuals should be

autonomous and self-directed, paying little attention to how they are perceived and evaluated by other people. By implication, then, being responsive to public opinion indicates excessive dependency, defensive self-esteem, pretension, or insecure attachment. Thus, the attributional error that Sabini et al. suggest pervades lay attributions also seems to characterize professional psychologizing.

Several historical forces have likely converged to lead people in Western societies, particularly the United States, to underestimate the influence of self-presentational concerns, including the fear of embarrassment, on their behavior. First, as Sabini et al. suggest, American culture places a high value on individualism. In part, this may be because the individuals who initially settled the American colonies were particularly likely to be individualists seeking their own freedom or fortune—or else they wouldn't have undertaken such a bold life change as coming to the New World. Indeed, the formation of the United States itself was based in large part on the colonists' efforts to pursue their own lives without interference from the Crown. People who believe that they are endowed with "unalienable" individual rights such as "life, liberty, and the pursuit of happiness" are likely to see their actions as predominately self-determined. This individualist mentality was furthered by westward expansion, which did indeed require a great deal of rugged individualism and self-sufficiency.

Second, the special brand of American social philosophy and commentary that blossomed in the 19th century stressed each person's right, if not obligation, to self-determination. For example, Thoreau's (1988) admonition to allow people to march to their own drummers implied that people could and should disregard the fetters of convention, conformity, and social pressure. Third, the Judeo-Christian tradition that has dominated American religion exhorts its followers to show little concern for worldly matters of all kinds and to disregard social convention in favor of moral law. Furthermore, the prevailing Protestant emphasis on personal morality and responsibility may have played a role by emphasizing personal conscience and choice over social pressures (and even religious authority).

When contrasted with Eastern cultures that encourage people to consider the implications of how they are perceived, not only for themselves but also for their families and other social groups, it is easy to see why Americans in particular may be prone to downplay the influence of face concerns on their behavior (Morisaki & Gudykunst, 1994). The irony, of course, is that all people try to convey desired images of themselves to avoid embarrassment (Leary, 1995), and we have little evidence that people in the United States are actually less affected by such concerns than people in other parts of the world (although they may promote different kinds of public images).

Sabini et al.'s analysis can be profitably taken a step deeper by digging beneath people's concerns with maintaining face and avoiding embarrassment to the more fundamental motive that underlies this tendency. Specifically, people's fear of embarrassment appears to stem from their concerns with being sufficiently valued and accepted by other people (Leary, Koch, & Hechenbleikner, in press). That is, one consequence of losing face is typically a decline in the degree to which the individual is valued as a relational partner by others. Importantly, people will suffer relational devaluation whether it is their own face that is threatened (and they convey impressions that detract from their social desirability to other people) or the face of another person (in which case they may be likewise regarded as an undesirable relational partner who fails to maintain other people's claimed images; Goffman, 1959). In either instance, the person's acceptance by other people may be jeopardized. Given the strong (and probably innate) motive to avoid rejection (Baumeister & Leary, 1995), well-adjusted people should want to avoid embarrassing, face-threatening predicaments. Of course, people are sometimes excessively concerned with maintaining face, but the functional value of being concerned with one's public impressions appears to be beyond dispute (Leary, 1995; Miller, 1996).

The pressing question is why people don't seem to recognize the influence of these kinds of concerns on either themselves or others. In part, Cooley (1902) may have been on the right track when he offered the metaphor of people not thinking about how the ground they walk on bears them up. Concerns with self-presentation are so pervasive—indeed, people are rarely free of all self-presentational constraints on their behavior—that they may find it difficult to appreciate the degree to which their interest in maintaining a desired public image constantly affects their own and other people's actions.

Note

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On the Primacy of Embarrassment in Social Life

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It is conceivable that social psychologists are finally getting it right. After decades of study of internal, cognitive concepts such as attitudes and attributions, social scientists are giving increased attention to the manner in which interpersonal motives and influences shape the self and direct behavior. For instance, Leary's (1999) sociometer model persuasively argues that self-esteem is tied to the quality of our relations with others, and Swann's self-verification studies (e.g., De La Ronde & Swann, 1998) show that our friends and lovers can make or break our self-concepts. In fundamental ways, who and what we are may be embedded even more thoroughly in our social networks than many theorists have realized.

Now, in the same spirit of newfound enlightenment, Sabini, Siepmann, and Stein (this issue) suggest that (a) the motive to save face and avoid embarrassment is a powerful, pandemic force that underlies diverse domains of social behavior and (b) our collective underestimation of the strength of that motive has left us individually and professionally confused about the true sources of our actions. At least one of these assertions is entirely correct, in my view, and the other has merit. There is, however, some imprecision in Sabini et al.'s argument that dilutes its impact. With greater clarity, their suggestions may be more compelling.

First, let's put their contentions in historical context. Despite Goffman's (1955, 1959) provocative discussion of face in social life, the terms *face* and *embarrassment* rarely appear in social psychology textbooks. (Indeed, the broader concepts of self-presentation and its synonym, impression management, that encompass face and embarrassment have only re-

cently gained routine mention.) These topics have yet to be discovered by the profession as a whole. Nevertheless, there have been occasional prior efforts to demonstrate that impression management is a major player in various social psychological phenomena. For instance, following the reasoning that the usual cognitive dissonance study induces participants to tell lies to others—thereby creating a threat to face—Schlenker, Forsyth, Leary, and I (1980) demonstrated that the usual "less is more" change in attitude that follows a dissonance manipulation occurs only when participants report their attitudes publicly and not when they send them privately to others who are unaware of the dissonance-arousing events. We concluded that instead of reflecting a desire for cognitive consistency, some so-called dissonance effects occur because people simply do not want to look bad to others. Other wide-ranging applications of a self-presentational perspective were explored by Baumeister (1982). Thus, Sabini et al.'s assertions regarding the role of face in social life are not entirely new ideas. They are right to revisit this concept, however, because impression management has often been "treated like a rude bastard relative at a family gathering" by mainstream social psychology (Baumeister, 1986, p. vi). The value of the framework Sabini et al. propose has generally been overlooked.

Where, then, do their assertions take us? Fear of embarrassment figures prominently in their account of social life, therefore I think that a good answer to that question hinges, in part, on what embarrassment is and what it is not. Sabini et al. seem reluctant to take a stand on this issue, but recent studies of embarrassment have delineated the state rather clearly (see Miller, 1996, for a thorough review). Embar-

arrassment is the acute state of awkward and flustered abashment and chagrin that follows events that produce a threat of unwanted evaluations from real or imagined audiences. It almost always occurs in public. If people do experience embarrassment when they are alone, it is inevitably because they are vividly imagining what others would think if they were present (Tangney, Miller, Flicker, & Barlow, 1996). The mere knowledge that others are aware of some misstep or misbehavior—no matter how they respond—is enough to trigger embarrassed emotion. Importantly, embarrassment is a discrete emotion: It occurs automatically and involuntarily, lasting moments instead of hours, and it is accompanied by a coherent, obvious sequence of nonverbal behavior that clearly distinguishes it from related states such as amusement or shame (Keltner, 1995). It is also accompanied by unique physiological changes, such as (in many cases) blushing, the visible reddening of the face and neck that signals one's chagrin. Altogether, then, a person's embarrassment is ordinarily plain to anyone who is watching; observers can tell when someone among them is embarrassed (Marcus & Miller, 1999).

Why should such an emotion exist? Presumably, because of its interpersonal effects. Embarrassment seems to exist because we care about what other people are thinking of us (see Miller, 1996). For one thing, the diverse events that cause embarrassment all increase the probability that others will form undesired impressions of us (Miller, 1992). Moreover, children do not become embarrassed by the same subtle events that embarrass adults until they are mature enough to take others' perspectives and to envision what those others are thinking of them (Bennett, 1989). In addition, dispositional susceptibility to embarrassment—or embarrassability—is closely tied to (a) concern about the appropriateness of one's behavior and (b) fear of negative evaluation (Miller, 1995). The converging data suggest that, at its core, embarrassment springs from people's concerns about others' evaluations of them and that it results when undesired evaluations are imminent.

For that reason, witnesses can take evident embarrassment as a sure sign that people are chagrined by their recent behavior. Because people cannot persuasively feign embarrassed behavior (e.g., blushing), observers can treat a person's obvious abashment as a trustworthy sign that the person is authentically contrite and regretful (Castelfranchi & Poggi, 1990). Remarkably, and consistent with this possibility, those who become embarrassed after some misbehavior elicit more favorable evaluations from observers than do those who remain unperturbed by their sins (e.g., Semin & Manstead, 1982). Indeed, most of the time, embarrassed people receive supportive and empathic reactions from bystanders who help them shrug off

their predicaments (Metts & Cupach, 1989). When people communicate their heartfelt chagrin to others by becoming embarrassed, they usually mollify those others and receive more kindly treatment from them than they would have received had they not become embarrassed.

These facts raise the provocative possibility that embarrassed emotion evolved as an alarm and repair mechanism that helps us cope with the threat of social disapproval and the specter of rejection (Miller & Leary, 1992). Embarrassment alerts one to potential threats and usually motivates desirable behavior that reassures others and rectifies one's transgressions (Miller, Bowersox, Cook, & Kahikina, 1996). It also reliably communicates one's mortification at one's predicament and one's sensitivity to others' judgments. Thus, embarrassed emotion both warns sufferers of adverse social outcomes and helps remedy any interpersonal damage that has been done. It's unpleasant but adaptive, and that's probably why embarrassment exists.

Why, then, do people fear embarrassment, as Sabini et al. suggest? There are several reasons. First, embarrassment is intrinsically aversive. The emotion itself is uncomfortable and upsetting, and we are quite aware that it signals some real or imminent social peril. Second, adolescents tend to punish those who become embarrassed in their midst. Although adults typically respond to others' embarrassment in a supportive manner, youngsters more often laugh at and tease their embarrassed peers (Stonehouse & Miller, 1994). The benefits of embarrassment are therefore less apparent when we are young. Third, embarrassed people tend to overestimate the severity of their predicaments. We routinely think that others are paying us more attention than they really are (Gilovich, Kruger, & Savitsky, 1999), and we assume that others are judging us more harshly than they really do (Semin, 1982). Moreover, people typically do not realize that their embarrassment is mollifying and reassuring—rather than aggravating—their audiences. Finally, people are usually quite aware of the lengths to which they go to avoid potentially awkward situations, but they are less likely to detect and recognize similar behavior from others; as a result, people may be burdened by the illusion that they dread embarrassment more than others do.

The result of all this, as Sabini et al. correctly note, is that the mere threat of embarrassment is a potent influence on social behavior. To avoid embarrassment, people undoubtedly often behave in more polite, respectful, charitable, and honest ways than they would if they were free of the scrutiny of others. In these cases, fear of embarrassment serves the ends of socialization and social control. On the other hand, there's also no question that people sometimes risk their own long-term well-being to evade tempo-

rary, short-term embarrassment (see Miller, 1996). They put off awkward pelvic or prostate exams, fail to use condoms, and do not seek help for their emotional problems or drug use. They may also be unlikely, as Sabini et al. assert, to stand up to pushy experimenters in social psychology studies. As Leary and I argued, "the possibility of being embarrassed seems to dictate and constrain a great deal of social behavior; much of what we do and, perhaps more important, what we don't do is based on our desire to avoid embarrassment" (Miller & Leary, 1992, p. 210).

Still, we should distinguish dread of embarrassment, which is an anticipatory anxiety, from embarrassment itself. As noted previously, embarrassment is an emotion that follows events that portend real damage to a person's desired identity; it is characterized by feelings of surprise, exposure, fluster, and chagrin (Miller & Tangney, 1994). When embarrassment occurs, people are awash in startled, awkward sheepishness. In contrast, fear of embarrassment precedes and anticipates such events and is probably a blend of apprehension and excitement that is akin to shyness (Miller, in press). The two states are quite different and should not be confused, particularly because moderate embarrassment is ordinarily a beneficial, constructive response to adverse situations, whereas fear of embarrassment is often less adaptive, leading people to do things (as Sabini et al. suggest) "that we (and they) see as wrong, in one sense or another."

I think another important clarification is in order, as well. The interpersonal motive that Sabini et al. consider to be so important in social life is not fear of embarrassment, in my view, but something more. Tying so many specific behaviors to a motive to save face and avoid embarrassment is too narrow a conception. The reason why face, fear of embarrassment, and embarrassment itself are so influential in social life is that they all reflect humans' extensive and enduring concerns with what other people are thinking of them. The involuntary nature of embarrassment and the lengths to which we go to avoid it are just instances of a broader phenomenon, *sensitivity to social evaluation*, which is at work whenever others are present (or may later learn of one's actions). This concern unquestionably affects behavior in social psychology experiments. Indeed, the human conscience may just be "the inner voice that warns us that someone may be looking" (Mencken, 1949, p. 617).

And why should we be so alert to others' judgments? Because they bear on a fundamental, universal, prepotent social motive, the "need to belong" (Baumeister & Leary, 1995). People are driven to establish a minimal level of acceptance

from and closeness to others, presumably because those of our forebears who behaved in such a manner were more likely to survive in our difficult ancestral past. When humans lived in small tribal groups, those who were shunned and rejected by others were certainly less likely to reproduce, and, with saber-toothed tigers lurking about, "concern over acceptance from others may literally have been a matter of life or death" (Miller, 1996, p. 130). I think that embarrassment, a servant of social evaluation, evolved as one manifestation of the basic human need to seek inclusion and avoid rejection in our dealings with others, and it is this primal motive that is more important—but less obvious—than most people realize.

Thus, I quite agree with Sabini et al. that embarrassment is a prevalent and meaningful influence on social life, but I think that it is only part of a much bigger phenomenon, one with more reach and power than most social scientists appreciate. Does the operation of the need to belong in such specific guises as a fear of embarrassment undermine classic concepts such as the fundamental attribution error? Yes and no. To the extent that people have an potent, inborn need to belong, a considerable amount of social behavior springs from this internal motive, but it is a motive that experimenters rarely study. Moreover, the need is probably often quiescent—for instance, when we are absorbed in private tasks—only to spring to life when the situation changes and evaluative others enter the room. Allocating causal influence unambiguously in instances like these—in which personal predispositions are activated by situational variables—is a daunting task. Worse, a given behavior can probably be entirely due to self-presentational motives on one occasion but be due to dissonance or some other intrapsychic source on another occasion (Baumeister & Tice, 1986). I suspect that we scientists are sometimes like the participants in actor/observer studies, who attribute importance to the influences we happen to notice at the time (Storms, 1973), and different theorists may form different attributions for the same event.

So, I'm not quite sure where to situate along an internal-external dimension a universal human motive that is entirely social and that depends on the presence of others. For that reason, I'll leave to other commentators a more definitive analysis of Sabini et al.'s contentions regarding the really fundamental attribution error. For me, the primary value of their article is that it alerts us again—or for the first time—to the interpersonal motives that underlie so much social behavior. Embarrassment is not the central construct they make it out to be, but it is the "tip of a very important iceberg" that has enormous influence on the currents of social life.

Note

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On Perceiving Multiple Causes and Inferring Multiple Internal Attributes

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The Milgram (1974) experiments are among social psychologists' most prized possessions because they reveal something surprising: They suggest that when people are given instructions by an authority figure to hurt someone, people are likely to deliver the punishment. The traditional social psychological account of these findings (e.g., Ross, 1977) is that behavior is externally caused to a greater extent than laypeople realize. It follows that laypersons who assume that behavior is internally driven (by dispositions) are mistaken. But are they? Do people really err by attributing to internal attributes, and if so, what is the nature of this error?

The debate about whether lay perceivers are in error surfaced 20 years ago (Harvey, Town, & Yarkin, 1981; Reeder, 1982). The debate endured, in part, because it proved to be difficult to identify a suitable criterion for establishing error. Potential criteria include asking perceivers to make predictions about a target person's behavior and comparing perceivers' attributed attitudes to the actual attitudes of a target person. When these criteria have been employed, evidence for error has emerged. Reeder, Fletcher, and Furman (1989), for example, had perceivers stand by while a member of their group was selected randomly to write an essay opposing the sale of alcohol to minors. Despite having firsthand knowledge of the constraints on the writer, watching the writer compose her essay, and listening as the writer read her essay aloud, perceivers subsequently overestimated the extremity of the writer's personal attitude (in the direction of the assigned essay).

In their target article, Sabini, Siepmann, and Stein (this issue) acknowledge that perceivers are often in error. But their acknowledgement comes with a twist: The error is not the one that social psychologists typically embrace. Sabini et al. object to the notion that perceivers fall into error when they attribute behavior to internal forces. Following Lewin (1936), they make a logical argument that behavior is a function of both personal and environmental causes. Sabini et al. suggest that perceivers do tend to err, however, by attributing to the "wrong" internal causes. For example, where the Milgram (1974) results are concerned, perceivers tend to focus on dispositions related to morality (having a conscience that prevents one from unjustifiably harming others), when other dispositions (such as a disposition to obey authority or to avoid embarrassment) are of greater importance. The impli-

cation, then, is that multiple attributes within a target person must be considered in order to understand the person's behavior.

It is important to note the precise way in which Sabini et al. extended the simple internal-external distinction of traditional attribution theory. First, their analysis implies that behavior is caused by both internal factors and external factors. Second, they imply that internal (dispositional) factors are multifaceted, rather than singular. The remainder of this commentary considers if lay perceivers hold a view similar to that of Sabini et al. That is, do lay perceivers also view behavior as caused by both internal and external forces? In addition, to what extent and with what effect do laypersons consider multiple attributes within a target person?

The empirical evidence suggests that perceivers often view behavior as multiply determined (McClure, 1998). Studies relevant to the fundamental attribution error (FAE) found that even when perceivers indicate full awareness of constraining situational forces (Johnson, Jemmott, & Pettigrew, 1984) and recognize that constrained behavior is relatively uninformative (Vonk, 1999), perceivers still attribute a correspondent disposition to the target. The point about multiple causality is, perhaps, most obvious where attributions about ability are concerned (Reeder, 1997; Reeder & Fulks, 1980). Consider that Balzac, an unrepentant spendthrift, wrote many of his works under financial duress (Boorstin, 1992). Yet the presence of (external) financial pressure would hardly preclude our attributing a variety of traits to the writer, including a genius for prose. It makes the most sense to assume that Balzac's writing required both financial incentive and genius. The presence of external forces also seems not to detract from dispositional inferences about certain instances of immoral behavior (Reeder & Spores, 1983). To the layperson's eye, the gang member who is paid \$10,000 to carry out a murder is a cold-blooded killer. Although we recognize the causal role of the money, it does not diminish our sense of moral outrage. In fact, the presence of the external incentive appears necessary to draw out the immoral tendency that lies within the gang member (as implied by Sabini et al.'s magnet metaphor).

The second implication of Sabini et al.'s analysis concerns multiple internal attributes. Rather than focusing on a monolithic attribution to internal causality or on an attribution to a particular dispositional charac-

teristic (such as morality in the Milgram, 1974, studies), their analysis recognizes multiple internal attributes as giving rise to behavior. This focus contrasts with that offered by the dominant contemporary account of dispositional inference (Krull & Erickson, 1995). In this account, Western perceivers tend to focus on a single attribute (such as a trait or attitude) within a target person. Inferences about this focal attribute proceed through a sequence of stages, with the earliest being relatively automatic and independent of cognitive resources. In the later stages of inference, if sufficient cognitive resources are present, inferences about the focal trait may be adjusted (or discounted) to take account of situational forces. Although this account could potentially be extended to address inferences about multiple attributes within a target person, such an extension is not currently available.

The narrow focus in attribution work is surprising, perhaps, given that the founders of person perception research, Asch and Heider, portrayed a perceiver whose impressions of others are multifaceted. Asch (1946), for example, implied that a person's sense of humor is processed with reference to the person's level of intelligence. Accordingly, the humor of a fool is qualitatively different from that of a wise person. Heider (1958) also described a perceiver who is focused on multiple attributes within the person. When judging the level of ability that is reflected in task performance, for instance, Heider suggested that perceivers should take note of the amount of effort that was expended. Thus, a person who attains a given level of performance by investing less effort than others should be attributed relatively higher ability. Heider also implied that high performance requires both effort and ability on the part of the performer. In this view of person perception, it is apparent not only that impressions of others are focused on multiple internal attributes; it is also clear also that perceivers are sensitive to the "fit" among these attributes.

Recent work in attribution has begun to investigate this multiple inference view. Following up on Heider's (1958) ideas, Reeder, Hesson-McInnis, Krohse, and Scialabba (in press) examined the role of inferred effort when perceivers made attributions about ability. Perceivers watched a videotape of a soccer player who performed either at a low level or at a high level. The soccer performance was given in the presence of situational forces that either encouraged high performance (a \$200 reward for doing well) or discouraged high performance (a \$200 reward for doing poorly). After receiving this information, perceivers were asked to list any relevant attributes that applied to the target person. Effort was the most frequently mentioned attribute, followed by ability and concerns that the target was nervous or "choked." Structured ratings indicated that inferences of effort followed a systematic pattern. Following high performance, inferences about effort

were elevated and relatively unaffected by situational forces. Thus, in agreement with Heider (1958), perceivers apparently view effort as a necessary factor for high performance. But following low performance, inferences about effort were more strongly affected by the situation (effort was perceived as greater when the situation encouraged high performance).

Inferences of ability in this study replicated a pattern observed in previous studies (Reeder, 1997; Reeder & Fulks, 1980): In the presence of high performance, inferences of ability tended to be elevated and relatively unaffected by the situational variation. In the presence of low performance, however, inferences of ability were higher when the situation encouraged low performance rather than high performance. Additional analyses suggest that inferences of ability were mediated by inferences of effort. One possible interpretation is that the situational rewards were perceived as motivating the target to exert (or withdraw) effort. Inferences about effort, in turn, were used to "adjust" inferences of ability. For example, in the low performance condition, inferences of effort and ability were negatively correlated (targets who were thought to have invested greater effort were attributed relatively less ability). In addition to making multiple inferences about the target, perceivers may attempt to integrate these inferences in a coherent manner.

The notion that perceivers attribute multiple attributes might also be relevant to the FAE. When perceivers react to a target person's constrained behavior, they may make inferences about more than one attribute within the target. For example, consider what happens when perceivers learn about a target person who followed the request of an authority figure to write an essay of a particular sort. Perceivers may form an impression of more than just the target's attitude; they also may judge the target to be obedient. This inference of obedience, in turn, may be relevant to inferences about the target's attitude or standing on other traits. In a preliminary investigation of this idea, Reeder, Vonk, and Lawrence (2000) showed participants a videotape of a student who either had some free time on her hands (Free Choice Condition) or who was instructed by her supervisor to help professors with various tasks, including moving books (No Choice Condition). Subsequently, a professor asked the student to help him move some books. When the student agreed to the request for help, she was rated as helpful, regardless of the situation. Thus, perceivers appear to have committed the FAE: Ratings of helpfulness for constrained behavior (in the No Choice Condition) were not discounted.

Did perceivers in this study ignore the situational constraints operating on the target person? An examination of inferences of obedience suggests that they did not. In particular, ratings of obedience were significantly higher within the No Choice Condition than in

the Free Choice Condition. Rather than ignoring the situation when the behavior was constrained, perceivers apparently recognized that the target's helpful behavior was a dutiful response to the supervisor's request. Consider also that obedient people may be thought to be helpful. Indeed, ratings of helpfulness and obedience were positively correlated. It is possible, therefore, that ratings of helpfulness in the No Choice Condition were elevated, in part, due to an attempt to reconcile inferences of helping with inferences of obedience.

Much work remains to be done in order to gain a full understanding of when (and with what effect) perceivers infer multiple attributes within a target person. It is worth noting, however, that this perspective is consistent with recent work on the connectionist perspective on person perception (Kunda, 1998; Read & Miller, 1993). Connectionist models propose that a constraint-satisfaction process is brought to bear on a network of inferences about a target person. In summary, this brief review of attribution literature suggests that lay perceivers tend to think along the same lines as Sabini et al. That is, not only do lay perceivers tend to view behavior as caused by both internal and external forces, they also tend to form integrated, multifaceted impressions of others.

Note

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Getting Down to Fundamentals: Lay Dispositionism and the Attributions of Psychologists

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The Sabini, Siepmann and Stein target article (this issue) I have been invited to discuss deals in large part with observations about the attribution process, observations that are associated with my name. Accordingly, let me begin by recounting a bit of personal history. Long ago, in summarizing some of my newly completed research on attributional shortcomings (Ross, 1977), I sought to distinguish the particular biases that had been the subject of that research from a broader and more important bias that Lewin, Heider, and other leading theoreticians had long recognized. That bias involved making dispositional attributions and inferences about behavior that was in fact dictated by immediate properties of the actor's situation, including properties that had been imposed or manipulated by wily experimenters. Without anticipating that the label rather than the phenomenon in question would be of concern to future readers, I characterized the bias in question as the *fundamental* attribution error (FAE), in order to convey the idea that the error in question pertained to the most basic or fundamental task that social perceivers face when called upon to interpret an action or outcome—that is, to decide whether the event in question reflected some distinguishing property of the particular actor or some distinguishing property of the immediate task or situation confronting the actor (or even some "interaction" between those two types of properties).

In choosing that particular descriptor, I did not imagine that the error was fundamental in the sense of being "irreducible." Indeed, I discussed a number of perceptual, cognitive, motivational, and even cultural and linguistic factors that prompted it. And I certainly did intend to suggest that social observers underestimate the impact of *all* situational influences. In fact, I explicitly noted research showing that people sometimes may be inclined to make erroneous inferences about their own and others' preferences and motives because they overestimate the impact of *external* or extrinsic incentives (Deci, 1971; Lepper, Greene, & Nisbett, 1973; Strickland, 1958)—a contention that Miller and his colleagues (Miller, 1999; Miller & Ratner, 1996, 1998) took much further in their provocative work on the "myth of self-interest." Nevertheless, I did use the term *ubiquitous* to describe the error or bias in question, largely because it seemed to me to underlie a great many phenomena of particular concern to social psychologists, including the tendency for observers to form overly quick, broad, and confident

impressions about personality and to blame the victims of trauma or deprivation. Moreover, I suggested that the layperson's failure to appreciate the impact of certain situational forces and constraints helps account for the "nonobviousness" of many of our field's most celebrated "demonstration experiments," including those noted in the current essay.

In later work, after it had become clear that my labeling of such phenomena was becoming the object of both controversy and confusion (see Nisbett & Ross, 1980; Ross & Nisbett, 1991), my colleague and I generally opted to for the term *lay dispositionism*. We also took some pains to make three points. First we emphasized that the attributional phenomena we were discussing may, to an important degree, be a reflection of individualistic or "Protestant ethic" cultures; indeed, there is mounting evidence that this bias is less apparent in the attributions and social inferences made by people living in more collectivist cultures. Second, we pointed out the dubious logical status of any attempt to offer generalizations about situational attributions in general. We can never specify the set of potential influences relevant to a single act undertaken by a single actor, much less the set of all potential influences on all actions of outcomes. Finally, and perhaps of greatest relevance to Sabini et al.'s article, we noted that the accuracy of the causal account offered for any particular behavior can never be evaluated. That is, we acknowledged that social psychologists have not somehow solved, *en passant*, the thorny problem that Aristotle, Hume, Kant, and many other philosophers have wrestled with. All that the experimental method and the statistical analyses we perform can do is to allow us to determine whether the responses of a group of individuals subjected to a particular condition or treatment have differed from some comparison or control group in a manner that allows us to reject the null hypothesis. For that reason, questions of attributional accuracy are best addressed in contexts where the object of investigation is the accuracy not of causal accounts but rather of *inferences* about the attributes of particular actors or *predictions* made about particular actions and outcomes. Both of the latter can, at least potentially, be evaluated in terms of some objective, verifiable standard.

Sabini et al., however, essentially ignore these criteria for assessing attributional performance and focus their attention on the accuracy of causal accounts. Such attributions, of course, are important, because they

help to determine how the individuals in question will respond to each other and to the opportunities and challenges that confront them. Work in health psychology, education, and various domains of personal well-being and achievement has shown that exploration of the consequences of personal versus situational attributions may rank as one of the most important applied contributions of our field. But, for the reasons I have indicated and for some other reasons that Sabini et al. articulate quite convincingly, such causal accounts are a poor target for investigations of accuracy.

Let me turn, at last, to the specifics of Sabini et al.'s provocative, and I think ultimately useful, essay. First (as my earlier remarks suggest), I take little issue with the authors' arguments about the lack of conceptual clarity attending the term *fundamental attribution error*. By any sensible account, all behavior reflects the joint operation of, or interaction between, whatever stimuli impinge on the organism and whatever innate characteristics of the organism (or residue of its learning) dispose that organism to respond to those particular stimuli in that particular fashion. Consider the following two statements: (a) John gave the poorly dressed woman a dollar because she looked hungry; and (b) John gave the poorly dressed woman a dollar because she pointed a revolver at him. Both statements suggest both a situational factor influencing John and some disposition or motive on his part. Yet the first statement suggests that the person making the statement has inferred (and is trying to convey) something more about John than that he responds to a particular strong situational force in the same manner as most of us would, whereas the second statement does not suggest this.

This example, I trust, serves to illustrate why I was left somewhat unsatisfied by the target article's account of the role of "motives" and the internal-external distinction it seems to entail. In considering the classic Milgram (1963) experiment, Sabini et al. suggest that sophisticated and unsophisticated readers alike failed to anticipate the experiment's results because they underestimated the impact of the subjects' motive to obey the experimenter, to avoid the embarrassment of confronting him about the immorality of his request, or both. My contention, backed by some data, is that the relevant failure hinged on underestimation of the impact of the particular features of the situation that commanded obedience. We would get a clearer idea of the relevant error, however, if we ask observers of the Milgram drama (a) what they just learned about the distinguishing characteristics of any single obedient participant, (b) how that participant would respond in some other very different situations involving obedience, and (c) how the next n subjects would respond to the Milgram situation. (See Bierbrauer, 1979, for relevant data.) I find it useful to describe the attributional failure as an underappreciation of the extent to which

the observed subject in particular, and people in general, can be made to deliver painful shocks to a victim because of fiendishly clever arrangements in the Milgram situation (see Ross, 1988). If Sabini et al. prefer to say that people underappreciate how motivated people are to respond to those particular situational features in that particular way, my objection would be more aesthetic than scientific. If, however, they want to conclude that people are inclined to underestimate some broad cross-situational tendency for individuals to be obedient to authority, "in general," then I invite them to collect a little data comparing predictions with outcomes or comparing inferences about dispositions with objective measures of those dispositions.

Detailed consideration of the other studies that Sabini et al. cite and analyze in terms of the impact of internal motives versus external situational factors would lead to the same conclusion. We can certainly cite motives that play a role in leading people to respond to particular manipulations in particular ways. But, the relevant results seem nonobvious not because we underestimate the impact of the general motive in question but, rather, because we underestimate the capacity of particular situational factors to evoke that motive. This is true even in the case of the motivational theory that did the most to hone the experimental skill of a generation of social psychologists, that is, dissonance theory. What is surprising to readers of those classic studies is not that people rationalize and justify their behavior or that they alter attitudes and beliefs that conflict with other attitudes and beliefs that they hold with greater conviction and motivation. Rather, it is the specifics of the studies involved: the fact that the particular manipulations employed could do the job and create the relevant changes. We are not surprised that con artists, environmental polluters, or political leaders who order bombing of civilian targets tend to adopt beliefs that allow them to rationalize the harm that they do to others. Nor are we surprised that smokers continue to rationalize the harm that they do to themselves. But every new generation of psychology students is surprised to find out that the particular manipulations employed by the artful Festingerians (such as providing a small payment rather than a large one to participants who lie to a peer or inducing participants to undergo a severe initiation rather than a mild one) can make people change their perceptions and evaluations.

Beyond quibbling about the best way to describe the purported bias, I think that Sabini et al. are quite right to insist that researchers and theorists be more specific in generalizing about erroneous dispositional attributions. That is, we must specify the types of situational factors whose impact is likely to be underestimated. I think the authors also are on the right track when they give special attention to the factors involved in threat to face, social embarrassment, and the like. (In fact, I

would have liked to see some data documenting such influence.) The list of factors likely to be underestimated can be expanded quite a bit, however, on the basis of existing research. And the suggestion that the term *really fundamental*, or at least *really important*, should be reserved for influences or errors involving the type of social motives and sources of discomfort that Sabini has explored so thoughtfully in his earlier work is (to say the least) peculiar. First of all, many classic experiments on the power and subtlety of situational influence do not involve interpersonal factors at all. Rather, they involve so-called *channel factors*, that is, factors that facilitate or impede the link between attitudes, beliefs, values, preferences, intentions, and so forth and corresponding actions (see Ross & Nisbett, 1991). An obvious example is the famous Darley and Batson (1973) "Good Samaritan" study. Another is the study by Leventhal, Singer, and Jones (1965) on the effects of channel facilitation on medical compliance. (Channel factors also play an obvious role in the Milgram situation (Ross, 1988), in that the absence of a well-defined and salient channel for effective disobedience was arguably an essential factor in producing the high levels of obedience that were obtained.)

Furthermore, a lot of research in the "situationist" tradition involves interpersonal factors that could hardly be termed embarrassment or face-saving. Cialdini (1993) catalogued many of these, and most social psychologists could quickly generate their own list, including modeling effects (e.g., Bandura, Ross, & Ross, 1961) and social labeling (e.g., Miller, Brickman, & Bolen, 1975; Rosenthal & Jacobsen, 1968). The failure of observers to adequately appreciate the impact of role advantages and constraints (e.g., Ross, Amabile, & Steinmetz, 1977) and the broader tendency to separate actors from their roles when we assume that we "know" what our teachers, doctors, ministers, coaches, and politicians "really are like" (see Nisbett & Ross, 1980) is also notable. And it seems unlikely that either the actors or the observers in this situation are governed by motives of the sort emphasized by Sabini et al. Other classic studies, such as Freedman and Fraser's (1966) foot-in-the-door study, may involve processes more closely related to those described by Sabini et al., although the best term for this broader class of processes remains elusive. (Agreeing to put up a big ugly lawn sign in response to a request, a couple of weeks after having agreed to put a little sign in the window for another person, presumably has something to do with a desire to feel consistent and positive about oneself. But it is difficult to see why the earlier compliance would make a later refusal any more "embarrassing.")

Again, I think it is notable how many demonstrations exist wherein people are induced to do things by situational manipulations that most observers would think insufficiently powerful to do the job. Moreover, I

am confident that individuals who observed participants in these studies would make inaccurate attributions and predictions about the actor(s) whose responses they witness (although a little data on the subject would be a lot more useful than my confident assertion). But I can't think of any single term or unifying principle to characterize the entire set of influences that we have exploited in such studies.

It is tempting to close this discussion with an acknowledgment that, notwithstanding my various reservations, the target article offers some valuable insights about important issues (which it certainly does). Instead, let me issue a challenge to Sabini et al., and to any other colleagues who feel that social psychologists have been too bold in their claims about the power of situational forces and constraints, about the often important underestimation of that power by social observers, or both. In the course of this review, I cite several classic experiments (and the list could easily be expanded) in which particular situations or particular manipulations have led research participants to behave in ways that surprise most readers. Moreover, the observer of any particular participant in such studies would be apt to make erroneous inferences about the actors' dispositions and erroneous predictions about how each actor would behave in new and different situations. What I challenge my colleagues to do is to cite comparable "dispositionist classics," that is, studies in which some measure of individual differences accounted for much more variance than lay or expert observers had predicted or would have imagined. (Even better would be studies wherein it was also shown that seemingly powerful situational factors failed to exert their predicted influence when they "completed" with the relevant individual difference factor.)

Let me emphasize that I am not denying the importance or magnitude of individual differences. There are many contexts in which we could designate a particular person who predictably would behave in an unusual or extreme fashion. (We would not have expected Mother Teresa or Malcolm X to act just like everyone else, much less like each other, when asked to "turn the other cheek.") But, I maintain, in such cases, few laypersons would have underestimated the role that the relevant individual differences might play or claimed erroneously that it would be more important to know the situation than to know the actor in making an accurate behavioral prediction. Nor, I suggest, would observers generally be too timid in inferring individual differences when they happened to see a particular individual behave in particular extreme way in a particular situation.

In any case, I would love to see the authors' list of "dispositionist classics" that they believe would rival the "situationist classics" that can so readily be cited. What I have in mind, for example, are studies in which people who showed themselves to be high on Scale X,

or on Trait Y, or even on Behavioral Measure Z, proved (on average) to be even more extreme on some set of other measures than the casual observer would ever have predicted. There may well be some findings or studies that the authors could cite. But I will be mightily surprised if they can come up with anything that has inspired and provoked us in the way that Milgram and company did. And, lest my contentions here be misunderstood, let me make it clear again that I am not making claims about the relative power of dispositions versus situations or about the value of social psychology versus personality psychology. My claim is one about the shortcomings of lay psychologists, not researchers or theorists in particular subdisciplines of our field.

Discussion about systematic biases in our views on the causes and implications of human behavior, of course, should not be restricted to the arena of social psychology experiments. As Lewin recognized a half century ago, it is the tendency for us to underestimate the present or potential impact of situational pressures and constraints—big and small, blatant and subtle—that leads us to deal inappropriately and ineffectually with so many of the critical problems that confront our society.

Note

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