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## AUTHORS' RESPONSE

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### Perspectives on Personality and Social Psychology: Books Waiting to Be Written

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We feel quite honored to have our book reviewed by four such distinguished psychologists as Daryl Bem, Baruch Fischhoff, Lewis Goldberg, and Mark Snyder. We also learned a great deal from the reviews. Each of the reviewers, while appreciating at least some aspects of our book, has mused about the way it could, and perhaps should, have been written. In our response, we highlight these alternative texts. We suspect that the reader will agree with our reviewers, and with us, that the interface of the fields of cognitive, social, and personality psychology holds the promise of some excellent books waiting to be written by the right people.

Fischhoff, a cognitive psychologist with social leanings (or at least many friends and admirers in the field of social psychology), has chosen to see our book in the context of his subdiscipline. He views it essentially as a cognitive "take" on the field of social psychology, which of course in part it is. Once viewed that way, however, it may be found wanting; for it is not the sort of book a card-carrying cognitive psychologist could be expected to write. Notably, it is short, to say the least, on formal definitions and models. As Fischhoff observes, although we refer repeatedly to the overconfidence literature (i.e., to work showing that people place more stock in their social judgments than they have any right to), we fail to review literature that tackles the thorny problem of defining overconfidence. Nor do we specify the various types and sources of miscalibration, something Einhorn and Hogarth (1978) did with considerable precision in discussing the confidence that employees place in their hiring decisions.

Fischhoff's objection here is well taken, as is his complaint that we repeatedly fail even to mention some of the large bodies of work in the judgment tradition that are relevant to social judgment, let alone give some real flavor for the theory and precise models possible. A book that did these things, that actually attempted to derive as much social psychology as possible from cognitive psychology, would in our view be a great contribution. But it is one that we did not try (and are not ideally equipped) to write. To produce such a book would require not only a substantial knowledge of social psychology but a deep understanding of cognitive psychology, especially current work in judgment and decision-making. We nominate . . . Baruch Fischhoff!

Two of the reviewers, namely, Mark Snyder and Daryl Bem, are personality-social psychologists, or, to be more precise, psychologists trained in the social tradition who now

reside with equal comfort in the field of personality. Like Fischhoff's, their essays are laudatory, although not for the same reasons. Where he sees our book as a cognitivist's tour of social psychology, they see it, or at least the portions of it emphasized in their reviews, as a social psychologist's tour of personality. They also have some complaints, more mildly expressed perhaps than felt. A good social psychologist still, Snyder acknowledges the power of situations to affect behavior; but he insists that the importance of personality comes primarily from its influence on the situations people choose to put themselves in. Thus people behave as they do, he maintains, substantially because of the situations they find themselves in, but they find themselves in those situations substantially because of their enduring dispositions. In the situationist-chicken-versus-dispositionist-egg dispute, therefore, Snyder generally comes down on the side of the egg, and he gently chides us for our reluctance to do likewise. Again, there is some justice in the complaint. Although, as Snyder acknowledges, we offered examples of the way that dispositions restrict and determine situational choices (examples inspired largely by Snyder's own work), we gave this process less emphasis than we might have. The reason we did not spend more time on it is the paucity of hard evidence on the degree to which people actually do choose their situations as a function of more or less enduring or stable dispositions. One line of relevant evidence that we did cite in the text is by Caspi, Bem, and Elder (1989), who showed that ill-tempered boys have poorer employment outcomes as adults, to a substantial extent because they choose to drop out of school at a higher rate than do their better tempered peers. At the same time, it seems clear that such "choices" owe much to the hostility of the environment these youths have created, and must continue to face. We would certainly not be surprised to find that life outcomes are often the result of situation choices. We would be even less surprised to find out that such outcomes generally are the result of more complicated Person  $\times$  Situation interactions. But we do not believe that psychologists presently have the goods to assess the magnitude of such effects—either in absolute terms or relative to expert and lay convictions about their magnitude.

A second reason for our reticence in pushing the view that dispositions drive choices is that we are far from sure just how much qualification of unbridled situationism is required by the view. The large, extremely difficult-to-answer ques-

tion is, "How much scope do people really have to select their situations?" School children have little choice about the situations they confront. They sit all day in classes not of their choosing taught by teachers not of their choosing in schools not of their choosing. On the playground they face the prospect of being told what game to play, not being picked for the team, being assaulted by an angry classmate, and so on. Many people—factory workers, homemakers, even corporation executives—might feel with some justice that their own scope of choice is similarly limited. And we suspect that people who live in inner-city ghettos, to say nothing of those in other cultures who face even greater constraints of role or economic privation, would be similarly unimpressed by the argument that they have chosen the environments that shaped their beliefs and behavioral dispositions.

But we may be wrong. There may be better evidence and better arguments than we are aware of. If so, there is a book, or at least a very important review article waiting to be written by a Person  $\times$  Situation interactionist of Mark Snyder's caliber.

Bem goes further in his complaints about our stance. He agrees with Snyder (and with us) that people pick their own situations to some degree and that this provides for very real continuity and stability in the behavior of the people around us. He then goes on to insist that the layperson is accurate about both the degree of stability and its origin in terms of situation choice and predispositions to construe the world in particular ways. Bem even purports to know the layperson's metatheory of personality.

We review our friend's behavior and then select descriptors that strike us as pertinent precisely because they seem to conform to the patterning of his or her behavior. . . . our lay dispositional conception of personality is not a nomothetic trait theory but a context-sensitive, idiographic *type* theory. . . . The context-sensitivity of our intuitive approach to personality not only finesses the inconsistency problem but also automatically solves the problem of situational specificity that can so easily embarrass a nomothetic trait theory. . . . And if our initial descriptive attempt encounters empirical difficulties . . . we can further modify or qualify the type description . . .

Now if people did this, they would, as Bem says, be right for the *right* reasons about the degree of consistency in the social world around them rather than, as we hold, right for the *wrong* reasons. The problem is that there is not a shred of evidence for Bem's view that the layperson is a circumspect idiographic theorist, whereas there is an armload of evidence, a chapter load of evidence (chap. 5 to be precise), that the layperson is an unregenerate, 1938-style, no-holds-barred, nomothetic personologist who shows not the slightest interest in details of the situation before making overconfident, disposition-based predictions about behavior. Ten years ago we ourselves thought it was distinctly possible that the lay theory of personality might be more idiographic than nomothetic, and much of the research presented in chapter 5 was conducted by us in the role of honest brokers—just trying to find out what the layperson's theory of personality is. But our research left little doubt that people make predictions about behavior based on one observation of behavior that would be justified only if the correlations across situa-

tions were in the range of .80 instead of the range of .10 to .20. Moreover, they show virtually no modification of their predictions by virtue of knowing that large situational factors have changed from one situation to another. And they are confident that they can predict long-range behavior in unknown contexts from a brief interview or other small sample of behavior in a limited context.

What has Bem to say about this evidence? He insists that people are being tricked into playing the standard nomothetic game ("given a person, specify how he or she will behave in some particular situation") or the nomothetic personnel-selection game ("specify the person who will behave in a particular way in this particular situation"). But what makes these games unfair? We try to perform such judgmental tasks every day. "How will my child behave if I do this; if her teacher does that; or if we move her from school X to school Y." "Which of my employees is most likely to succeed or fail at this particular task, and exactly how likely are such successes or failures?" Notably, our subjects do not regard these sorts of judgments as unfair. Indeed, one sometimes senses a degree of impatience in subjects when one tries, in the course of research, to provide them with much in the way of situational detail. They *know* how their roommate behaves at parties and other social situations, and they don't want us to waste their time with irrelevant details about the particular party situation.

Of course, Bem may yet be able to prove that people would make impeccably calibrated judgments with his preferred "idiographic game" ("given a person, specify the situations in which he or she will behave distinctively and what those distinctive behaviors will be"). But to say that we doubt it is to seriously understate our lack of confidence in the layperson's accuracy about the predictability of everyday life. The book showing that the layperson is a thoughtful, restrained idiographic personality theorist is one, in our view, that is *not* going to be written—even by the ever-persuasive Daryl Bem.

Finally, Lewis Goldberg, who is a personality theorist with a substantial knowledge of mainstream social psychology. To say that he is not enamored of our book would scarcely capture the passion with which he expresses his views. (Nor his sarcastic humor, which is often very funny indeed. We must admit that we could stop ourselves from laughing at several points in his review only by reminding ourselves that we were the targets of his barbs!)

Goldberg is mad, make no mistake. Unlike Snyder and Bem, who take us at our word that the book is intended as an olive branch from social psychologists to personality psychologists, Goldberg perceives our book as an antipersonality tract (an "olive branch full of thorns"). He states that, "In chapters 2 through 5, a series of arguments are advanced to prove that [personality dispositions] don't exist." This is a remarkable characterization. Chapter 2 presents the evidence on the power and subtlety of situational influences on behavior. This could be perceived as inherently an antidispositionist argument only if we were making the claim that situations customarily account for nearly all the variance in behavior—a claim which, to our knowledge, not even the most rabid situationist has ever made. Chapter 3 presents evidence that the construal of situations is crucial to determining their effects on behavior and that small situational details, along with factors best characterized as *chance*, can often affect construals in big ways. Far from

being antidispositionist, we explicitly acknowledge that there are stable individual differences in tendencies to construe situations in particular ways, and, following Kelly, Mischel, Bem, Cantor and Kihlstrom, and others, we endorse the view that personality differences are often best understood in terms of such construal differences. Chapter 4 presents evidence not that dispositions “don’t exist” but that they are weak in absolute terms provided that the test is a reasonably “fair” one (i.e., one in which the different actors face essentially the same situations, in a context where their responses are not constrained by roles, reputations, commitments, or the expectations of those around them). Behavior in discrete situations can be predicted from behavior in other discrete situations at correlation levels of only around .10. Behavior in discrete situations can be predicted by personality tests or by aggregated prior behavioral base rates usually only in the vicinity of .20 to .30, with the maximum cross-validated predictions for any single behavior, by any amount of additional evidence, being in the vicinity of .4. Chapter 5 shows that laypeople believe they are capable of far greater accuracy in prediction than this and thus are far more dispositionist in their theories than the data allow them to be.

Goldberg inexplicably, and repeatedly, reads this latter comparative point as an absolute one. For example, he quotes our claim that “there are no famous studies in which stable personal attributes, either as measured by the investigator or as revealed in the record of past behavior, have proved to be markedly better predictors of behavior than academicians or even laypeople had anticipated” (p. 95), and then offers the following retort:

Phooley! One does not have to venture out of classical social psychology to find superb “dispositionist demonstrations” of the sort that are not supposed to exist: In the domain of attitudes, the stable personality attribute of right-wing authoritarianism (RWA) as measured by Altemeyer’s (1981, 1988) scale regularly produces correlations in the .40 to .50 range against a wide variety of relevant criterion indices, including mean shock level in the Milgram paradigm . . .

(Three other examples of work with respectable correlation levels are also presented.) This response, of course, does not address our claim that there do not exist “stable personal attributes” that are “markedly better predictors than academicians or even laypeople had anticipated.” To know that, one would have to know what laypeople or academicians would *predict* about the relevant correlations. And there is certainly good reason to believe that both groups, as they have been repeatedly shown to do with other dispositional variables, would overestimate the relevant correlations. Recall that subjects think that behavior in a single situation can be predicted by behavior in another single situation (not by virtue of knowing scores on an entire personality scale, as in Altemeyer’s work) extremely well, at a level that could be obtained only if the correlations were in the region of .8. They would almost surely be more disappointed than impressed by correlations in the range cited by Goldberg.

If we erred in the balance of our text (and, given Goldberg’s response, it seems that we did), it was in the failure to clarify the thrust of our assertion about the absence of research showing lay overestimation of behavioral consistency or “the power of the *person*.” The problem, we should have emphasized more clearly, has little to do with personality

researchers, who have done a great deal of valuable work in developing useful personality assessment instruments and uncovering nontrivial consistencies in behavior across situations and over the life span. It lies primarily with the observer who expects and sometimes even perceives far more consistency than even the most skilled personologist can demonstrate.

A possibly related failure to distinguish between the absolute and the comparative issue of effect size is evident in Goldberg’s description of an article by Funder and Ozer (1983), which he chides us for not citing. Goldberg states that these authors “systematically compared the effect sizes elicited in some classic experiments in social psychology with those achieved in studies of individual differences, and concluded that the two types of effect were about the same size.”

In fact Funder and Ozer did not systematically compare anything. What they did was to show that the average effect size in a few selected classics was equivalent to a correlation magnitude of about .4. That correlation magnitude is about the maximum ever obtained in cross-validated studies predicting behavior in specific situations by dispositional variables, and is substantially higher than the .2 to .3 generally reported in even the best known studies that predict individual behaviors from measures of dispositions. Furthermore, as Funder and Ozer themselves acknowledged, there are of course many studies showing situational-effects strength levels far stronger than that level. We describe many such studies in our book.

But most crucially, the point is not the relative magnitudes of person versus situation effects but rather the magnitude of each relative to our shared beliefs and perceptions. Although it is simply indisputable that situational effects can be readily obtained that are far greater than any dispositional effects that have ever been reported, this fact is of relatively little scientific interest in itself, and we certainly didn’t intend to imply that social psychology is somehow more valuable or important than personality psychology by virtue of it.

For what it is worth, it would be trivial to specify situation “manipulations” that account for virtually 100% of response variance (e.g., the apparent presence or absence of a weapon in the possession of the young man requesting that you hand over your wallet) and almost as trivial to specify “person” differences that would do likewise (e.g., the civil-rights votes of Senator Helms vs. Senator Kennedy). Neither of these “effects” is particularly likely to shock anyone’s intuitions, or to provide the basis for a textbook classic. On the other hand, the fact that virtually *no one* would, or did, predict the levels of destructive obedience obtained in the basic Milgram situation (and not the magnitude of the *between-condition differences* cited by Funder and Ozer) is what made Milgram’s study a situationist classic for the ages.

Again, the essential point is that laypeople (or at any rate Western laypeople) show one type of estimation error consistently: They believe that dispositional effects are far larger than they are. In addition, they are badly calibrated with respect to situational effects, underestimating the power of some situational factors of particular interest to social psychologists (and, as we noted in our text, overestimating the power of some situational factors).

These points are particularly well made by one of the studies reviewed by Funder and Ozer. They note that the

situational effect of being in a hurry on helping a victim in the Darley and Batson (1973) study is equivalent to a correlation of about .4. But Pietromonaco and Nisbett (1982) showed that college students thought that this variable would make no difference at all. In contrast, they thought that an individual difference variable studied by Darley and Batson (nature of religious orientation) would make a great deal of difference. In fact, that variable accounted for essentially none of the variance in behavior.

Our book after all was intended in good part as a text for college students. We believe that modern social science has no more fundamental or important message for students than that their basic approach to predicting behavior is flawed. They are far too dispositionist and far too little attuned to some of the situational variables that have been demonstrated to be important.

Goldberg appears to believe that our discussions of effect magnitude were a matter of subdiscipline aggrandizement. His anger is hard to understand otherwise. We are truly sorry not to have been clearer on this point. On reflection, we see that we could have emphasized the positive achievements of individual difference research far more than we did. Such a strategy might have avoided professional resentments such as Goldberg's. Indeed, at various points in writing our book we actually thought of writing a true personality-social textbook—one that presented the major findings of both disciplines and integrated them theoretically. That proved to be beyond our ability—in part because, at least in our view, the most important findings about situations and construals demonstrated to date do not mesh well with the most interesting and powerful individual difference findings. (A perhaps revealing exception is the very satisfying story that one can tell linking social psychological work on the importance of situa-

tional elements that give people the feeling of choice with individual difference work on the importance of dispositional beliefs about the degree of one's control over events.) The person who finds a way to knit the two fields together in a clear and convincing way will have written a very important book indeed. If our esteemed colleague Lewis Goldberg cannot accept the relevant chapters of our text as an olive branch, perhaps he will accept them as a challenge to write that book.

### Notes

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