Towards a balanced social psychology: Causes, consequences, and cures for the problem-seeking approach to social behavior and cognition

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Abstract: Mainstream social psychology focuses on how people characteristically violate norms of action through social misbehaviors such as conformity with false majority judgments, destructive obedience, and failures to help those in need. Likewise, they are seen to violate norms of reasoning through cognitive errors such as misuse of social information, self-enhancement, and an over-readiness to attribute dispositional characteristics. The causes of this negative research emphasis include the apparent informativeness of norm violation, the status of good behavior and judgment as unconfirmable null hypotheses, and the allure of counter-intuitive findings. The shortcomings of this orientation include frequently erroneous imputations of error, findings of mutually contradictory errors, incoherent interpretations of error, an inability to explain the sources of behavioral or cognitive achievement, and the inhibition of generalized theory. Possible remedies include increased attention to the complete range of behavior and judgmental accomplishment, analytic reforms emphasizing effect sizes and Bayesian inference, and a theoretical paradigm able to account for both the sources of accomplishment and of error. A more balanced social psychology would yield not only a more positive view of human nature, but also an improved understanding of the bases of good behavior and accurate judgment, coherent explanations of occasional lapses, and theoretically grounded suggestions for improvement.

Keywords: Bayesian inference; biases; normative models; personality; positive psychology; rationality; reasoning; social behavior; social judgment; social psychology

> *Odious behavior ("sin") is at the heart of our most powerful research in social psychology.*
> (Elliot Aronson 1999a, p. 104)
>
> *How could people be so wrong?*
> (Lee Ross & Richard Nisbett 1991, p. 139)
>
> *Oops, I did it again.*
> (Britney Spears 2001)

1. Introduction

Although everyday social behavior and cognition includes both appalling lapses and impressive accomplishments, mainstream social psychology has for decades emphasized the negative side of this equation. A prevalent research strategy has been to propose a prescriptive norm for social behavior or cognition and then to demonstrate that human performance falls short of it. Using this strategy, some of the most influential studies of social behavior documented conformity with false group judgments, obedience to malevolent authority, and failure to help those in need. Studies of social cognition showed how – among numerous other shortcomings – people misuse social information, perceive themselves erroneously, and are too quick to attribute attitudes and personality traits to others. The selective demonstration of negative phenomena is further compounded by the message that people's intuitions regarding social behavior and cognition are also flawed. For example, people are said to believe that others, but not they themselves, are prone to bias (Friedrich 1996; Pronin et al. 2002). Some investigators have begun to revive interest in human strengths (Seligman & Csikszentmihalyi 2000; Sheldon & King 2001; Snyder & Lopez 2002) and cognitive accomplishments (Gigerenzer et al. 1999; Klein et al. 2002), but so far their influence on social psychology has been limited.

The purpose of the present article is to examine some of the causes and consequences of the prevalent negative research orientation and to sketch analytical and theoretical routes leading to a more balanced social psychology. The time for reform is ripe because the historically rooted paradigm of uncovering ever more behavioral and cognitive flaws may be approaching a dead end. It is becoming progressively less informative as it continues to proliferate, causing human strengths and cognitive skills to be underestimated and impairing the development of theory.

The persistent emphasis on the negative is problematic because research designed to uncover misbehavior or cognitive failures is sure to find some. Without efforts to also examine behavioral strengths and cognitive successes, a distorted view of human nature emerges that yields a cynical outlook on human nature rather than usable guidance for behavior and judgment. It is doubtful, for example, that people could function effectively if they refrained from all obedience, intervened in all apparent crisis situations, discarded judgmental heuristics, or suspended judgment altogether; yet, that is what research demonstrating human shortcomings in each of these domains would seem to recommend.

Studies of bad behavior and flawed reasoning often settle for rather simple demonstrations. The empirical section of the typical article shows that people can be induced to do something objectionable or to think in a way they should not. The discussion section may contain some speculation of how many social problems must be due to this tendency, and a call may be placed for research on how to reduce its prevalence. The analysis generally stops there, short of asking *why* such a behavioral or cognitive tendency exists, or what general purpose it might serve. As a result, the development of integrative theory and sensible advice is stymied (Katzko 2002).

The situation is reminiscent of the early days of vision research. When visual illusions were first discovered, they were considered mistakes produced by arbitrary design flaws (Gregory 1971). An early interpretation of the Müller-Lyer illusion, for example, was that it reflects a general tendency to overestimate acute angles and to underestimate obtuse ones. Then, in 1896, psychologist A. Thiery proposed that this and other illusions reflect processes that permit accurate perception in real-life contexts. Today, optical illusions are no longer seen as failures of the visual system, and airline pilots are not taught that the Müller-Lyer and Ponzo illusions pose threats to their performance. In contrast, the pre-1896 view still dominates social-cognitive psychology. Behaviors and judgments that violate experimenter-imposed norms are interpreted as revealing flawed psychological processes that need to be fixed (Funder 1987).

The current state of social psychology has parallels in biomedical research, which is often based on problem-finding and indeed may be funded on the basis of the problem it seeks to alleviate. The search for a cure for a particular disease has popular and even political appeal. But ultimately, it is the systematic, theory-based research of basic physiology that explains how the human body usually functions well, and also how it malfunctions under certain conditions (Fields 1994; Skalka 1993). In a parallel manner, basic, theory-driven research on social psychological processes will most fully illuminate the peculiar shortcomings *and* the adaptive successes of the social animal.

2. Negativity in social psychology

Two traditions, a classic behavioral one and a more recent cognitive one, characterize the history of social psychology. The emphasis of both has been disproportionately negative.

2.1. Social behavior

The most remarkable fact about social behavior, according to the mainstream view, is how often it violates normative standards of conduct. In the words of one eminent researcher, "odious behavior ('sin') is at the heart of our most powerful research" (Aronson 1999a, p. 104). Historically, the concern with the odious began with analyses of human behavior in crowds (Le Bon 1895). With the possible exception of Floyd Allport (1924), the founders of social psychology worried that men (and women) could only be trusted to behave properly when left to their own devices, and that the social influence of the group would transform them into irrational, suggestible, and emotional brutes (see Asch 1952 for a review and critique).

In the 1950s and 1960s, a number of laboratory studies cemented the view that social influence has nefarious consequences on otherwise rational individuals. These studies demonstrated conformity with bizarre group behavior, obedience to destructive authority, and apathy among the bystanders of an emergency. Yielding to social influence was tantamount to violating behavioral norms of independence and empathy. Even research addressing seemingly positive aspects of human nature, such as interpersonal attraction, or neutral topics, such as attitude change, focused on the negative. One of the most widely cited studies of human attraction concluded that superficial cues of physical attractiveness overwhelmed cues to other personal qualities that people claim they value (Walster et al. 1966). The basic theme of attitude change research, whether from the cognitive dissonance tradition or the competing self-perception approach, has been that people are typically unaware of the degree to which their attitudes come from rational-

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ization rather than from rational thought (Aronson 1969; Bem 1972). But these conclusions are only implicitly negative. As we now illustrate, some of the most influential studies of social behavior and cognition have been explicitly interpreted as demonstrating surprising flaws in human nature.

2.1.1. Conformity. Solomon Asch (1956) pitted naïve participants against a unanimous group of confederates who, on occasion, rendered bizarre judgments concerning the relative lengths of lines. This situation included considerable social pressures to conform, but no incentives to resist. In the maximum-impact experiment, 90% of the participants gave two or more incorrect responses and about one-third of all responses were false. Conformity meant that participants violated the normative expectation that they should honor their own perceptions and be able to tolerate disagreement with others. Although Asch was also interested in – and empirically demonstrated – processes that allow resistance, the story of conformity carried the day (Friend et al. 1990).

2.1.2. Obedience. Stanley Milgram (1974) led his participants to violate a norm of good behavior in particularly dramatic fashion. Believing they were assisting in a learning experiment, the participants faced an experimenter who relentlessly ordered them to deliver ever-increasing electric shocks to a faltering confederate. Overall, about 40% of the participants administered what they must have thought were lethal voltages. This was a surprising finding on the assumption that ordinary people would not hurt innocent others even when ordered to do so. A panel of psychiatrists predicted that only the rare sociopath would inflict mortal harm. Like Asch, Milgram went on to do under-appreciated work that demonstrated how various situational variables, such as the distance between participant, victim, and experimenter, caused the compliance rate to vary.[1]

2.1.3. Bystander intervention. Inspired by the infamous murder of Kitty Genovese, a series of experiments by John Darley and his colleagues showed that people would fail to intervene in an emergency inasmuch as other witnesses were present (Darley & Latané 1968) and inasmuch as they were under time pressure (Darley & Batson 1973). Overall, about half the participants intervened,[2] although normatively all of them were supposed to. Quoting from Luke (10:29–37), Darley and Batson noted that the behavior of their research participants fell short of the example set by the Good Samaritan.

2.2. Sources of negativity

In each case, the aspect of the results that aroused the most interest was not the power of the situation per se, but the power of particular situations to elicit bad behavior. However, the same studies could also be construed as equivalently revealing the sources of nonconformity, independence, and helping. Asch, Milgram, and Darley and Latané showed that variations in the setting, such as the presence of allies or being held individually accountable, increased the prevalence of normative behavior, and many participants acted normatively even in the maximum-impact conditions. But this variation was seldom emphasized in the reviews and texts that made these studies famous. In 1997, a

segment of NBC's *Dateline* featured a re-enactment of the Asch experiment and excerpts from Milgram's obedience film, but made no mention of any of the moderator variables. Instead, the broadcast began with episodes from *Candid Camera* showing how strange situations can elicit strange behavior (e.g., the person who faces the back of the elevator because everyone else does). A spirited on-line discussion sponsored by the *Society of Personality and Social Psychology* did not reveal any concerns about this biased presentation. Instead, one commentator warned that the power of the situation to elicit compliance had not been emphasized enough.

The emphasis on negative outcomes leaves the powerful impression that the underlying psychological processes must be intrinsically maladaptive. Just as in visual perception, however, process and outcome are separate issues. Processes that can produce bad behavior in particular circumstances may yield desirable or adaptive results in other circumstances. The judgments of others can be informative (Deutsch & Gerard 1955), obedience to legitimate authority is important for social order (Hogan et al. 1994), and hesitation to get involved in someone else's struggle may save one's own neck. A more balanced recognition of the costs and benefits of conformity, obedience, intervention and other seemingly problematic behaviors would not only be more realistic, it would also ask that theories explain the range of human behavior, not just the negative end.

So why is the emphasis so unbalanced? At the most general level, it seems that negativity itself is a powerful motivator. Social psychological research has documented numerous ways in which negative information commands resources of perception, attention, and memory in ways that positive information cannot (Baumeister et al. 2001; Rozin & Royzman 2001). If the dominance of the negative evolved as an adaptation to life in uncertain and potentially hazardous environment, it may, in part, explain the negative bent of social research. This idea cannot explain, however, why other fields within psychology have a more positive outlook. Much developmental work, for example, is motivated by the search for capabilities among infants that no one expected they had. To move past the somewhat tautological idea that social-psychological research tends to be negative because of negativity bias, we consider four specific sources of this bias.

2.2.1. Zero-tolerance norms. In the classic studies, the frequency of misbehavior was considered to be surprisingly high even if it was limited to a minority of participants. But what is the smallest number sufficient to trigger surprise? Because the strict view that "nobody will be induced to behave badly" is too easily refuted, data analysis commonly proceeds along probabilistic lines. For example, an investigator might lay a confidence interval around the observed percentage of violations. As sample sizes increase, shrinking confidence intervals ultimately exclude zero. At that point, norm violations are considered established even if most participants acted properly. Consider the stock finding of ethnocentrism in laboratory groups. In the classic study, Henri Tajfel found that most participants distributed rewards equitably among ingroup and outgroup members. Only when the allocation matrix made fairness impossible did a significant number of participants reward outgroupers less than ingroupers (Tajfel et al. 1971). This finding led to the widely accepted conclusion that people discriminate

against outgroups without sufficient justification (but see Gaertner & Insko 2000 for a recent challenge of this view).

2.2.2. Informativeness of norm violation. Norm violations stand out as figures before a ground, just as they stand out statistically as signals against a background of noise. Because it is the expected behavior almost by definition, norm adherence does not demand explanation, and may even seem boring (Jones & Davis 1965). If people had behaved as they should, Asch's experimental design would have appeared ludicrous, Milgram's colleagues would have felt vindicated, Darley and Latané's research would have confirmed that people live according to scriptural precepts, and few readers would have heard of any of these studies. But that is not what happened. Instead, classic social psychology garnered great attention by exposing expectations of normative behavior as naïve.

Note that on purely numerical grounds, a persistent emphasis on norm violation ought to be self-eliminating. As demonstrations pile up, their surprise value should dissipate as the counter-norm becomes the new norm. This does not appear to have happened, probably because most of the social norms that are invoked are anchored not merely in statistical expectations, but in moral or ethical precepts.

2.2.3. Appeal of counterintuitive findings. Ordinary people know a great deal about human behavior, and this knowledge has helped to identify basic psychological principles (Kelley 1992). Nevertheless, as findings based on commonsense hypotheses risk being dismissed as "bubba psychology" (McGuire 1997), "psychology has often embraced counter-CS [i.e., common-sense] data as knowledge" (Kluger & Tikochinksy 2001, p. 411). Pressures to refute common sense arise from both inside and outside the field. From the inside, findings consistent with intuitive expectations seem uninteresting. From the outside, this view is reinforced by those who claim that they "always knew that." Senator William Proxmire once offered a "golden fleece" award to federally supported psychologists who obtained results he considered obvious. In contrast, demonstrations of norm violation are protected from ridicule, and may even gain a cachet of urgency and truth. To report that more people conform, obey, and fail to intervene than even one's grandmother (i.e., "bubba") would have expected, is an effective rebuttal to all those laypeople who feel they understand behavior as well as trained social psychologists do.

But some recent reviews question the robustness of counter-intuitive findings (Kluger & Tikochinsky 2001). A longstanding staple in the cupboard of counterintuitive knowledge has been that one's confidence in judgment has no relation to one's accuracy. However, recent research has shown that under realistic circumstances (e.g., when observational circumstances are varied), the confidence of eyewitnesses is quite closely related to their accuracy ($r = .59$; Lindsay et al. 1998; see also McCullough 2002). This is just one example. Kluger and Tikochinsky (2001) reported nine other cases in which an accepted counter-intuitive finding was reversed. Of these, eight lay within the areas of social and personality psychology.

2.2.4. Usefulness to society. Following Lewin (1948), many social scientists are concerned with the practical relevance of their findings (Aronson 1999a; Redding 2001). The goal of curing social ills requires first that a social prob-

lem be identified. Then, the critical undesirable behavior needs to be reproduced in the laboratory. Finally, the empirical demonstration can be interpreted as reflecting a typical – and thus potentially dangerous – human liability. At times, this sequence is followed by efforts to retrain individuals in order to alleviate their diagnosed flaws.

2.3. Social cognition

Over the last three decades, the cognitive reorientation of social psychology has shifted attention away from social behavior and towards social perception and judgment. Initially, normative models were believed to characterize how people ought to make inferences as well as how they actually do make inferences (Kelley's 1967; Peterson & Beach 1967). By 1980, this optimistic view had been displaced by a focus on inferential shortcomings and errors (Kahneman et al. 1982; Nisbett & Ross 1980). This emphasis continues today (Gilovich et al. 2002; Myers, 2002), and it has penetrated the literature on the application of psychological science to areas such as medicine, counseling, and management (Bazerman 2001; Heath et al. 1994).

The shift towards the negative followed a similar development in the field of judgment and decision making (Mellers et al. 1998), which, in turn, was stimulated by a series of articles by Tversky and Kahneman (e.g., Tversky & Kahneman 1974). Kahneman and Tversky (1973) challenged the axiomatic status of rationality in economic theories of choice, and social psychologists soon saw the potential of this new paradigm for the study of social cognition. Dawes (1976) reviewed the historical context for the psychological approach to irrationality. From Aristotle to the Catholic Church to Sigmund Freud, he argued, irrational thought and behavior was viewed as the result of capricious emotional forces disrupting the workings of an otherwise rational mind. In contrast, the modern view holds that the presumably rational apparatus of conscious thought is itself fraught with deficiencies. This is an even more depressing verdict than the traditional one. If conscious capacities cannot be counted on to detect and correct creeping errors, what can?

Ross (1977) took the next step by recasting the history of social psychology from the perspective of the heuristics-and-biases paradigm. He argued that the classic studies of social misbehavior gained their significance from the inability of everyday observers to predict their outcomes and to understand their implications. This inability, he suggested, betrayed a profound failure to think scientifically. The enthronement of the scientific method – as social psychological investigators understood it at the time – as the gold standard of good reasoning was a crucial step. It provided an uncompromising norm for human judgment, much as strict ethical demands had served as standards of behavior. But the metaphor of the intuitive scientist elevated the human mind only to denigrate it. And it had to be so because without certain key biases, "social psychology's most beloved phenomena would not have occurred and its most beloved experiments would have been mere platitudes" (Gilbert 1998, p. 130). Thus, a merger was achieved that tied the psychology of misbehavior to the psychology of flawed thinking.

Once this view was established, debates focused primarily on *which* of various negative metaphors explains the social perceiver's failings best. Following Dawes (1976), some

favored the metaphor of the "cognitive miser" by emphasizing limited mental resources, reliance on irrelevant cues, and the difficulties of effortful correction (Wilson & Brekke 1994). Others preferred the "totalitarian ego" metaphor (Greenwald 1980) to emphasize needs for self-esteem and control, as well as the self-deception necessary for the satisfaction of these needs (Ehrlinger & Dunning 2003). Despite their differences, both the cognitive and the motivational approach viewed distortions and errors as the fundamental and most informative aspects of social cognition. Whereas the early theorists regarded individual rationality as a haven from the irrationality of the group and a protector against collective excesses, the current view leaves little room for refuge. To illustrate, we now turn to three of the most widely studied biases.[3]

2.3.1. False consensus. In the study of perceptions of social groups, the preponderant bias became known as the false consensus effect (FCE). The FCE is understood as a projective tendency in which self-referent information serves as a judgmental anchor, from which other-referent or group-referent properties are inferred. In the paradigmatic study, undergraduates decided whether to assist in a study of communication by walking around campus wearing a sandwich board that read "Eat at Joe's," and then were asked to estimate how many other students would agree to wear the sign (Ross et al. 1977). The implicit model for the social perceiver was that of an ideal scientist who would discard idiosyncratic perceptions and appraise sample data with the cold eye of objectivity. To match this ideal, a social perceiver would need to ignore his or her own paltry contribution to the population average and base consensus estimates only on observations drawn from sufficiently large and unbiased samples (which happened to be unavailable to the participants in this study). To detect bias, investigators did what naïve social perceivers are incapable of doing. Comparing the average consensus estimate offered by compliant participants with the average estimate by noncompliant participants, they found a statistically significant difference.

2.3.2. Self-enhancement. When people evaluate themselves, they rely in part on comparisons with others. The bias of self-enhancement is said to occur when people think they are better than the average person. In the paradigmatic study, participants rated positive traits as more descriptive of themselves than of most others (Brown 1986). The verdict that this bias was ethically unwarranted stemmed from a comparison of the findings with the humanistic ideal that well-adjusted people feel as favorably about others as they do about themselves (Rogers 1961). The verdict that the bias violated a norm of rationality stemmed from the assumption that people should be able to make correct estimates of their relative standing in the population. It was further assumed that when self-descriptions differ from descriptions of the average other, the former must be wrong.[4]

2.3.3. The fundamental attribution error. According to attribution theory, people try to explain behavior by looking for its causes in the person, in the situation, or in both (Heider 1958). Research on the "fundamental attribution error" (FAE)[5] maintains that they characteristically fail at this task by overestimating the importance of properties of the person. In the paradigmatic study, participants read essays that

either favored or opposed the regime of Fidel Castro (Jones & Harris 1967). Some participants learned that the authors of the essays had freely chosen which position to take, whereas others were told that the authors were assigned their positions. When participants estimated the true attitudes of the essay-writers, they concluded that those with free choice were more likely to believe what they said than were those who were coerced. This finding effectively demonstrated the "discounting principle," which demands that a potential cause for a behavior is weakened to the extent that other plausible causes emerge (Kelley 1972). It was a different result, however, that captured the attention of a generation of researchers. Even in the coerced condition, participants attributed more pro-Castro attitudes to writers of pro-Castro essays than to writers of anti-Castro essays. The inference was much weaker than it was in the noncoerced condition,[6] but it remained significantly larger than zero. Again, the normative ideal violated was that of an idealized social scientist, who, in this case, would completely refrain from dispositional inferences once an effective situational cause had been found.

2.4. Sources of negativity

A striking indication of social psychology's preoccupation with the negative is the sheer number of published errors. Table 1 presents a partial list of errors reported over the past few years.[7] Some of these refer to the same or a nearly identical phenomenon; others share the same label, although they refer to different phenomena (the "jingle-jangle" effect, Block 1995); and still others are contradictory. For now, suffice it to note their number and variety. Just as God has been said to have an inordinate fondness for beetles, having made so many (Evans 1996), social psychologists may have an inordinate fondness for errors, having found so many.

Table 1. *Some errors of judgment identified and labeled by social psychologists*

Overconfidence bias	Correspondence bias
Fundamental attribution error	Halo effect
False consensus effect	False uniqueness effect
Positivity bias	Negativity bias
Confirmation bias	Disconfirmation bias
Justice bias	Male bias
Hot hand fallacy	Gambler's fallacy
Self-protective similarity bias	Hindsight bias
Self-serving bias	"Ultimate" self-serving bias
Optimistic bias	Pessimistic bias
Sinister attribution error	Conjunction fallacy
Ingroup/outgroup bias	Positive outcome bias
Hypothesis-testing bias	Diagnosticity bias
Durability bias	Vulnerability bias
Self-image bias	Labeling bias
Observer bias	External agency illusion
Systematic distortion effect	Intensity bias
Asymmetric insight illusion	Just world bias
Dispositional bias	Romantic bias
Clouded judgment effect	Bias blind spot
Empathy neglect	Empathy gaps

Note: Partial list of major topics of studies published since 1985.

Most of these errors are presented as lamentable shortcomings signaling dangerous flaws in the system (Funder 1992).[8] Tversky and Kahneman (1971) characterized human judgment as "ludicrous" (p. 109), "indefensible" (p. 108), "self-defeating" (p. 107), and guilty of "a multitude of sins" (p. 110). Ross and Nisbett (1991) described the typical person as "oblivious" (p. 124) and "insensitive" (p. 82), as well as beset by "ignorance"(p. 69), "general misconceptions," and a "whole range" of other "shortcomings and biases" (p. 86). The only question left, it seemed, was "How could people be so wrong?" (p. 139).

This condemnationist orientation heuristically equates bias with inaccuracy, and ignores the long-range outcomes of presumably non-normative judgments. Like the presumed social misbehaviors, however, many social-cognitive biases yield considerable benefits. Social projection increases the accuracy of social perception (Kenny & Acitelli 2001; Krueger 1998b) and satisfaction with a partner (Murray et al. 2002). Positive self-concepts not only are their own hedonic rewards, but they also tend to increase the accuracy of self-judgments whenever the attribute in question is distributed with a negative skew (Krueger 1998a). For example, relatively few people are deathly ill; most are in the range from medium to very good health, and these people will have health "scores" higher than the arithmetic mean.[9] Drawing dispositional inferences even from situationally constrained behavior, often interpreted as a manifestation of the "fundamental attribution error," may be a sign of the perceiver's own social competence and adjustment (Block & Funder 1986). In sum, it appears that many social-perceptual biases signal the operation of an adaptive system of social perception much like certain visual illusions reveal the efficiency of the visual system under realistic circumstances (Funder 1987). Again we need to ask, why is the emphasis of social research so negative?

2.4.1. Norm violation, usefulness, and counter-intuitiveness. Some of the reasons for the negative tone of research on social cognition parallel the ones considered in the context of social behavior, including the apparent informativeness of norm violations, the desire to alleviate social problems, and the appeal of the counterintuitive. When judgments consistent with a norm of rationality are considered uninformative, only irrationality is newsworthy. Assuming that social problems can be traced to poor thinking (Jones & Roelofsma 2000), many researchers seek to identify "systematic irrationalities" (Stanovich & West 2000, p. 646) and ways to eliminate them (Baron 1998). The appeal of counter-intuitive findings is even stronger in the area of social cognition than in the area of social behavior. As one writer put it, "Mistakes are fun! Errors in judgment make humorous anecdotes, but good performance does not. It is fun to lean back in our chairs and chuckle about our goofs" (Crandall 1984, p. 1499).

This rhetoric of irrationality created the perception of a deep crisis in human cognition that could only be overcome if people learned to set aside heuristics and reason as normative models prescribe (Lopes 1991). The rhetoric continues even though some investigators maintain that they never meant to impugn the capabilities of human judgment in the first place (Kahneman 2000; Ross & Nisbett 1991).

2.4.2. Rationality as a null hypothesis. Much like earlier research on violations of behavioral norms, research on cog-

nitive biases has been beholden to the methodological ideal of experimentation as the *camino real* to causal inferences (Gigerenzer 1996a; Krueger 1998c; Rozin 2001). As part of this ideal, social-cognitive research inherited the analytical tool kit of null hypothesis significance testing (NHST). Whereas NHST can be used to detect causal relationships, its task in bias research often is merely to detect the presence of a significant difference between the average judgment and a normative standard. Thus, NHST is used in its weak, confirmatory form. Being identified with a complete lack of a difference, rationality at best remains a null hypothesis that has failed to be rejected.

As sample size increases, the precision of measurement is improved, more robust statistical tests are employed, and ever-smaller effect sizes pass the threshold of significance (Kirk 1996; Wilcox 1998). In some cases, this allows biases to reach significance level even when the modal response is identical with the demands of the normative model.[10] The dichotomous decision rule of NHST – a bias either has been demonstrated, or it has not (yet) been demonstrated – leaves no room for bounded, or good-enough rationality, nor does it distinguish between biased and unbiased individuals. As the boundary between rationality and irrationality dissolves, any opportunity to learn how many respondents got it right is lost.

When efforts to detect bias fail, nothing positive can be said about the presence of rationality because the null hypothesis not only represents rational judgment but also chance variation (Gigerenzer & Goldstein 1996). As the negation of causation, chance can neither be produced nor explained (Hayes 2001). Therefore, psychological mechanisms are more readily invoked to explain bias than to explain the absence of bias. With no direct way of explaining accuracy, the absence of bias, when it occurs, might even be explained by the mutual cancellation of opposite errors (see Epley et al. 2002 for a case of a co-occurrence of social projection, i.e., the "spotlight effect" and the FAE).

3. Problems with the negative emphasis

The view that people characteristically violate norms of good behavior and rational thought raises two further problems. First, some of the imputations of misbehavior and error are themselves difficult to justify, and second, the problem-seeking approach tends to be atheoretical. The lack of attention to behavioral and judgmental accomplishments not only prevents understanding of adaptive behavior or accurate judgment, but it also retards a full understanding of the sources of the misbehaviors and errors when they do occur.

3.1. Rash imputations of error

The imputation of irrationality should demand a high standard of proof. The average human, no less than the common criminal, deserves to be considered innocent until proven guilty. More importantly, a false imputation of incorrect thinking threatens the validity of subsequent empirical or theoretical analysis. The typical lack of such a high standard opens the door to incoherent findings and contradictory conclusions.

3.1.1. Contradictory errors. In many studies of social judgment, the null hypothesis of rationality is sandwiched be-
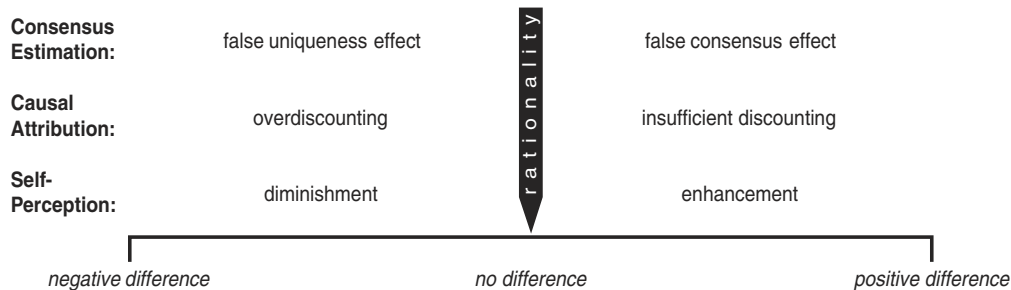
Figure 1.   Pointed rationality between ranging biases.

tween opposite biases. Consider the three paradigmatic areas of judgment. As shown in Figure 1, consensus estimates are unbiased only if they do not covary with the perceivers' own responses. Most studies show projective bias, but scattered reports of false uniqueness raise the unsettling possibility that opposite biases might co-exist (Krueger 2000a). In self-perception, people are typically found to self-enhance, but there are also reports of self-diminishment (John & Robins 1994; Yik et al. 1998). In attribution studies, the correspondence bias (or FAE) is the standard finding, but its inverse, the insufficient discounting of situational causes, has also been found. When the experimental design demands that the situational cause be discounted, participants overestimate its effect (Krull & Dill 1996; Quattrone 1982).

The co-existence of contradictory biases is not limited to the three paradigmatic areas of social judgment. Intuitive predictions have been found both to overstate and understate the probability that past events will recur. Belief in "the hot hand in basketball" exemplifies the former finding (Gilovich et al. 1985), whereas "the gambler's fallacy" exemplifies the latter (Keren & Lewis 1994). Similarly, many studies show that people neglect base rates when making predictions (Bar-Hillel 1980; Kahneman & Tversky 1973), whereas others suggest that they use them too much (Edwards 1982). Such contradictions can escape notice when opposite biases are presented as part of different topics using different terminologies. Textbook chapters on social cognition maintain that people make faulty predictions by relying too much on specific case information while underusing category (i.e., base rate) information. Chapters on intergroup relations maintain the opposite, namely that people overuse their categorical (i.e., stereotypic) beliefs while neglecting individuating information (Funder 1995b).

Opposite biases can even emerge in the *same* study. When this happens, *ad hoc* assumptions may take the place of theoretical integration. Kruger and Dunning (1999) reported that participants who scored low on a test of ability grossly overestimated how well they did relative to other test takers. In contrast, participants who scored high on the test slightly underestimated their relative standing. Kruger and Dunning dealt with this apparent inconsistency by attributing each error to a distinct cognitive failure. Poor performers, they argued, overestimated their ability because they lacked the meta-cognitive insight into their own weaknesses. They were "unskilled and unaware of it." The opposite bias displayed by the high performers was attributed to their falling "prey to the false consensus effect" (p. 1126) – one bias to which the unskilled were apparently immune.

When the rituals of NHST are suspended, it is no longer necessary to interpret all observed differences between estimates and normative values as distinct biases with correspondingly distinct mechanisms. Instead, the asymmetry in estimation errors can be explained by regression to the mean in conjunction with an overall, group-level, better-than-average effect. Estimated and actual performance can be expected to be positively but imperfectly correlated – hence regression – and overall, people can be expected to be optimistic rather than pessimistic – hence asymmetric "errors" (Krueger & Mueller 2002; see also Ackerman et al. 2002).

The debate over the putative "unskilled-and-unaware" effect was but a replay of an earlier controversy over asymmetric errors in consensus estimation. Meta-analyzing studies on consensus bias, Mullen and Hu (1988) noticed that people holding minority attributes grossly overestimated the prevalence of these attributes, whereas people holding majority attributes slightly underestimated the prevalence of majority attributes. Again, it was not necessary to associate different errors with different flaws of thinking (as Mullen and Hu did). Simply assuming that most people expect to be in the majority and noting that estimates are inevitably imperfect, a regression model replicated the pattern of asymmetric errors (Krueger & Clement 1997). Over the past decade, several other biases have been reinterpreted as the result of such random imperfection and regression. Among them are such core phenomena as over- (and under-) confidence (Klayman et al. 1999) and illusory correlations (Fiedler & Armbruster 1994).

As soon as one asks whether changes in one bias may result in changes in others, one moves towards a more comprehensive model. Since Floyd Allport's original exposition (Allport 1924), people have been charged both with "pluralistic ignorance" and social projection. Pluralistic ignorance reflects the perception of a difference between the self and the group. It is said to occur when people in general underestimate the prevalence of a certain (usually socially desirable) attitude. In contrast, social projection (or the FCE) reflects a perception of similarity between the self and the group. It is said to occur when those who hold a certain attitude believe it to be more common than those who do not hold it.

Studying attitudes toward alcohol consumption, Prentice and Miller (1993) found that on average, college students felt that others were more permissive toward drinking than they themselves were (pluralistic ignorance). At the same time, those who expressed a less permissive attitude thought

there was less permissiveness on campus than did those students whose own attitudes were more permissive (social projection). Prentice and Miller deflected the idea that these two errors might be opposites by noting that both can co-occur empirically. Indeed they can, because pluralistic ignorance is the difference between the true prevalence of an attitude and the mean estimate of that prevalence, whereas projection is the correlation between estimates and people's own individual attitudes. It is easily shown that as projection increases, pluralistic ignorance decreases (Krueger 2002). Once again, projection is more beneficial than irrational.

Another example involves the relationship between social projection and self-enhancement. When self-enhancement is construed as the "better-than-average effect," it is easily misinterpreted as the opposite of projection, that is, as a "false-uniqueness effect" (Fiske et al. 1998; Markus & Kitayama 1991). Whereas seeing oneself as different from the average suggests a psychological contrast (which need not be a false one; Krueger 1998a), social projection suggests assimilation. Again, however, a closer look at the units of analysis dissolves the paradox. Whereas self-enhancement is a mean-level effect, social projection is a correlational effect. For an individual judgment item, both effects tend to emerge, but they are negatively related across items. The more people assume others to be similar, the harder it is to feel superior. In this case, social projection serves as a brake against narcissistic over-evaluation of the self (Krueger 2000b).

3.1.2. Wrong or misapplied norms. Some putative demonstrations of error are themselves erroneous because the norm against which behavior or judgment is compared is incomplete, wrong, or misapplied. In each of the three paradigmatic social-judgment tasks, the norm of zero difference can no longer be taken for granted. As we have seen, social predictions and self-perceptions would be respectively less accurate if people ignored their own responses and if they rated themselves as being average on negatively skewed attributes. If they attributed coerced behavior entirely to the situation, they would concede that experimenters always manage to secure compliance from their participants, arguably an overgeneralization (Morris & Larrick 1995).

Research on the classic base-rate integration problem yields a similar conclusion. Participants who seemingly fail to produce a Bayesian probability estimate may be doing rather well if one assumes that they approach the task as a matter of signal detection (Birnbaum 1983). In this as in many other cases, the evaluation of performance depends on which of several plausible normative standards is brought to bear. And why should human performance be asked to measure up against the particular normative standard to which an experimenter happens to subscribe? As Nozick (1996) noted, "theorists of rationality have been intent upon formulating the one correct and complete set of principles to be applied unreservedly in all decision situations. But they have not yet reached this – at any rate, we do not have complete confidence that they have" (p. 46). Perhaps it is more sensible to ask whether research participants fail (or succeed) on their own terms (Ayton 2000; Moldoveanu & Langer 2002). Such an empowerment of the participants implies, of course, that "normative theories will be drained of all their evaluative force" (Stanovich & West 2000, p. 655). At any rate, this proposal does not mean

that judgments cannot be evaluated. Instead, multiple norms may need to be considered, and an effort should be made to understand which one best represents the meaning and goals of the participants.

3.1.3. Incoherent explanations of misbehavior and error. A minimum requirement for rational judgment is to avoid outright contradictions (Dawes 1998; Krueger 2000a). Because the coherence criterion is such a powerful device, it ought to be applied to explanations of misbehavior and bias, too. When this is done, we find that many accounts of human judgment lack the very coherence they demand of naïve research participants. With regard to social projection, Dawes (2000) observed that "it is not the research subjects or intuitive experts that reach an irrational conclusion, but the psychological theorists analyzing them" (p. 134). In the following, we pursue Dawes' argument to examine three prominent accounts of the FAE, which deserves close inspection because of its flagship status as the self-proclaimed "fundamental" flaw of social intuition.

3.1.3.1. Logical incoherence. Recall that the classic demonstrations of non-normative behavior aimed to show that situational forces overwhelm people's intentions or dispositions to behave responsibly (e.g., Conner 2000). At the same time, however, their surrender to situational pressures has been taken to indicate negative dispositions, such as lack of autonomy, empathy, or ethical fiber. If people conform with patently false judgments, obey malevolent masters, and fail to help those in need, they might also readily accept anti-democratic values, ignore the suffering of others, and participate in genocide. When evil becomes banal, there must be something wrong with people. Inferences of this kind amount to precisely the kind of dispositional attributions in which untutored people are said to overindulge.

The most influential account of the FAE rests on Heider's (1958) classic distinction between internal and external sources of behavior. It assumes that

> the human skin [is] a special boundary that separates one set of "causal forces" from another. On the sunny side of the epidermis are the external or situational forces that press inward upon the person, and on the meaty side are the internal or personal forces that exert pressure outward. (Gilbert & Malone 1995, p. 21)

This version of attribution theory assumes that behavioral causation is a zero-sum game. Dispositional causes must be discounted when situational causes are shown to be effective. It follows that perceivers are mistaken to appeal to dispositional causes when a change in the situation explains the observed behaviors. What are the implications of this hydraulic person-situation model for the researchers' own chain of inference? To preserve coherence, they need to argue that perceivers' dispositional judgments were elicited (and thus caused) by specific experimental conditions. If so, the conclusion that perceivers' dispositional inferences were reflections of their own disposition to commit the FAE would be an expression of the very error it is meant to explain. This paradox is so delicious that it deserves to be savored like a fine Merlot. To claim that only situational effects are real while bemoaning participants' dispositional lack of insight into this important truth is incoherent. *The FAE cannot be discovered unless investigators, by their own criteria, commit it themselves!*

The hydraulic model does not do justice to the dynamic interplay of personal and situational variables (Sabini et al. 2001). Lewin (1951) famously observed that behavior is a function of the person *and* the situation, which specifically means it does not have to be one or the other. The Milgram setting, for example, can be understood as a situation in which two opposing external forces interacted with two opposing internal forces. The implacable experimenter provided a situational force towards obedience, whereas the complaining victim provided a situational force towards compassion. At the same time, the individual's disposition toward obedience was opposed by whatever disposition he or she had towards compassion (Sabini & Silver 1983). When observers predicted less obedience than Milgram obtained, they not only underestimated the experimenter's situational influence, they also *over*estimated the victim's situational influence. It is also correct to observe that observers overestimated the dispositional tendency towards compassion at the same time that they *under*estimated the dispositional tendency towards obedience. The equivalence of these two superficially different accounts underlines how behavior makes little sense without the interplay of both situation and disposition (Wright & Mischel 1987).[11]

3.1.3.2. Statistical incoherence. The second interpretation of the FAE was developed in an effort to sidestep the difficulty of justifying the a priori separation of situational and dispositional causes. According to this statistical account, behavior can be assumed to be under situational control when behavioral variance is low. When the variance is high, it is assumed to be under dispositional control (Jones & Davis 1965; Kelley 1967). Thus, a religious service is a more powerful situation than the opening of a department store. In the former, nearly everyone does as required (e.g., pray), whereas in the latter, many people do not do as hoped (e.g., shop; Snyder & Ickes 1985). Note that this interpretation still presupposes a hydraulic model because it conceives of situations and dispositions as competing accounts of behavioral variance (Ross 1977).

Even as the statistical argument has become accepted as a "logical standard [that] does not seem . . . to have any serious competition" (Gilbert 1998, p. 135), its implications for the FAE have been overlooked. Indeed, the statistical criterion *reverses* the standard interpretation of situational power (Sabini et al. 2001). Because many participants disobeyed Milgram's experimenter, the situation bore a closer resemblance to the opening of a department store than to a prayer service. When estimating that hardly anyone could be induced to administer severe shocks, psychiatrists predicted the opposite, namely that the situation would have an extremely strong effect by evoking uniform independence. Thus, they *over*estimated the power of the situation by underestimating interpersonal variation. Predictions regarding behavior in the Asch conformity experiment or in Darley and Latané's studies on bystander apathy can be interpreted in a parallel manner. Because it so often implies that people overestimate instead of underestimate situational forces, the statistical interpretation of the FAE is incoherent.

3.1.3.3. Empirical incoherence. The third argument refers to the presumably large effect sizes obtained from situational variations. These effects are often portrayed as much larger than the effects of individual differences in personality, a difference that naïve perceivers presumably fail to appreciate. This argument also presupposes a hydraulic causal model. If, for example, a personality variable correlates .40 with a behavioral outcome, it is frequently assumed that the situation must explain the remaining 84% of the variance (Kunda & Nisbett 1986; Mischel 1984). Because there is no well-developed taxonomy of situations or accepted set of situational variables, variance is assigned to situations by default – they get whatever is left after the effects of personality variables are accounted for (Funder 2001a). But it is as plausible to assign variance not explained by any particular personality variable to other personality variables that were not measured, as it is to assign it to situational variables that were also not measured (Ahadi & Diener 1989). Despite the rhetoric about the "power of the situation," very little is known about the basis of that power or its real amount.

It is not even clear that the effects of situational variation are greater than the effects of dispositional variation. When Funder and Ozer (1983) recalculated the effect sizes for situational variables such as the distance of the experimenter and the victim in the Milgram experiment, the number of bystanders and degree of hurry in the studies on bystander intervention, and the level of incentive offered to induce attitude change through cognitive dissonance, they found correlations ranging between .30 and .40. These values were similar to the correlations typically found between individuals' behaviors across different situations (Funder 1999; Funder & Colvin 1991) and the notorious "personality coefficient" that situationists consider to be the upper limit for the effect of personality on behavior (Mischel 1968; Nisbett 1980). In a sophisticated analysis, Kenny et al. (2001) compared person, situation, and interaction effects directly, and found the person effect to be the largest. Thus, the third account of the FAE violates the coherence criterion in that it relies on empirical data that either do not support or that reverse the commonly assumed direction of the error.

3.2. Theoretical shortcomings

An exclusive focus on norm violations discourages cumulative research and theoretical development. Misbehaviors and errors tend to be viewed in isolation; they have narrower implications than is often assumed; and they do not contribute much to theories of the whole range of behavior.

3.2.1. Isolation. To the extent that behavioral social psychology becomes the study of misbehavior, and cognitive social psychology becomes the study of judgmental shortcomings, the field is reduced to a catalog of things people do badly. Each misbehavior generates its own explanation, but these explanations are seldom integrated, much less drawn from broader theories of behavior or cognition. Many of the errors listed in Table 1 are associated with particular groups of investigators or even single psychologists. This isolation facilitates a profusion of overlapping labels, it allows the discovery and survival of mutually contradictory errors, and it discourages the development of overarching theory (Kruglanski 2001). In Asch's (1987) words, "the current expansion [comes with] a shrinking of vision, an expansion of surface rather than depth" (p. x).

3.2.2. Limited implications. Errors in judgment are studied because of the belief, often explicitly expressed, that they have important implications for evaluating human reasoning. But some errors reveal little more than the difficulty of the presented task (Funder 2000). People who are good at solving problems on the Scholastic Aptitude Test (the SAT-1, which is highly saturated with conventional IQ) are also good at solving many of the standard problems of judgment and decision making (Stanovich & West 2000). Indeed, many problems used in heuristics-and-biases studies would be suitable for use as SAT items because they correlate as well with total scores as do individual SAT items themselves.

Consider the implications of this psychometric finding. To detect differences among highly able test takers, the Educational Testing Service (ETS) has written many difficult SAT items without claiming to uncover systematic and discrete cognitive deficits (cf. Stanovich & West 2000, p. 646). By the standards of heuristics-and-biases research, however, each difficult SAT item should merit a separate article presenting the discovery of a cognitive flaw.

3.2.3. Incompleteness. Because findings of error are seen as demanding of explanation, whereas rationality may merely be assumed, theoretical explanations of error do not even seek to explain the entire range of performance. They concern the participants who get the answer wrong, not the ones who get it right. One way to overcome this limitation is to examine the relationship between bias and accuracy. Often this relationship is positive. Projected consensus estimates (Hoch 1987), self-enhancement (Krueger & Mueller 2002), and overattribution (Block & Funder 1986) benefit the perceiver most of the time. These examples are no exceptions. The hindsight effect (Hoffrage et al. 2000), the positive testing strategy (Klayman & Ha 1987; Oaksford & Chater 1994), the halo effect (Borman 1975), overconfidence (Dawes & Mulford 1996; Erev et al. 1994), and various heuristics in probability estimation (McKenzie 1994; Moldoveanu & Langer 2002) have similar advantages.

The idea that judgmental biases serve adaptive functions vindicates Egon Brunswik's (1956) view that social perception operates through a lens of probabilistically valid cues and probabilistically correct use of these cues. By and large, cues are valid enough and perceivers use them well enough to achieve a fair degree of judgmental accuracy. Brunswik's approach distinguishes between adaptive errors and harmful mistakes (see Funder 1987 for details on this distinction). As noted earlier, visual illusions are also erroneous interpretations of experimental reality, but they reveal underlying mechanisms of the visual system that yield accurate and adaptive results under most ecologically representative conditions (e.g., Vecera et al. 2002). If these illusions were eliminated from human perception, perceptual accuracy would surely get worse, not better.

A frequent charge is that people "over- or under-apply particular rules or use shortcut 'heuristics' instead of relying on normative rules" (Jacobs & Klaczynski 2002, p. 146). The explanatory power of this charge depends on whether people can be expected to know when and how to switch from a heuristic mode to a more formal mode of thinking. Often, no such meta-decision can be made without running into the paradox that the switch cannot be made without foreknowledge of the answer (Krueger et al. 2003). Suppose people know that most distributions of social (e.g.,

self-esteem) or academic (e.g., grades) characteristics are negatively skewed. The heuristic expectation of being better than average would minimize the aggregated error, although it would produce some false positives (Einhorn 1986). To avoid overgeneralization, the person would have to know by non-heuristic means on which dimensions he or she is merely average or worse. If the person effortfully recruits such knowledge for each dimension, the need to think heuristically never appears in the first place, but neither do its effort-saving advantages.

The heuristics-and-biases paradigm makes any benefits of heuristic strategies impossible to detect. When the stimulus is held constant, the data cannot show how accurate a person would be across stimuli or under more realistic circumstances. When the stimuli selected for research are limited to those for which use of the heuristic yields inaccurate results, it is tempting – and rather typical – to conclude that the heuristic represents a systematic flaw (Kühberger 2002).

When multiple stimuli are employed, statistical analysis typically focuses on bias to the exclusion of accuracy. When bias is expressed by a partial correlation between heuristic cues and judgment after the reality criterion is controlled, it is impossible to estimate how much a heuristic contributes or detracts from accuracy. All that can be said is that "all heuristics – by mathematical necessity – induce weighting biases" (Kahneman 2000, p. 683). If only the partial correlation between the bias cue and the prediction is tested for significance (with the reality criterion being controlled), the utility of the bias cue for the improvement of accuracy necessarily remains unknown.

4. Back to balance

We do not question all research on problematic behaviors or flawed reasoning. We do suggest, however, that social psychology is badly out of balance, that research on misbehavior has crowded out research on positive behaviors, that research on cognitive errors has crowded out research on the sources of cognitive accomplishment, and that the theoretical development of social psychology has become self-limiting. We now offer empirical, analytic, and theoretical recommendations to redress the current imbalance.

4.1. Empirical suggestions

4.1.1. De-emphasize negative studies. If current trends continue, new entries for Table 1 will continue to appear, and indeed several have been added since this article was first submitted. Many of these will be old biases resurfacing under new names or new biases contradicting old ones. Even to the extent that new biases are discovered, one could question what new exhibits in the Museum of Incompetence will contribute to our understanding of social inference. A slowing rate of output of error-discovery would not only stem the fragmentation of the literature, but also free journal space for studies that examine errors in the context of accomplishments and vice versa.

Not all current research is negative. A "positive psychology" movement has begun to focus research on human strengths and abilities to cope and develop (e.g., Diener & Biswas-Diener 2002; Diener & Seligman 2002; Lyubomirsky 2001). Though important, increased research on positive topics will be an insufficient remedy. A one-sided re-

search emphasis on positive behavior, perhaps complete with null hypotheses where *bad* behavior represents the null to be disconfirmed, might eventually generate problems parallel to those besetting the one-sided emphasis on negative behavior. We recommend that the *range* of behavior be studied, rather than showing that behavior is bad – or good – "more often than most people would expect."

In the area of judgment and decision making, Gigerenzer and colleagues (Gigerenzer et al. 1999) find that heuristics can "make us smart" as well as produce error. A movement in cognitive psychology, parallel in some ways to positive psychology, has also begun to question the logical and empirical bases for studying errors (Cohen 1981; Moldoveanu & Langer 2002). Some of the arguments have been "Panglossian" by suggesting that psychologists have no grounds for evaluating the judgments of their fellow humans (Cohen 1981), whereas others have suggested that certain imputations of error are themselves erroneous (Dawes 2000; Lopes 1991).

4.1.2. Study the range of behavior and cognitive performance.
As an example of a more comprehensive approach, consider Stanovich and West's (2000) study of individual differences in judgmental performance and general cognitive ability. Even smart research participants get certain problems wrong, which suggests that these were simply too difficult or perhaps even incorrectly keyed. More importantly, Stanovich and West placed both normative and counter-normative decision making in a common framework to explain when normative decisions might be expected, what psychological processes produce them, and the prescriptive status of the normative model employed.

For another example, Paul Ekman (1991/1992) examined people's ability to discriminate between spontaneous and staged nonverbal behavior. In one study, observers were better able to detect concealed emotions by attending to postural cues instead of facial expressions (Ekman & Friesen 1974). Our point is not that this work was "positive" in any particular sense, but rather that it examined the conditions under which both failures and successes occur. Other studies have shown that people can form accurate impressions on the basis of minimal information (see Hall & Bernieri 2001 for a survey). Short soundless videos suffice (Ambady et al. 1995), as do handshakes (Chaplin et al. 2000), or even a mere peek at a person's office or bedroom (Gosling et al. 2002). Studying "empathic accuracy," Ickes (1997) explored the conditions under which people can intuit the thoughts and feelings of their interaction partners. Funder (1995a) and Kenny (1994) have evaluated the accuracy of judgments of personality with criteria such as interjudge agreement and correct behavioral prediction (see also Diekman et al. 2002 for an innovative study on the accuracy of gender stereotypes).

An important property of these research programs is that they allow the possibility of accurate judgment. There is a criterion – a target is lying or not, thinking a particular thought or not, characterized by a particular trait or not – that the participants might successfully predict. This contrasts with the artificial design of many error studies where nothing true can possibly be said about the target. Consider the study of expectancy effects. In the paradigmatic (non-ecological) error study, a participant such as a teacher receives false information about the potential ability of some students. The classic result is that such false expectancies predict modest increases in test scores (Rosenthal 1994). Arguing from an ecological perspective, however, Jussim (1991) asked how expectancies typically arise, and whether their predictive utility is necessarily false in the sense of being self-fulfilling prophecies. Indeed, most teachers' expectancies are based on valid information, and the effect of erroneous expectations is comparatively small (see also Brodt & Ross 1998 for the predictive utility of stereotypic expectancies). Again, we do not merely wish to emphasize the positive conclusion, but the availability of research designs that allow participants to be correct.

4.2. Analytic suggestions

4.2.1. Handling NHST with caution. The proliferation of documented errors owes much to the ritual use of NHST. Skepticism about the value of NHST has a long history (Harlow et al. 1997), and these concerns apply a fortiori to a value-laden field such as social psychology. The method's most serious shortfall is that by misapplying modus tollens to inductive inferences, NHST misses its own ideal of rationality. According to this ideal, a null hypothesis (e.g., of rational thinking) may be rejected if the data are improbable under that hypothesis. Logically, knowing that P implies Q means that -Q implies -P. When the consequence is denied, the antecedent cannot be true. If the null hypothesis suggests that certain data are improbable, however, finding such data does not guarantee that the null hypothesis is improbable (Cohen 1994). Because knowledge of just that improbability is needed for the rejection of the hypothesis, NHST does not deliver what researchers want. It does not provide the inverse leap from data to hypothesis. As a consequence, reliance on NHST can generate contradictory claims of bias, each apparently supported by improbable data.

Because we do not expect NHST to fall out of favor, we emphasize the need to understand its limitations and to use additional data-analytic strategies. Several commentators have proposed an integration of NHST with Bayesian concepts of hypothesis evaluation (Krueger 2001; Nickerson 2000; Task Force on Statistical Inference 2000). The Bayesian approach acknowledges that data do not speak directly to the truth or falsity of a hypothesis unless there is a prior theory or expectation about the chances of the hypothesis to be true. If such expectations are specified, Bayes' Theorem gives a posterior probability for each hypothesis. The differences between prior and posterior probabilities then reflect how much has been learned from the evidence, and research becomes an incremental learning process. The following examples illustrate how the Bayesian approach combines prior expectations with significance levels to allow the estimation of the probabilities of the hypotheses in the light of the data.

4.2.2. Bayesian inferences. Consider the simplest case, in which a researcher is not able (or willing) to advance any hypothesis but the null hypothesis, H_0. The alternative hypothesis, H_1, may then be the empirically observed effect size. The probability of the data under the null hypothesis, or data more extreme, is the familiar significance level derived from the statistical test, $p(D|H_0)$. The probability of the data, or data more extreme, under the alternative hypothesis is $p(D|H_1)$. If the observed effect size stands in as the alternative hypothesis, this probability is .5 because the distribution is centered on the observed effect after the fact (other cases

Table 2. *Bayesian evaluation of hypotheses: The effects of varying priors*

Case 1: Uniform Priors			
Hypothesis	$p(H_i)$	$p(D|H_i)$	$p(H_i|D)$
−.4	.2	.00000005	8.33E-08
−.2	.2	.00003	.000005
0	.2	.05	.083
.2	.2	.5	.833
.4	.2	.05	.083

Case 2: Bias as a "risky" hypothesis			
Hypothesis	$p(H_i)$	$p(D|H_i)$	$p(H_i|D)$
−.4	.125	.00000005	6.67E-08
−.2	.125	.00003	.000004
0	.5	.05	.267
.2	.125	.5	.667
.4	.125	.05	.067

Case 3: Bias as a "safe" hypothesis			
Hypothesis	$p(H_i)$	$p(D|H_i)$	$p(H_i|D)$
−.4	.125	.00000005	2.38E-08
−.2	.125	.00003	.000001
0	.125	.05	.024
.2	.5	.5	.952
.4	.125	.05	.024

are discussed below). Bayes' Theorem then combines expectations with evidence to yield what the researcher wants, namely, the probability of each hypothesis, given the data. This probability is the product of the probability of the data, given the hypothesis and the prior probability of the hypothesis divided by the overall probability of the data, or

$$p(H_0|D) = \frac{p(D|H_0)p(H_0)}{\sum p(D|H_i)p(H_i)} .$$

When studies are selected for their statistical significance – as they often are when articles on bias are published – Bayesian posterior probabilities tend to be higher than significance levels. The reason for this is twofold. First, good studies are supposed to be risky, which means that the prior probability of H_0 is assumed to be high. Second, significance levels are positively but imperfectly correlated with the posterior probabilities of the null hypothesis across studies. By regression to the mean, the posterior probability of the null hypothesis is less extreme than the significance level. Thus, Bayesian inferences are conservative because they take prior expectations into account.

A crucial advantage of the Bayesian approach is that data analysis in an individual study can reflect the maturity of the field. Theoretical advances and past research evidence can inform the selection of contending hypotheses and the prior probabilities assigned to them. Research can then move away from rejecting individual hypotheses with data that depart from it in whichever direction, and thus away from incoherent claims. Consider three examples with multiple hypotheses and varying priors (Table 2). The examples share the assumption that the observed effect size is .2 in standard units, and that the significance level, $p(D|H_0)$ is .05. The other values of $p(D|H_i)$ can be found in tables for cumulative probabilities in normal distributions. Bayes' Theorem then gives the posterior probability for each hypothesis (see rightmost column in Table 2).

In the top panel of Table 2, the five possible hypotheses start out equiprobable, suggesting a novel area of investigation where theory is tentative and the empirical base is thin. The posterior probability of the null hypothesis (.08) is not as low as the significance level, and the posterior probability of the observed effect is .83 instead of .92 because there are several alternative hypotheses. If applied to the study of bi-directional bias, this example shows that data indicating, say, a positive bias, also reduce the probability of negative bias. Here, the probability that the true effect is −.2 or −.4 has decreased drastically.

The probabilities in the center panel reflect the assumption that the hypothesis of no bias is as probable a priori as the combined hypotheses of bias. This assumption is implicit in much error research. The demonstration of an error is considered important because the implicit priors suggest that such a demonstration would be difficult to obtain. If such an expectation were made explicit, however, one would have to acknowledge that the posterior probability of rationality did not shrink much (here it dropped from .5 to .267). A Bayesian approach prevents the researcher from having it both ways. *A Bayesian cannot claim that rationality is a strong contending hypothesis and then reject it on the grounds of significance alone.*

The probabilities in the bottom panel reverse the situation presented in the center panel. The observed effect size has a prior of .5, and the remaining priors are equally shared by the other four hypotheses. This example reflects a more mature area of study because researchers already expect to find what they end up finding. The incremental benefit of each new study diminishes as a field matures. Looking back at the three sample cases, the average difference between prior and posterior probabilities was highest for uniform priors ($M = .25$), intermediate for the high prior of rationality ($M = .22$), and lowest for the case in which a certain bias was already expected ($M = .18$).

4.2.3. Advantages of the Bayesian approach. The Bayesian approach encourages investigators to be clear about their expectations. They can no longer use NHST as a surrogate for theory (Gigerenzer 1998), knock down the null hypothesis as a straw man, or treat bias as a foregone conclusion (Krueger 1998c). Bayesianism permits the integration of new evidence with theory and past research even at the level of the individual study. This may prove to be a crucial advantage because some critics of NHST have proposed that all evaluation of hypotheses be ceded to meta-analyses (Schmidt 1996). However, this suggestion creates a social dilemma for individual researchers. If a final judgment regarding the existence of a phenomenon can only be reached by aggregating the results of multiple studies, there is no incentive for a researcher to gather data. Rather, the most effective strategy would be to hope that others will do the studies, wait until enough studies have accumulated, and then do the meta-analysis before anyone else does. With the Bayesian approach, the lessons from the past are not set aside to be rediscovered by meta-analysts. Individual researchers who replicate their work can quantify its di-

minishing returns and reach a rational decision for when to stop.

4.3. Theoretical suggestions

4.3.1. Explanations for the complete range of performance. Our central recommendation is that empirical work and theoretical modeling address the whole range of performance, seeking to understand and explain how both positive and negative phenomena may arise and how they are interrelated. Of course, this recommendation is not new. In the preface to the first edition to his social psychology text, Asch (1952) noted that "social relations at the human level, even the simplest, require emotional and intellectual capacities of a high order. I have tried to call attention to their productive character, which is to my mind also necessary for the understanding of destructive social processes" (p. x).

4.3.2. Social behavior. With Asch's evenhanded stance in mind, the classic behavioral studies can be recast in their original light. We note the high rate of independence in Asch's own work, and the sometime successful struggle towards resistance in Milgram's studies. Milgram emphasized these successes by opening his classic movie with the portrayal of several disobedient subjects. His theoretical account referred to Lewin's idea of competing "force fields" emanating from the victim and the experimenter. Each participant's ultimate behavior then revealed the relative strength of these external forces as well as the relative strength of the competing dispositions internal to *that* person. The notable virtue of this approach was that it aimed not at the phenomenon of obedience per se, but at the differences between circumstances under which behavior is more likely to be influenced by the wishes of the experimenter or by the needs of the victim.

In Darley and Latané's work on bystander intervention, the tone was not as balanced. It emphasized how people's behavior often violated Biblical norms, but the research also included conditions that increased the rate of intervention (Darley & Latané 1968). Following the lead of the pioneers, comprehensive theoretical accounts of behavior should address its moderators, and not just the surprising, attention-getting, and simplistic message that people can be made to behave badly.

No theoretical account of a range of behavior is complete without a cost-benefit analysis. For example, most readers of this article probably would stop at an intersection if told to do so by a traffic officer, perhaps even if the reasons for the stop are not obvious or appear to be wrong (e.g., when no traffic is on the cross street, while impatient drivers accumulate behind you). Why is this? A superficial reading of Milgram's work might suggest that people blindly obey anyone who looks like an authority figure. But at least two other reasons suggest themselves. First, some instructions from authority are based on expert knowledge. The traffic officer may know that a fire truck is on the way. Second, and more generally, obedience to legitimate authority is an important part of social order and should be withdrawn only under compelling circumstances. Surely, a command to give lethal shocks to an innocent victim is one of those circumstances but notice that the issue is not that obedience is a bad thing, but rather of where to draw the line (and how to know where that line is). Similar accounts could be of-

fered for conformity (as the behavior of others can be an important source of information about what is safe and appropriate to do), and bystander intervention (as it may be rational to hold back from immediate intervention while assessing the legitimacy of the need and one's own capabilities to help).

After social psychology began to focus its interest on cognitive processes, few modern classics have been added to the canon of misbehavior. But to the extent that such studies are done, it is important to include opportunities for participants to do the right thing, to interpret the findings in terms of the circumstances that produce the whole range of behavior, and to evaluate the costs and benefits implicit in the behavioral choices. Admittedly, this strategy will produce fewer counter-intuitive or "cute" findings, but it would yield more solid and informative research. In the meantime, it will be helpful for social psychologists to broaden how they think about, and teach, the landmark studies in their field. It might even be salutary if occasionally a social psychologist were to object when NBC tries to turn classic research into *Candid Camera*-like demonstrations of how people are funny.

4.3.3. Social cognition. The road to reform may be especially difficult in the field of social cognition, which suffers from a particular addiction to counter-intuitive findings. All inferential errors are counter-intuitive in the sense that they show ordinary inferences to be wrong. This is the most important reason, we suggest, why lists like Table 1 continue to grow, even as entries duplicate and contradict each other. Overcoming this addiction will be difficult and will require two kinds of reform.

4.3.3.1. Consider adaptive mechanisms underlying error. First, as in the case of behavioral social psychology, some of the classic studies could be appreciated in a new light. Researchers might follow the example of research on visual perception, as conducted after 1896, and entertain the possibility that the psychological mechanism underlying an apparent inferential error might lead to adaptive results outside of the laboratory (Evans & Over 1996; Funder 1987).

Although some researchers in the heuristics-and-biases tradition have acknowledged this idea, experimental demonstrations rarely show how heuristics can produce accurate judgments. Even more notable is the way that their research is so widely interpreted as implying that human judgment is fundamentally erroneous (e.g., Shaklee 1991). We submit that few readers of this literature have carried away the dominant message that representativeness, availability, or the fundamental attribution error are essential components of adaptive social cognition. But of course, to the extent that these and other heuristics have been correctly characterized, they probably are. Like processes underlying the Müller-Lyer illusion, the heuristics that drive human inference are more likely to be part-and-parcel of adaptive cognition than arbitrary design flaws.

In an extensive research program, Gigerenzer and his research group (Gigerenzer et al. 1999) have explored the conditions under which "fast and frugal" heuristics can, like mechanisms of visual perception, lead to interesting errors while yielding many adaptive and accurate results in the complex, chaotic, and consequential settings of the real world. This position has met with some resistance (e.g., Margolis 2000), despite the assertions elsewhere in the er-

ror literature itself that heuristics are not necessarily maladaptive.

A related theoretical development is evolutionary psychology (e.g., Buss & Kenrick 1998; Klein et al. 2002), which assumes that the basic mechanisms of human cognition, like the basic mechanisms of human anatomy and physiology, evolved as adaptations to life in the ancestral environment. The evolutionary approach suggests, for example, that it is adaptive to predict future events on the basis of apparent similarities with current circumstances (one version of the representativeness heuristic) or to pay attention to salient and vivid information (the availability heuristic). A theoretical challenge for both social cognition and evolutionary psychology is to work towards greater convergence, and we expect that this challenge will be met. As we have shown, important "errors" such as consensus bias, self-enhancement, and even the so-called fundamental attribution error can lead to accurate judgments and positive outcomes.

4.3.3.2. Explain error and accuracy in the same framework. The other theoretical reform for cognitive social psychology is that models be constructed which not only explain how errors occur but which also account for accurate judgment. For the area of personality judgment, Funder (1995a) called for an "accuracy paradigm" to complement the dominant "error paradigm." The accuracy paradigm identifies accomplishments of social judgment by using correspondence criteria (Hammond 1996) rather than departures from normative models by coherence criteria. For example, rather than focusing on how people distort artificially input stimulus information, a study might evaluate the circumstances under which participants manifest inter-rater agreement in their personality judgments of real acquaintances (e.g., Funder et al. 1995), or are able to predict the behavior of themselves or others (e.g., Kolar et al. 1996; Spain et al. 2000).

Kenny (1994) presented a Weighted Average Model of social judgment (WAM) to explain the basis of inter-judge agreement in personality rating. The components of agreement include culture, stereotypes, communication, common observation, and personality, and their sum determines the degree of inter-judge agreement that may be found. A related approach, the Realistic Accuracy Model (RAM; Funder 1995a; 1999), assumes that personality characteristics are real, and it seeks to explain how humans

manage to evaluate the attributes of others correctly at least some of the time. Figure 2 shows the process and reveals the ancestry of this theory in Brunswik's (1956) lens model of perceptual judgment.

First, the person who is the target of judgment must emit a cue, usually a behavior, that is *relevant* to the existence of the trait in question. A courageous person must do something brave, a smart person must do something intelligent, and so on. Unless an attribute of personality is manifested in behavior, an observer cannot judge it accurately. Second, this relevant behavior must be *available* to the judge. A highly relevant behavior performed in the next room, with the door closed, obviously is of no help to the judge's accuracy. Less obviously, different behaviors are available to co-workers than to spouses, to parents than to children, and therefore different others will be differentially accurate across the traits that vary in their availability across these contexts. Third, the relevant, available behavior must be *detected*. A social perceiver may be perceptually acute and paying close attention, or distracted, preoccupied, or simply imperceptive. Finally, the relevant, available, and detected information must be correctly *utilized*. The judge must interpret the information in light of past experience and general knowledge. An intelligent and experienced judge can be expected to do this well, but if past experience or knowledge is misleading or if the judge applies it poorly, accuracy will be low.

This simple model has several implications. First, it describes not just a cognitive process of judgment, but rather, the interpersonal process necessary for *accurate* judgment. Second, it implies that accuracy is a difficult and remarkable achievement. A failure at any of the four stages of accurate judgment will dramatically reduce accuracy as failures at each stage combine multiplicatively. Third, the model implies that the traditional paradigm of social psychology addresses, at most, half of what is required to understand accuracy. The paradigmatic study presents social stimuli directly to participants, thus bypassing relevance and availability completely, and largely bypassing the task of cue detection. Traditional studies of social cognition concern the utilization stage.

A fourth implication is that although the RAM is optimistic in the sense that it describes the route to successful social judgment, it is not Panglossian because it recognizes the barriers between judgment and accuracy. In particular, it can incorporate the four moderators of accurate judg-
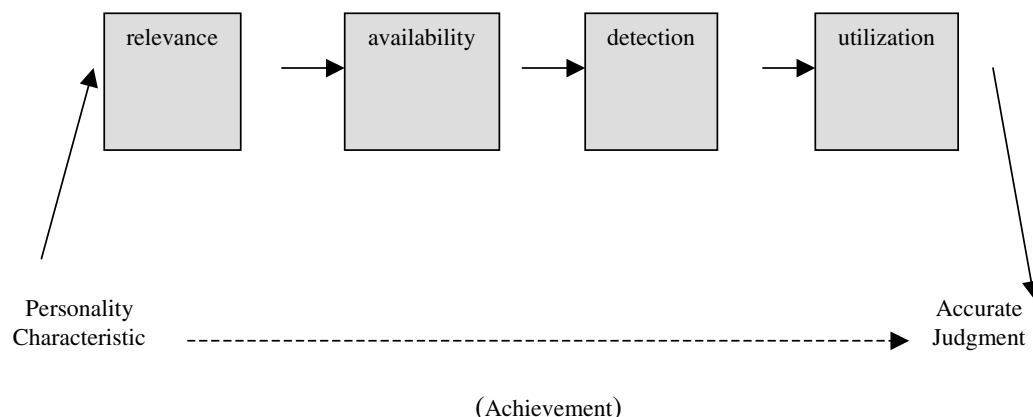

Figure 2. Realistic Accuracy Model (RAM).

ment identified in prior research – properties of the judge, target, trait, and information (Funder 1995a) – and suggest new ones. Some judges are inattentive or cognitively inept, for example. Some targets emit few relevant behaviors – because they are inactive or, in some cases, even deceptive. Some traits are difficult to judge because they are available in only a few contexts, or because their cues are difficult to detect. The information itself may be inadequate in the sense that the judge has not had enough or varied enough experience with the target for sufficient cues to be available on which to base a reasonable judgment. In sum, the RAM describes how people manage to make some of their most difficult judgments, addresses the many ways the process can go awry, and points to four specific stages where efforts to improve accuracy might productively be directed.

5. Conclusion

Discerning the pathological element in the typical is the social psychologist's privilege.
—Alexander Mitscherlich

I have made a ceaseless effort not to ridicule, not to bewail, not to scorn human actions, but to understand them.
—Baruch Spinoza

For decades, social psychology has emphasized how human behavior falls short of ethical standards and moral imperatives. When research attention shifted to judgment and inference, violations of norms for rational thought took center stage. As a final, and perhaps inevitable merger of these two intellectual strands, we now find that ordinary people's moral judgments are being exposed as both hypocritical (Batson et al. 2002) and irrational (Carlsmith et al. 2002). As psychoanalyst Alexander Mitscherlich observed, the fascination with the negative has turned social psychology into a psycho-pathology of everyday life. A more balanced, full-range social psychology, as we tried to sketch it, would be more sensitive to Spinoza's perspective. While seeking not to pass rash or harsh judgments on research participants (and the populations they represent), research in the spirit of Spinoza would seek to understand how people master difficult behavioral and cognitive challenges, and why they sometimes lapse. Ultimately, a more realistic and thus a more compassionate view of human nature may result.

This shift in perspective need not entail a return to the chimerical ideal of a value-free science. Social psychologists have always, at least implicitly, acknowledged which phenomena within their domain they consider desirable and which they consider undesirable. We think that this is as it should be. But we propose that alternative models of rationality be compared carefully, that values be discussed openly, and that the social animal not be judged in trials designed to establish guilt.

In our effort to bring about a shift in perspective, we presented a critique of the current negative paradigm in rather pointed fashion. We realize that social psychologists are often accused (and accuse themselves) of being overly harsh when evaluating work in their own field. Kelley (2000), for example, attributed the marginal status of social psychology among the sciences in part to its tendency for self-loathing. In contrast, our critique was meant to be constructive. We doubt that the traditional pan-critical approach to human behavior and thinking can sustain the field. Instead of demanding respect, it is likely to backfire.

NOTES
1. In 18 experimental conditions, compliance ranged from 93% (when the participant did not have to administer shocks personally) to 0% (when two authorities gave contradictory orders, when the experimenter was the victim, and when the victim demanded to be shocked). In the two best-known and most frequently portrayed conditions, when the experimenter was present and the victim could be heard but not seen, the obedience rates were 63% (at Yale) and 48% (at "Research Associates of Bridgeport"). Across all conditions the average rate of compliance was 37.5% (Milgram 1974, Tables 2, 3, 4 and 5).
2. The proportion of interveners in the lowest-helping conditions of the Darley and Latané (1968) and Darley and Batson (1973) studies were, respectively, 31% and 29%; across conditions the average proportions were 59% and 40%.
3. Over the years, cognitive-social psychology and the psychology of judgment and decision-making (JDM) progressively interpenetrated each other. In the classic collection of papers on heuristics and biases (Kahneman et al. 1982), eight of the 35 contributions were authored by social psychologists. Twenty years later, they account for half of the 42 contributions (Gilovich et al. 2002).
4. Without this further assumption the phenomenon would have to be renamed.
5. The same effect was earlier labeled the "correspondence bias" (see Gilbert & Malone 1995) but the more evocative "fundamental" label has come to predominate.
6. In the noncoerced condition the attributed difference in attitudes was 42.2 points; in the coerced condition the difference was 21.2 points.
7. Sheldon and King (2001) reported that an OVID search on the terms *error* and *bias* yielded more than twice as many hits as the terms *strength* and *virtue*.
8. A recent exception and a potential harbinger of change was an article that included a thorough examination of boundary conditions under which biases are found. As the authors commented, they did "not wish to argue that the [bias under discussion] is a poor strategy" (Goodwin et al. 2002, p. 241). It remains to be seen whether secondary accounts of this research will emphasize these boundary conditions and adaptive possibilities, or simply the finding of bias itself.
9. Intelligence and general well-being are two more examples of variables that probably have modal values higher than the arithmetic mean.
10. See, for example, Klar and Giladi's (1997) report on the "Everyone-is-better-than-average effect." Although most participants recognized the definitional truth that on average, people are average, the significant minority that erred, erred in the same direction, thereby yielding a difference between the average judgment and the modal judgment.
11. Machiavelli (1513/1966) noted that "Without an opportunity [the abilities of Moses, Cyrus, Romulus, and Theseus] would have been wasted, and without their abilities, the opportunity would have arisen in vain" (p. 26). According to an apocryphal story, a jealous Serifotis once told Themistokles that he, Themistokles, was famous only because he was an Athenian. The great strategist concurred and observed that he would be as obscure if he were a Serifotis as the Serifotis would be if he were an Athenian.

Open Peer Commentary

Beware of individual differences

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Abstract: Most judgmental biases are found at the level of samples, but do not apply to each person; they reflect prevailing, but not universal, response tendencies. We suggest that it is more promising to study differences between biased and unbiased persons, and between easier and more difficult tasks, than to generalize from a majority of research participants to humans in general.

That humans err is hardly new. The ancient Romans said *errare humanum est.* The intriguing issue in research on judgmental biases is, therefore, not that humans may err in many ways, but to understand why human reasoning that results in adaptive behavior under most circumstances sometimes goes astray. We agree with Krueger & Funder (henceforth K&F) that this perspective has been lost in research on judgmental biases, and we suggest that neglect of individual differences constitutes part of the problem.

Research on judgmental biases yields main effects and individual differences. Usually, a majority of the respondents shows the "human" bias, whereas a minority shows the opposite bias or no bias at all. For example, Tversky and Kahneman (1974) observed that the majority, but not all, of their respondents neglected base rates and sample sizes. Moreover, variations in the framing of a problem may affect the error rate (Hertwig & Gigerenzer 1999). This shows that respondents vary in relevant knowledge and in cognitive ability, and that tasks vary in difficulty. Although it is productive to study the hierarchy of the difficulty of such problems, as well as which errors covary across respondents, to know whether or not more than 50% of the research participants exhibit a particular response tendency is unlikely to result in major insights.

Unfortunately, the individual differences are usually masked by the way the data are analyzed: as sample means, followed by comparisons of the observed mean to expectations under an elaborated normative model. If a discrepancy is obtained, it is claimed that a "human" bias has been discovered. Ross (1977) even suggested an *intuitive psychologist* as a personification of such tendencies at the aggregate level. But what about those research participants who were not susceptible to the bias under study? Are they *rational psychologists*? And why did they respond appropriately? In many experiments, they probably had better access to relevant knowledge, they understood the instructions as meant by the experimenter, or their affect-related schemata did not bias their judgments. Obviously, to suggest such answers is not as spectacular as to discover a "human" or even a "fundamental human" bias, but it reveals more about human cognitive processes and about the sources of both accurate and inaccurate judgments.

K&F noticed that some tasks that were used to study judgmental biases might qualify as items in an intelligence test. We agree, but would like to add that other biases are related to long-term affect. That depressives tend to be sadder but wiser (Taylor & Brown 1988) is a case in point. Another example is the tendency to report above-average levels in desirable attributes (Klar & Giladi 1997). Note that this tendency does not apply to each individual; whereas a majority of the respondents claims to be above average, a minority reports to be below average.

We are going to illustrate this with some data on the so-called *optimistic bias,* which is a tendency to estimate one's personal risk to experience aversive events as being lower, and one's chances to experience pleasant events as being higher, than those of the average person of one's age, sex, and education (Helweg-Larsen & Shepperd 2001). We let 114 students (71 women and 43 men) estimate the likelihood (in percentages) that: (a) they would experience 14 pleasant events (e.g., to be successful in their job), (b) another person of their age, sex, and education would experience these pleasant events, (c) they would experience 18 aversive events (e.g., to die in a traffic accident), and (d) another person of their age, sex, and education would experience these aversive events. To obtain measures of bias, difference scores were computed by subtracting estimates for other persons from estimates for oneself. Moreover, the risk estimates and difference scores were separately averaged across the 14 pleasant and the 18 aversive events.

Consistent with the optimistic bias view, the respondents estimated the chances that the 14 pleasant events would occur to themselves ($M = 57.02$, $SD = 11.39$) as higher than that they would occur to another person ($M = 49.30$, $SD = 11.29$); $t (113) = 6.72$, $p < .001$. Correspondingly, they estimated the chances that the 18 aversive events would occur to themselves ($M = 21.21$, $SD = 12.55$) as lower than that they would occur to another person ($M = 24.51$, $SD = 12.75$); $t (113) = 3.19$, $p < .01$. That, however, is only half the story: A minority of 21.9% of the respondents indicated that pleasant events were less likely to occur to themselves than to others, and 31.6% indicated that aversive events were more likely to occur to themselves than to others. Thus, a substantial minority of the respondents showed a pessimistic bias. To check whether the individual differences in judgmental tendencies were consistent across particular events, we estimated the internal consistencies of the difference scores and obtained alphas of .67 and .83 for pleasant and aversive events, respectively. Thus, the individual differences were reliable.

Moreover, when the estimated risks for oneself were compared to the actual risks, instead of the risks estimated for others, the majority of the respondents overestimated some risks. For example, the average risk estimate to die in a traffic accident was 16.05% for oneself and 17.15% for another person. But with a population in Germany of more than 80 million, with about 8,000 persons dying in traffic accidents each year, and a remaining life expectancy of our participants of approximately 55 years, their actual risk to die in a traffic accident was less than 1%. Risk estimates of 0% or 1% were provided by 26.3% of the respondents only. Thus, when actual risk was used as the standard of comparison, 73.7% of the respondents overestimated their risk.

There are two implications of these findings for research on judgmental biases. First, like many other biases, the "optimistic bias" does not apply to all humans; rather, it reflects that there are more persons who show one sort of judgmental tendency than there are persons who show the opposite sort. Second, depending on the particular standards to which the actual judgments are compared, opposite judgmental biases can be shown.

Functional clothes for the emperor

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Abstract: A more complete and balanced theoretical framework for social psychology, as recommended in the target article, must include functional explanations of processes – moving beyond enumerations of processes and their properties. These functional explanations are at a different, but complementary, level from process descriptions. The further advancement of social psychology relies on the incorporation of such multilevel explanations.

Krueger & Funder (**K&F**) state that "the problem-seeking approach [in social psychology] tends to be atheoretical" (target ar-

ticle, sect. 3). This claim may be met with some incredulous denials; there are, after all, a cornucopia of theories in social psychology, many of which are discussed by K&F themselves. The theoretical vacuum truly does exist, however, and it resides in the need for functional explanations of processes, rather than mere enumeration of theoretical processes and properties. Functional explanations (e.g., what is phenomenon X designed to do?) are at a different explanatory level from process descriptions. In the information-processing model described by Marr (1982), functional explanations are at the computational level, whereas process explanations are at the algorithmic/representational level. Therefore, the further advancement of social psychology relies not only on the interaction of situational and dispositional factors, as K&F illustrate very well, but also on multilevel explanations.

Thus, in a very real sense, K&F are correct in pointing out that social psychology has little in the way of solid theoretical clothes with which to dress their findings. This brings up, even more urgently, the issue of what could constitute appropriate "clothes" for the discipline. Like pointing out that the emperor has no clothes, it can be difficult to acknowledge that something does not exist when many other people have a stake in saying that it does exist. The dilemma is not just stating that social psychology lacks a metatheoretical framework, but having this message come from someone who is above dismissal as one "unfit for his office or too stupid for any use" (in the words of Hans Christian Andersen [1916/1995]). K&F will hopefully be recognized for the credentials they do, in fact, possess.

A further problem, once the theoretical imbalance and shortcomings in social psychology are acknowledged, is to develop a proper theoretical framework for the functional abilities that constitute social psychology. K&F suggest a couple of directions for this work: the functional considerations derived from theories of bounded rationality and evolutionary psychology. But primarily, K&F have chosen to focus on the initial issue of the adequacy of theory construction in social psychology in terms of both internal and external consistency, and they have some excellent suggestions for methodological tools to improve social psychological research. They devote only a short section to the issue of how social psychology should proceed in developing a better framework for understanding the functional design of the mind. This commentary elaborates on this topic for a simple reason: With the realization that social psychology – like the fabled emperor – has no functional clothes, there is an acute need for clothes.

The emperor of the children's fable shows himself in the end to be a fool who cannot acknowledge his error, and is therefore held up for further ridicule. Let us not make the same mistake in social psychology, particularly when proper metatheoretical clothes are so close at hand. With the implementation of an evolutionary theoretical paradigm, social psychology phenomena become amenable to interactive and integrated functional explanations that cover the range of relevant behaviors. This is true not just for the brief examples given in the target article (i.e., the representativeness heuristic, the availability heuristic, consensus bias, self-enhancement, and the fundamental attribution error), but also for other aspects of social behavior and cognition that K&F noted.

For example, the study of helping behaviors in social psychology has an obvious parallel in the study of altruism in evolutionary biology. These parallels are strong and informative, despite the differences in initial orientation (violations of helping norms in social psychology; the development of any helping in biology) and behaviors typically targeted (emergency helping of strangers in social psychology; kin-based and repeated interaction helping in biology). Recent work to integrate these disciplines have produced strong support for their compatibility and have begun to show the way for an integrated overall theory of altruism/helping (e.g., Burnstein et al.; Cialdini et al. 1997). As emphasized in the target article, this integration and resulting functional theory produces an explanatory framework that covers the complete range of behaviors (i.e., all degrees of helping, from the life-threatening, to the mundane, to failures to help).

Another example is the evolutionary analysis of the fundamental attribution error (FAE; Andrews 2001), which has provided a functional-level description of attributional processes that can explain the historical adaptiveness of the FAE, its current maladaptive qualities, and predicts further functional design features of the FAE that are likely to exist based on this account. In addition to being consistent with existing knowledge of the evolutionary history of the human species, this description is also boundedly rational in that it specifies the conceptual reference class of conditions under which the FAE will be adaptive and accurate (and thereby the conditions outside those bounds as well).

Beyond the benefits of metatheoretical clothes for the existing body of research in social psychology, there are further benefits to adopting a functional-level framework for social behavior and cognition. Such a framework allows social psychology to become better integrated with other behavioral sciences, which will facilitate scientific progression (Tooby & Cosmides 1992). A more direct benefit to researchers is that a functional framework can facilitate the identification of rich topics and hypotheses that are both important and, heretofore, little researched. Daly et al. (1997), for example, point out that the topic of kinship – studied extensively in anthropology, biology, and sociology – is nearly absent as an important topic in social psychology.

K&F allude to, but do not emphasize, that bounded rationality is consonant with evolutionary psychology. In fact, one aspect of the ecological rationality program of Gigerenzer and colleagues is that it is a specific application of evolutionary insights into the fields of judgments under uncertainty and decision-making (Gigerenzer & Todd 1999). Such a theory of evolved functional design, by virtue of its emphasis on what cognitive mechanisms are designed to solve (and the subsequent ability to place failures of cognitive mechanisms into context), yields an account that covers a range of behavioral and cognitive performances. For example, the theory that the mind is predisposed to register numerical information in natural frequency formats (Gigerenzer & Hoffrage 1995), not only provides an explanation for certain patterns of judgments under uncertainty, but also explains some of the difficulties children have in the course of mathematics instruction (Brase 2002a). This has led to further specifications about the nature of information representation in the mind (e.g., Brase 2002b; Brase et al. 1998).

Additional requirements for a balanced social psychology

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Abstract: Ambiguous data obtained by deception say nothing about social behavior. A balanced social psychology requires separating statistical hypotheses from substantive hypotheses. Neither statistical norms nor moral rules are psychological theories. Explanatory substantive theories stipulate the structures and processes underlying behavior. The Bayesian approach is incompatible with the requirement that all to-be-tested theories be given the benefit of the doubt.

One may agree with Krueger & Funder's (K&F's) global thesis that social psychology depicts too negative a picture of human nature. However, they concede too much to the "negative psychology" camp. For example, they could have noted that Milgram's (1963) data were rendered ambiguous by the deception paradigm used. The subjects that were told to facilitate the learner-confederate's learning with electric shocks were confronted with two in-

Table 1 (Chow). *The deductive rules used in statistics and research*

Panel 1. Disjunctive syllogism used in making statistical decisions

	Statistical decision 1	Statistical decision 2
Major premise	Chance or nonchance (q or $-q$)	
Minor premise	Decide to reject chance (statistically significant result) (q is not the case)	Decide not to subscribe to nonchance (statistically nonsignificant result) ($-q$ is not the case)
Conclusion	Not chance ($-q$)	(q)

Panel 2. Conditional syllogisms used in drawing conceptual conclusions

	Modus tollens	Affirming the consequent
Major premise	If Theory T, then the data are in the form of Y in Condition C.	
Research	*Set up Condition C to collect data.*	
Minor premise	Data are not in the form of Y.	Data are in the form of Y.
Conclusion	Theory T is rejected.	Theory T is retained tentatively.

Panel 3. The inductive logic that warrants retaining the to-be-tested theory tentatively in case of affirming the consequent (assuming that there are only three recognized control variables)

		Control variables			
	Independent variable	Sex of participant	Age of participant	IQ of participant	Dependent variable
Experimental condition	To-be-tested level	M:F 5 1:1	18–45	95–105	Performance level
Control condition	Reference level	M:F 5 1:1	18–45	95–105	Performance level

compatible cues, namely, the pleading from the learner-confederate and the utter indifference of the experimenter-confederate. This conflict led to two possibilities. Those who were duped by the cover story defied the instruction. Those who were not duped "complied" as a pro forma exercise because they saw through the deception (Mixon 1976). In other words, Milgram's data were ambiguous as to whether or not there was any compliance at all.

K&F point out the inappropriateness of using moral rules as the theoretical foundation of empirical research. They could also have treated statistical norms in the same way, because negatively skewed distributions are not explanatory theories. What are required are well-defined explanatory theories that stipulate the psychological structures or processes underlying behaviors in various social contexts. At the same time, these theories are embedded in a global frame of reference. This seems to be consistent with the spirit of K&F's plea for studying the whole range of social behaviors.

The authors are correct in pointing out the futility of identifying the research hypothesis with the statistical null hypothesis. They could have been more thorough in pointing out that no statistical hypothesis (be it the null or the alternative hypothesis) can be identified with a research hypothesis, let alone a substantive theory (see Meehl 1967).

It is not clear how the authors may justify their preference for the Bayesian approach to choosing between two theories, H_1 and H_2. The choice is determined by the larger of the two posterior probabilities, $H_1|$Data and $H_2|$Data. At the same time, the two probabilities are a function of the prior probabilities, $p(H_1)$ and $p(H_2)$, respectively. The mathematics is impeccable. There is yet no non-arbitrary answer to the questions, "How are the two prior probabilities determined?" Relying on past research is not satisfactory, because what is true in the past does not guarantee that it is true now. Relying on the researcher's subjective probability is

not acceptable, because beauty is in the eye of the beholder. Hence, the Bayesian approach allows the possibility of giving a heavier weight to one theory at the expense of the other when one evaluates the data.

The Bayesian approach is also incompatible with Tukey's (1960) distinction between making a statistical decision and drawing a conceptual conclusion. Statistics is about data (viz., whether or not the data can be explained by chance influences). Such a decision has nothing to do with the past history or track record of the theories being tested. The deductive rule used in making this decision is disjunctive syllogism (see Panel 1 of Table 1). The major premise makes explicit two mutually exclusive options, chance influences on the data or some non-chance factors. The well-defined null-hypothesis significance-test procedure (NHSTP) provides the minor premise for the syllogism.

Choosing between explanatory theories is a conceptual endeavor that is governed by logic (both deductive and inductive). As may be seen from Panel 2, the major premise is the implicative relationship between the theory and the to-be-tested hypothesis. The latter is a stipulation of what the expected data are like. To conduct research is to set up the appropriate data collection conditions (see the italicized entry in Panel 2). The outcome of the statistical decision in Panel 1 supplies the minor premise for Panel 2.

The *modus tollens* rule is applied to exclude theories whose implications are inconsistent with the data. When the data are consistent with the theoretical expectation, the "affirming the consequent" rule is used to retain the theory tentatively in the presence of all recognizable controls (see Panel 3 of Table 1). The significant difference between the experimental and control conditions in Panel 3 can be said to result from the to-be-tested level of the independent variable when sex, age, and IQ are excluded.

The exercise described in Table 1 is objective because (1) all

contending theories are given the benefit of the doubt, (2) the decision and inference rules are independent of the researcher's vested interests, and (3) any undue influence brought about by the researcher's theoretical stance can be detected readily.

In sum, K&F could have made a stronger case for a balanced psychology had their advocacy stance been less zealous. It would also have helped had statistical issues been distinguished from methodological or conceptual issues.

Psychologists seek the unexpected, not the negative, to provoke innovative theory construction

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Abstract: Krueger & Funder (K&F) see social psychologists as driven to demonstrate that people's behavior falls below relevant moral and intellectual standards. We suggest that social psychologists search for demonstrations of when it is that people's actual behaviors and decisions deviate from expected or ideal behaviors and decisions, and what these "deviations" tell us about general decision processes, including those that do not produce unexpected actions. Often the discoveries are of positive rather than negative behaviors.

According to Krueger & Funder (K&F), social psychologists and judgment and decision-making researchers seek to demonstrate how people violate norms of decent human behavior and sensible human reasoning. K&F seem to suggest that this negative emphasis is rewarded, because these demonstrations of flawed behavior and reasoning are valorized by research peers and seized on by textbook authors. In any event, "a distorted view of human nature emerges" (target article, sect. 1, para. 3).

K&F point out that one of the sources of the negative picture of human behavior is the comparison of some behavior with some normative standard, revealing that the behavior, on moral or intellectual terms, falls below the standard. However, not all such comparisons result in negative views. According to the rational actor model in economics, people are solely motivated by self-interest. One of the contributions of social psychology has been to show that in situations of economic games, people act in ways that are more cooperative, and more considerate of the well-being of others, than is expected from the normative standard (Dawes et al. 1988). Here then is a demonstration of human strengths, one that K&F should be pleased about.

Social and judgment and decision-making researchers sought to produce results that challenged the prevailing model of human functioning at the time. In social psychology, the prevailing image was that of the individual functioning with some independence of the social context, and in decision-making, the image was of a fully rational, information-seeking individual. The social psychologists substituted the conventional image for the image of an actor integrating the information concerning "what is going on" to determine her interpretations of the situation, and therefore her reactions to it. The insight shared by these researchers was that the behavior of other actors who were facing the same "objective" situation gave a good deal of information about the essential nature of the situation and therefore how to react to it. By using experimental variations that should create different interpretations in the participants' minds, the researchers attempted to enter the minds of the participants, carrying out what we now call a phenomenological analysis; determining why they acted as they did.

K&F imply that it is the researchers who cast the behaviors of their participants as normative and ethical failures. The implication strikes us as untrue in regard to the conformity, obedience, and bystander research that is the centerpiece of their censure. To demonstrate this in the bystander area, here are two quotes, drawn from the two studies that they cite.

In one study (Darley & Latané 1968), subjects knew that many other subjects also heard the cries of a person in distress. They failed to intervene because their own responsibility for doing so was diffused. The experimenters commented that

> Subjects who failed to report the emergency showed few signs of the apathy and indifference thought to characterize "unresponsive bystanders." . . . Why then didn't they respond? It is our impression that non-intervening subjects had not decided *not* to respond. Rather they were still in a state of indecision and conflict concerning whether to respond or not. (Darley & Latane 1968, pp. 381–82)

In the Darley and Batson (1973) study, seminarians who were hurrying to another building to give a short sermon on the parable of the Good Samaritan often passed by a person in distress on their way to give the sermon. Here is what the researchers reported about why seminarians did so:

> According to the reflections of some of the subjects it would be inaccurate to say that they realized the victim's possible distress, then chose to ignore it; instead, because of the time pressures, they did not perceive the scene in the alley as an occasion for an ethical decision. . . . For other subjects it seems more accurate to say that they decided not to stop. Why? . . . Because the experimenter, *whom the subject was helping*, was depending on him to get to a particular place quickly. In other words, he was in conflict between stopping to help the victim and continuing on his way to help the experimenter. . . . Conflict rather than callousness can explain their failure to stop." (Darley & Batson 1973, p. 108)

The participants are characterized as good people, who, caught up in complex situations, act in ways that they themselves would not wish. The ameliorative point of the bystander literature is that these actions are the products of situational forces, and people can train themselves to resist these forces. Although we do not have space to demonstrate it, we think that the conformity and obedience researchers also characterized the thought processes of their subjects in similar and similarly sympathetic ways. Milgram's (1974) chapter on "binding forces" is an example of this (Ch. 12). In the decision-making research tradition, as Tversky and Kahneman (1983) comment,

> Our studies of inductive reasoning have focused on systematic errors because they are diagnostic of the heuristics that generally govern judgment and inference. In the words of Helmholtz, "It is just those cases that are not in accordance with reality which are particularly instructive for discovering the laws of the processes by which normal perception originates." The focus on bias and illusion is a research strategy that exploits human error, although it neither assumes nor entails that people are perceptually or cognitively inept.

K&F similarly mischaracterize social cognition research by suggesting that the metaphors underlying social cognition research have been predominantly negative. However, they fail to point out that the metaphors of the 1970s and 1980s (the "naïve scientist" and the "cognitive miser") have been replaced by the metaphor of "motivated tactician" (Fiske & Taylor 1991, Ch. 1). This metaphor emphasizes the pragmatic and functional aspects of social cognition, that is, that "thinking is for doing" (Fiske 1992; 1993).

Given the purpose of K&F's article, it is certainly justified to selectively review the literature. But writing about the status of contemporary social cognition without mentioning a single work on dual-process models (e.g., Chaiken & Trope 1999) is puzzling. The rise of the dual-process framework is a major recent development in the field of social cognition. Dual-process models attempt to integrate forms of heuristic reasoning with presumably more rational forms of reasoning into a single framework, without assigning evaluative labels to any of these forms of reasoning. This integration encompasses both social cognition and judgment and decision-making (e.g., Chaiken & Trope 1999; Kahneman & Frederick 2002; Sloman 1996). Again, the thrust of this work is inconsistent with the picture of social cognition depicted by K&F.

Take their discussion of research on dispositional inferences. One of the most influential social cognition models of such inferences starts from and documents the assumption that people correct for situational influences (Trope 1986; Trope & Alfieri 1997; Trope & Gaunt 1999). However, the correction may not be easily detectable because of the nature of the processes involved. This model does not blame people for falling prey to cognitive errors. Instead, it specifies the exact conditions under which insufficient discounting could arise. But again, this work is not mentioned.

Toward the end of their article, K&F cite a report by one of us (Carlsmith et al. 2002) that they seem to feel demonstrates that "ordinary people's moral judgments are . . . irrational." In fact, the research demonstrates that people who are assigning punishments to wrong-doers generally do so from a just deserts perspective, rather than a deterrence perspective. Why this demonstration that people reason in ways advocated by Emmanuel Kant is a demonstration of irrationality escapes us. That study is encased within a project attempting to demonstrate that the citizens' sense of justice is generally sensible and coherent, and legal code drafters would be wise to pay more attention to it than they do – hardly a message that expresses negativity for the moral reasoning of ordinary people.

In sum, social psychologists seek to find instances in which ordinary behavior deviates from conventional expectations for it, and to explore the reasons for these deviations. It is sometimes the case that these deviations could be labeled as "negative" ones, but in many cases the deviations from expected conduct are positive ones. Although we cannot say that no investigator has ever slipped and characterized participants' behavior as negative, we can say that the tradition of phenomenological analysis has led the researchers to sympathetically understand the participants' reasoning, and to describe it on those terms. By presenting a very narrow view of social psychology, K&F risk reifying the type of research that they are trying to abolish.

But what would a balanced approach look like?

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Abstract: Krueger & Funder (K&F) could have gone further to sketch out a more comprehensive vision of "balanced" psychology. The triumphs and travails of other sciences (e.g., economics) provide clues about the advantages and pitfalls of pursuing such an approach. Perhaps introducing more positivity into psychology may involve asking how people can do better, not how well they do already.

Krueger & Funder (K&F) are to be commended for their call for "balance" in social psychology. I just wish they had gone further. In complaining that social psychologists dwell unfairly on the negative, the authors provide what I assume some will describe as an unbalanced (and notably negative) discussion of the issues – selectively emphasizing some data while ignoring other data that contradict their assertions. Here is one example I know about: In Kruger and Dunning (1999), we asserted that incompetent people overestimate themselves because they cannot spot their own incompetence. K&F dismiss our analysis as a statistical artifact, yet fail to cite crucial data that directly rule this artifact out (Kruger & Dunning 1999, Studies 3 and 4; Kruger & Dunning 2002). I agree with the authors that researchers should strive for balance, but balance requires considering all the data that speak to an issue, not just a selective sampling that favors one broad argument over another.

But there is a more compelling way the authors could have gone further. The point that social psychology is (too) negative has been made in many guises before. Instead, the authors could have made a "constructive" case and brought a more comprehensive vision of a balanced approach into sharper focus by describing in more detail and precision what such a psychology would look like, even if by example. How does one more specifically weave human strengths into psychological theorizing in a broad and integrated way, without simply creating an ad hoc laundry list of competencies to lean up against the miscellaneous list of errors that the authors claim the field obsesses about?

Examples of incorporating human strengths into theorizing about the human animal are out there, and I am surprised that the authors did not consider their potential relevance for social psychology. Many social, behavioral, informational, and biological sciences adjacent to psychology start from the positive premise that people act in adaptive, indeed optimal, ways. Economics has made a good deal of intellectual hay over the last century assuming that people act in their rational self-interest. Animal behaviorists have assumed that animals act to maximize rewards and minimize punishments. Rational choice theorists in sociology assume that people enforce norms and bargain with others to optimize their social fortune. Computer scientists study how computer networks evolve to achieve maximum efficiency. One can assume, given the success of these fields, that one could import the idea of a rational, optimal, positive creature into social psychology.

But these fields also show that thinking about humans in positive ways requires a lot of hard theoretical work to get it right. Economics, in one telling example, has much trouble with the core issue of what exactly people are pursuing when they are rational. It became clear early on that people did not seek to maximize objective outcomes, and so the field created the concept of *utility*. But this concept is a slippery one to grasp. Utility does not necessarily mean hedonic pleasure, for people at times make choices that cause them pain and discomfort. Perhaps utility is synonymous with choice, but if it is tantamount to choice, how can it explain choice without being a mere tautology? And good luck at coming up with an objective and quantifiable measure of utility that is suitable for interpersonal comparison (Homans 1958; Luce & Raiffa 1957). But beyond that, economics is coming to grips with the idea that people are not necessarily rational in fundamental ways, as Danny Kahneman's recent Nobel Prize attests, and is beginning to work to incorporate error into its longstanding models.

I bring up this example not to disparage a psychology based on human strengths, but to show that getting it right will require some hard thought that will run up against some vexing and sometimes impossible issues. What are people maximizing when they get it right? Are they actually maximizing the right thing? Must people maximize, or does it suffice to satisfice? Talking about human strengths without first addressing these basic questions may lead to research that presents warm bottom lines, but will miss an opportunity to create a overarching framework for talking about strength and weakness.

In the meantime, I do not share the authors' pessimism about the future worth of the "error" tradition. As Robert Heinlein once said, it is difficult to learn from anyone who agrees with you, and it would be likewise difficult for people to learn unless research at times contradicts the usual rosy view people hold of themselves. Indeed, if psychology is serious about contributing to human capital (i.e., the knowledge and skills a society possesses), it would do well to point out peoples' imperfections so that they can correct them. There is a reason why hospitals regularly hold mortality conferences to examine patient deaths, rather than discussions about patients who lived long enough to pay the bill. Doctors, in the main, do a terrific job, but they are ever mindful that they can do better.

How do we best incorporate positive messages into psychological research? Serious research aimed at increasing human capital does not stop at characterizing whether people are good or bad at what they do naturally. Instead, such research focuses on how the situation can be changed to make people do *better*. I think all researchers, whether they be more comfortable with error or accu-

racy, would enhance our field greatly if they more quickly asked what policies or interventions would make people more accurate in their judgments and wiser in their actions. For myself, I am always struck by how quickly economists, computer scientists, and political scientists get to these issues in their talks, and how often such thinking is devoid in our own, with a few notable exceptions. Talking about how to create positivity, rather than congratulating whatever positivity is out there already, should be a task enjoyed equally by researchers, whatever their view of human competence. It would also make our field no less theoretical, but that much more interesting, sophisticated, and prestigious in the eyes of the world.

Balance where it really counts

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Abstract: A balanced approach that considers human strengths and weaknesses will lead to a more flattering set of empirical findings, but will distract researchers from focusing on the mental processes that produce such findings and will diminish the practical implications of their work. Psychologists ought to be doing research that is theoretically informative and practically relevant, exactly as they are doing.

If ideas come in and out of fashion, then those presented by Krueger & Funder (K&F) mark the return of the bell-bottom. Similar critiques of the errors-and-biases approach to social cognition have a history almost as long as the approach itself. Many of our reactions to K&F's criticisms have been well articulated before (Gilovich & Griffin 2002; Griffin et al. 2001; Kahneman & Tversky 1996). We will not repeat that history by pointing out recurring misconceptions, but will focus instead on K&F's prescription about what psychologists ought to study and what they ought not.

K&F suggest that social psychology is "badly out of balance" (sect. 4, para. 1), "that theoretical development of social psychology has become self-limiting" (sect. 4, para. 1), and that a solution to this theoretically limited imbalance is to slow the rate of error discovery. Although a more "balanced" approach contains all of the loaded connotations that imply an improvement over a thereby "unbalanced" approach, there are two reasons we doubt it will produce as much empirical yield as it does rhetorical flourish. First, because people in everyday life typically know what people do (Nisbett & Kunda 1985) better than why they do it (Nisbett & Wilson 1977), psychologists are of the most practical and theoretical value when they focus on mental processes (why and how), rather than simply on mental outcomes (what). The real value of science is its ability to make inferences about unobservable processes, a value that would be lost by simply accounting for what people do well and what they do poorly. Second, to the extent that psychologists wish to improve psychological well-being and human functioning, documenting human strengths may be less productive than documenting human shortcomings.

Redressing the right imbalance. K&F suggest that a balanced approach will lead, among other things, to "an improved understanding of the bases of good behavior and accurate judgment" (target article, Abstract). We agree that theoretical understanding of the bases of behavior and judgment is the most desirable goal of psychological research, but worry that "fixing" the imbalance between accuracy and error will not further this goal. Rather, it would create a more problematic imbalance between a focus on mental outcomes versus mental processes.

Tallying social cognitions that are "biased" or "unbiased," "right" or "wrong," or "good" or "bad," places judgmental outcomes at the focus of attention rather than the mental processes that produce them. Focusing primarily on outcomes of any kind – whether positive, negative, or neutral – inhibits theoretical development, because outcomes of complex mental processes are inevitably context-dependent and therefore superficially inconsistent. In a psychological science balanced between processes and outcomes, such apparent inconsistencies are part of healthy scientific progress, prompting theoretical and empirical reconciliations.

Focusing on mental outcomes is also problematic, because the way an outcome is framed often determines whether it is "good" or "bad." "Negative" research on conformity, for example, could just be positive research on "affiliation"; "disgust" can be reframed as "elevation" (Haidt 2003); and "stereotyping" as efficient "categorization." Even the widely influential research program on heuristics and biases pioneered by Kahneman and Tversky assumed that the heuristics people used to guide everyday judgments were generally beneficial – an assumption polemically confirmed by Gigerenzer and colleagues in their research on "fast and frugal" heuristics. In other words, the same mental processes can lead to mental outcomes that are sometimes "ludicrous" (Tversky & Kahneman 1971, p. 109), and at other times can be the very things that "make us smart" (Gigerenzer et al. 1999).

A focus on judgmental outcomes may create a rush to reframe previous research on human shortcomings as human strengths, or, worse, to "rediscover" mental processes that usually produce accurate judgments but occasionally lead to bias and error. Such a focus may lead some to believe that new insights have been gleaned when they have not, but this new gloss is unlikely to advance psychologists' understanding of the human condition.

Pursuing mental problems. Even a discipline balanced between mental processes and mental outcomes will gain more from an unbalanced focus on human shortcomings than on human strengths. K&F suggest, "everyday social behavior and cognition includes both appalling lapses and impressive accomplishments" (sect. 1, Introduction), but it is those appalling lapses that create the greatest psychological impact, and therefore are the more interesting to economists, lawyers, politicians, public policy makers, or anyone who matters beyond our experimental laboratories.

Humans are much more sensitive to shortcomings and mistakes than to strengths and accomplishments (Kahneman & Tversky 1979; Rozin & Royzman 2001; Taylor 1991). Failing hurts more than succeeding feels good. A few moments of self-reflection will make clear that a single colleague's slight, lover's insult, or negotiator's misstep can ruin a day, a relationship, or a reconciliation. It is harder to think of analogous compliments, sweet nothings, or creative compromises. Mental shortcomings, in this regard, seem somewhat analogous to physical pain; they serve as a clear signal that something is wrong or needs to be fixed. It is therefore no more erroneous for psychologists to focus on alleviating the mental shortcomings of their participants than for physicians to focus on alleviating the pain of their patients. Just as we would encourage our colleagues and students to attend to their broken leg rather than their unbroken arm, so too will we continue to encourage them to work in areas where their work can best improve the human condition.

Concluding thoughts. Waves of research come and go, and we doubt this clarion call for research on judgmental accuracy will create any more whiplash among researchers than any of its predecessors. K&F may be correct to hearken a regime change, but we hope the change will be to develop broader theoretical models, rather than simply add a new set of human strengths to the existing list of human shortcomings. Psychologists don't so much need redirection to the study of human strengths as they need to focus on the mental processes underlying mental outcomes, maintaining balance where it really counts.

Beyond negative and positive ideologies

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Abstract: There are reasons to endorse Krueger & Funder's (K&F's) critique, but also to disagree with their diagnosis. A "problem-seeking approach" is hardly the cause of imbalance and lack of theoretical integration. Precommitted, ideological theories afford a more appropriate explanation.

Krueger & Funder (K&F) characterize the status quo in social psychological research as an endless list of partly contradictory deficits, without conceptual integration and detached from environmental considerations. They offer several recommendations to overcome this unsatisfactory situation, such as reducing negativity, refocusing on assets and accuracy, and reducing null-hypothesis significance testing (NHST).

No doubt, this article is thought-provoking and challenging. The authors' characterization of the status quo can be hardly contested. However, I dare to deviate in the diagnosis of the causes and most appropriate cures. Pointing to a negativity bias may misrepresent the origin of the crisis, a refocus on assets may not cure the disease, and NHST can hardly account for the situation.

Why Krueger & Funder fall prey to the same "disease." A prime example is the fundamental attribution error (FAE; Ross 1977). Even behaviors that are clearly under situational constraints tend to be attributed to persons' dispositions. That dispositional attributions are not totally discounted is considered a violation of normative rules. The authors point out, correctly, that FAE researchers themselves commit the FAE, blaming human judges for behaviors that are largely under situational (experimental) control. They don't seem to notice, though, that they themselves fall prey to the same syndrome. Their appraisal is extremely skeptical, "problem-seeking," without attempting a balanced review. They commit the FAE by blaming researchers rather than the scientific situation (reviewing, publishing system, communicability). This is no surprise, after all, because all science is about the dialectics of laws and violations of laws. For a scientific contribution to be accepted as original, it has to deviate from established laws. A research strategy that focuses on norm deviations can be hardly blamed; it is inherent to the best exemplars in the history of scientific discovery, from Copernicus to Einstein to Kahneman's Nobel prize. Editors, peer reviewers, readers, and students will hardly change their appraisal of originality.

I also tend to disagree that NHST is crucial. Bayesian statistics leads to contrastive hypothesis testing as well. It matters little whether, say, success at lie detection is analyzed by a *t*-test – the prototype of NHST – or a correlation coefficient meant to measure accuracy. Both statistics can be converted into each other.

But let us turn to the central point, the focus on negative findings. Do K&F really mean negative *studies* or negative *interpretations* of ambivalent findings? Is there a sound basis for classifying raw findings as clearly positive or negative? Are unrealistic optimism, the FAE, or expectancy biases per se negative? Can a self-serving bias be negative and inclusive fitness (Hamilton 1964) positive? In fact, if there is a negativity focus, it can only arise from the preferred interpretations of heuristics and biases that can also be considered adaptive, economic, fast and frugal (Gigerenzer et al. 1999). Or could it be that the recent mainstream has already shifted, showing a benevolent tendency toward ecological or evolutionary excuses? Is this what the authors call for? Do they want us just to shift the criterion (as in signal detection) for classifying findings as positive, leaving studies and theories unchanged?

Ideologically motivated research. I suspect the actual crisis does not originate in a mere bias toward negativity, but in ideological, output-bound constraints imposed on research paradigms, which carry in their names the most preferred result. Paradigms are committed in advance to stereotype *threat,* to a *dual*-process

principle, to implicit *attitudes* assessed in an Implicit Association Test (IAT), to a fundamental attribution *error,* to base-rate *neglect,* or to *automatic* processes. Such precommitments – which often persist despite strong evidence that stereotypes may not threaten, processes may not be dual, attitudes not implicit, and automaticity not be really automatic – originate in a common meta-theoretical strategy, namely, just to generalize favorite empirical findings. It is this strategy that yields lists of unconnected phenomena and counter-phenomena: risky shift and cautious shift, hot hand and gambler's fallacy, stereotype threat and boost.

Overconfidence provides a prominent illustration. The proportion of correct responses to knowledge questions is often markedly lower than the subjective confidence expressed on a percentage scale. This "deficit" was recently reframed positively as adaptive behavior (Gigerenzer et al. 1999). Confidence was shown to exceed achievement only when knowledge questions are overly difficult and tricky. When tasks are representative of the natural learning environment, overconfidence disappears (Juslin 1994). Analogous to the FAE, this means to excuse participants and to attribute the bias to the researcher.

However, I'm afraid this positive refocus is still output-bound and does not tackle the ultimate problem; it merely replaces one bias with another. After all, adaptive behavior is not confined to the learning environment, but calls for transfer to new environments. A real improvement would require a comprehensive model of environmental learning, including ecological requirements, transfer conditions, cognitive functions, and informational constraints. Such an open approach would not be precommitted to overconfidence, or underconfidence. Any deviations from perfect calibration need not be attributed to human irrationality but could be understood in terms of refined human–environmental interactions. The researcher's FAE is greatly reduced, and it matters little whether NHST is replaced by Bayesian or Brunswikian analyses. Pluralistic competition of theory-driven ideas determines the outcome, rather than ideological fixation.

I particularly like the potential of the realistic accuracy model (Funder 1995a), which suggests a decidedly interpersonal approach to social psychology, involving sources and recipients, agents and patients, experimenters and participants. Within such a framework, what is positive for one party may be negative for the other. Lie detection is good for the detector and bad for the liar; an egocentric bias is good for the self and bad for one's partner; or false alarms in medical diagnosis are safe for the physician and threatening for the patient. The German language uses the same word, "täuschung," for fallacy (intrapersonal) and deception (interpersonal). This homonym highlights the debiasing value of a genuine interpersonal approach, which focuses on both the judges' "fallacies" and the experimenters' "deceptions," and which offers a fresh and more refined re-approach to biases and shortcomings in several other respects.

Apes and angels: Adaptationism versus Panglossianism

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Abstract: The "straw man" prior expectation of the dominant social psychology paradigm is that humans should behave with perfect rationality and high ethical standards. The more modest claim of evolutionary psychologists is that humans have evolved specific adaptations for adaptive problems that were reliably present in the ancestral environment. Outside that restricted range of problems, one should not expect optimal behavior.

Krueger & Funder (K&F) have written an excellent critique of the dominant "heuristics-and-biases" paradigm within social psychol-

ogy. One key point which they make is the unreasonableness of the null hypothesis used to characterize normative human behavior within this paradigm. The standard prior expectation is that, except for the specific heuristics and biases identified, humans should behave with perfect rationality and conform to the highest ethical standards, sometimes drawn from scriptural models. Of course, given the number of apparent irrationalities and moral failings identified by social psychologists, no one seems to be taking these prior expectations very seriously. Yet they continue to serve as the standards against which human behavior is routinely judged.

Evolutionists have been accused of Panglossianism (e.g., Gould & Lewontin 1979), that is, presuming perfect levels of organismic adaptation to the environment. However, adaptationists, and even optimality theorists, have made no such extravagant claims. Yet, even the modest claims made for the adaptedness of human behavior pale in comparison to the ideals for human behavior represented by what evidently function as the prior expectations of social psychologists in pointing out the errors of our ways. Many evolutionary psychologists (e.g., Tooby & Cosmides 1992), instead, envision the human mind as containing a large number of domain-specific functional specializations for dealing with particular adaptive problems. In this view, the human mind has been compared to a Swiss army knife, containing special adaptations for a variety of environmental contingencies. Yet no adaptationist ever claimed that the human mind contains adaptations for every conceivable scenario that could be dreamt up and simulated in the laboratory. The more modest claim has typically been that humans (and other animals) have evolved specific adaptations for particular adaptive problems that were reliably present in the ancestral environment. Step outside that restricted range of evolutionary adaptedness, and one should not necessarily expect to observe optimal behavior.

From an evolutionary point of view, the most surprising thing about the straw-man presumption of perfect rationality and ethicality is that no specific mechanism is proposed for how the organism was supposed to have solved the given problem. If biological preparedness is being assumed, under what specific circumstances should a special adaptation have been evolved? If developmental plasticity is being assumed, what specific contingencies of reinforcement or other conditions of learning should have produced the expected behavior? If these questions cannot be answered, then the prior expectations of what the behavior should have been (according to the straw-man null hypothesis) should be considered unreasonable.

Furthermore, many of the heuristics that people have been shown to use are precisely what one would expect from this perspective of bounded rationality (Gigerenzer et al. 1999). Specifically, some of the biases that social psychologists have decried, such as authoritarianism and conformity, are precisely the kind of direct and indirect social learning biases that are predicted as often being the optimal behavioral mechanisms by cultural evolution theory (Boyd & Richerson 1985). Furthermore, with respect to scriptural ethical standards, the idea that any animal should routinely act like the Good Samaritan and help other, genetically unrelated, individuals in need, when there is little possibility of reciprocation, is simply not a reasonable expectation within evolutionary theory (e.g., Trivers 1971). Under the contrived experimental circumstances, the observed apathy and nonintervention is to be fully expected of a well-adapted organism.

One of the distinctions that many evolutionists have championed is the difference between the *proximate mechanism* and *ultimate function* of any given behavior (Durrant & Ellis 2003). The proximate mechanism of a behavior is explored in terms of *how* the system might work. The ultimate function of the behavior is examined in terms of the adaptive problem it solves: *Why* does the system exist at all? Classic social psychology has concentrated on explaining the proximate questions of many human behaviors, for example, *how* do perceptions of authority affect obedience or conformity? However, this area of research has largely ignored ulti-

mate explanations. *Why* does authority elicit such a response in obedience? *Why* does conformity exist within the human species? Typically, when answers to these latter questions are sought, explanations have been constructed either post hoc, or based on a set of assumptions derived from other proximate concepts, such as the resolution of intrapsychic conflicts. For a logically coherent explanation to be composed, research should start with a theoretical concept grounded in biological reality rather than utopian desire. This is not to say that one line of questioning is inherently more important in understanding behavior than the other, but it does make a practical difference whether humans are viewed as risen apes, with inherent phylogenetic limitations, or fallen angels, essentially unconstrained to deviate from perfection.

Although a superficial reading of the target article might suggest that social psychology should abstain from examining so-called bad behaviors, a closer reading suggests that this penchant be supplemented with efforts to identify moderators as well as the functional significance of seemingly irrational thought and action. K&F certainly aren't proposing an imbalanced focus on the "sunny side" of social behavior, and advocates of that view would have to explain why social psychology *should* be discouraged from bringing empirical research to bear on pressing social issues. Instead, the authors urge social psychologists to look past their rather pathetic portrait of humans to acknowledge when ordinary perceptions and inferences are veridical, people behave in prosocial ways, and how ostensible errors and moral shortcomings might reflect an underlying optimality (parapraxes; Freud 1966). The good news is that quite a few of the more influential paradigms in social psychology do emphasize more distal goals (e.g., closure, self-verification, belonging, meaning) underlying seemingly irrational behaviors (e.g., Greenberg et al. 1997; Leary & Baumeister 2000; McGregor et al. 2001; Swann 1987; Tajfel et al. 1971; Tesser 1999; see Kruglanski 1996, for a review). In short, the recommendation to link counterintuitive and untoward social phenomena to distal functions is already in place among a number of prominent social psychological perspectives, and perhaps even more so in behavioral social psychology than in social cognition research.

The "bias" bias in social psychology: Adaptive when and how?

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Abstract: In following the form of a standard "bias" paper, the authors highlight a potentially serious bias of uncertain magnitude. A negative focus in research has certain adaptive features in terms of professional and public support, as well as theory generation. The bias deserves attention and correction, but in ways that do not exaggerate its liabilities or overlook its virtues.

Krueger & Funder's (K&F's) critique of the "bias and misbehavior" focus in social psychology itself follows the format of much of this literature, identifying a departure of uncertain magnitude from a preferred standard, and exploring potential causes, consequences, and antidotes. But the authors also argue that many, if not most, demonstrations of a bias tend to overestimate its importance or magnitude and overlook its adaptive qualities in the real world. Can this "'bias' bias" in social psychology be subjected to a similar analysis?

Although scans of recently published literature reveal a wealth of articles that do not purport to identify some sort of troublesome bias or human shortcoming, the interest in these latter topics has remained high over the years. Two particular virtues of this interest stand out. First, it has played an important role in helping social psychology pass public and professional "So what?" tests. En-

during interest in, and support for, science is consistently grounded in utilitarian concerns; and although by no means the only way of arguing for the importance and relevance of studying social behavior and cognition, the field's focus on the negative and the surprising has been an important tool in promoting social psychology's professional and public support.

Social science is inescapably linked to persuasion (cf., Friedrich & Douglass 1998) – to convincing various audiences that certain questions or theories are worth pursuing, that particular interpretations are superior to others, and that developments in the field should matter to the public and to potential sources of funding. Other sciences face similar constraints but benefit from: (a) more visible track records of utilitarian benefits, and (b) an absence of lay theories the public assumes are as good as the theories researchers have to offer. Biases and misbehaviors have provided vivid ways to communicate to multiple audiences why the field matters at all, and why it provides a necessary counterpoint to lay explanation and folk psychology. True – important topics have been left unstudied as a result – but part of the solution seems to lie in our simply doing a better job of articulating for fellow scientists and the public the benefits of pursuing those topics.

The authors note that the significance or consequences of purported errors and biases may be exaggerated both by the experimental designs used and by the manner of presentation. But in many cases, the true importance remains largely unknown. The core concerns for the most prestigious journals in the field are (appropriately) in the area of theory development, and yet that means that papers providing real-world demonstrations or applications without breaking new theoretical ground are typically diverted to less prestigious "applied" journals. One should hardly be surprised at a relative dearth of studies demonstrating real-world magnitude and importance given the structure of professional rewards in the field.

The second, and more important, virtue of the focus the authors critique is the role it can play in stimulating theory development. The focus on bias and misbehavior in the research literature has clearly drawn us to anomalies and, in doing so, has called into question basic assumptions about human behavior and cognition. Consistent with the central role of falsification in theory testing, attention to "negative" anomalies often provides the counter-examples that fuel theory development.

Perhaps the most salient external validations of this notion are the recent Nobel Prizes for basic research relevant to "behavioral economics" (Dubner 2003), but there are numerous other examples of integrative theories of the sort that the authors advocate. The empirical literature is peppered with demonstrations of boundary conditions and moderator variables that qualify various claims, but it often takes the development of a rather large and diverse empirical literature before powerful syntheses emerge. Petty and Cacioppo's Elaboration Likelihood Model of persuasion (Petty & Wegener 1999) is an example of a theory that emerged at least in part as an effort to reconcile widely disparate findings, some of which showed that people were inconsistent in how thoughtfully or "centrally" they processed messages. Yet another example comes from work integrating a broad and often conflicting literature on the so-called confirmation bias (Friedrich 1993; Trope & Liberman, 1996) – work that has been invoked to counter "negative" views of human behavior in other disciplines (e.g., Mele 2001). Space prohibits an exhaustive review of the wide range of integrative efforts, but it is noteworthy that these two examples and others are broad in scope and seek to explain both strengths and weakness in human cognition within a parsimonious set of principles.

A continuing, serious problem to which the authors appropriately direct attention is the tendency to construct narrow, post hoc theories to account for specific effects. As Seidenberg (1993) has argued, a vital but often ignored component of a good theoretical account is that it is founded on "independently motivated principles." That is, the principles used to account for a phenomenon should emerge from a broader understanding of human behavior

and its constraints and be justified outside of the effect to be explained. Evolutionary considerations (e.g., Friedrich 1993; Gigerenzer et al. 1999; Kenrick et al. 2002) and connectionist architectures (e.g., Smith 1996) are noteworthy sources of powerful, independently motivated principles that have been used in providing comprehensive accounts of strengths and weaknesses within a single framework.

The rewards of frequent empirical publication and narrowly focused methodological precision – not to mention the far more limited professional outlets for integrative theoretical work – might well play as large a role in the slow and skewed progress of theory development as researchers' fascination with the anomalous, the negative, and the statistically significant. Nevertheless, K&F's article is an important part of the self-correcting nature of our science. In keeping with much of the literature that the authors take to task, their work highlights a significant concern for the field with a potentially large but undetermined effect size. The "bias" bias itself reflects certain adaptive properties, and the challenge is to capitalize on its inherent strengths while limiting the harms that come from misapplication and overuse of certain strategies.

The irrationality paradox

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Abstract: In the study of judgmental errors, surprisingly little thought is spent on what constitutes good and bad judgment. I call this simultaneous focus on errors and lack of analysis of what constitutes an error, the *irrationality paradox.* I illustrate the paradox by a dozen apparent fallacies; each can be logically deduced from the environmental structure and an unbiased mind.

The objective of demonstrating that people systematically underestimate or overestimate a quantity has become a paradigm in social psychology. Researchers tell us that we overestimate small risks and underestimate large risks, that our average confidence in our knowledge is larger than the actual proportion correct, and that we overestimate the long-term impact of emotional events, such as losing a child. This paradigm is part of a broader movement that emphasizes human irrationality and leads to a paternalistic attitude towards citizens, such as in behavioral economics and in behavioral law and economics (e.g., Sunstein 2000). I would not object to paternalism if the norms were well reasoned and argued. Yet, in the study of judgmental errors, surprisingly little thought is spent on the question of what actually constitutes good and bad judgment (Gigerenzer 1996b; 2000). Rather, researchers tend to take normative claims about irrationality at face value or accept these by authority, not by an analysis of the problem. I call this simultaneous focus on errors and lack of analysis of what constitutes an error, the *irrationality paradox.*

This commentary is about the missing study of ecological rationality in social psychology, an issue that I believe is sympathetic to Krueger & Funder's (K&F's) important article, but which they did not put into the foreground. The basic tenet of ecological rationality is that the rationality or irrationality of a judgment can only be decided by an analysis of the structure of the environment or the experimental task. Herbert Simon (1990) expressed this tenet once through the analogy of a pair of scissors: "Human rational behavior is shaped by a scissors whose blades are the structure of task environments and the computational capabilities of the actor" (Simon 1990, p. 7). By looking only at one blade, one cannot understand how minds work, just as one then cannot understand how scissors cut.

Environmental structures include statistical structures, such as the signal-to-noise ratio, the shape of distributions, and the size of samples, as well as social structures, such as the presence of com-

Table 1 (Gigerenzer). *Twelve examples of phenomena that were first interpreted as "cognitive illusions" (left), but later revalued as reasonable judgments given the environmental structure (right)*

Is a phenomenon due to a biased mind . . .	. . . or to an environmental structure plus an unbiased mind?
Overconfidence bias (defined as miscalibration)	"Miscalibration" can be deduced from a unbiased mind in an environment with substantial unsystematic error, causing regression toward the mean (Dawes & Mulford 1996; Erev et al. 1994)
Overconfidence bias (defined as mean confidence minus proportion correct)	"Overconfidence bias" can be deduced from an unbiased mind in an environment with unrepresentative sampling of questions; this disappears largely with random sampling (Juslin et al. 2000)
Hard–easy effect	"Hard–easy effect" can be deduced from an unbiased mind in an environment with unsystematic error, causing regression toward the mean (Juslin et al. 2000)
Overestimation of low risks and underestimation of high risks	This classical phenomenon can be deduced from an unbiased mind in an environment with unsystematic error, causing regression toward the mean (Gigerenzer & Fiedler 2003)
Contingency illusion (based on prior beliefs or prejudices)	"Contingency illusion" can be deduced from an unbiased mind performing significance tests on samples with unequal sizes, such as minorities and majorities (Fiedler et al. 1999)
Most drivers say they drive more safely than average	The distribution of the actual numbers of accidents is highly skewed, which results in the fact that most drivers (in one study, 80%) have less accidents than the average number of accidents (Gigerenzer 2002; Lopes 1992)
Availability bias (letter "R" study)	"Availability bias" largely disappears when the stimuli (letters) are representatively sampled rather than selected (Sedlmeier et al. 1998)
Preference reversals	Consistent social values (e.g., don't take the largest slice; don't be the first to cross a picket line) can create what look like preference reversals (Sen 1993; 2002)
Probability matching	Social environments with N > 1 individuals competing over resources can make probability matching a more successful strategy than maximizing, whereas this would not be the case for an individual studied in isolation (Gallistel 1990)
Conjunction fallacy	"Conjunction fallacy" can be deduced from the human ability for pragmatic inference about the meaning of natural language sentences – an ability no computer program has so far (Hertwig & Gigerenzer 1999)
False consensus effect	This "egocentric bias" can be deduced from Bayes's rule for problems of which a person is ignorant, that is, where a person has no knowledge about prior probabilities (Dawes & Mulford 1996)
Violations of logical reasoning	A number of apparent "logical fallacies" can be deduced from Bayesian statistics for environments in which the distribution of events (e.g., P, Q, and their negations) is highly skewed (McKenzie & Amin 2002; Oaksford & Chater 1994), and from the logic of social contracts (Cosmides & Tooby 1992; Gigerenzer & Hug 1992)

petitors, social values, and contracts (Anderson 1990; Gigerenzer 1996c; Gigerenzer et al. 1999). Table 1 (left side) shows a dozen phenomena that have been claimed to be cognitive illusions – by mere assertion, not ecological analysis. Table 1 (right side) shows that as soon as researchers began to study the structure of information in the environment, what looked like a dull cognitive illusion often turned out to be a sharp scissors. Note that these researchers listed on the right side of the table provided formal arguments, not just an optimist's story set against a pessimist's saga. Their argument is a deductive one: The environmental structure plus an *unbiased* mind is *logically sufficient* to produce the phenomena. Note that a sufficient reason does not exclude the possibility of alternative explanations (this can be decided by empirical test; see below).

The general argument is that environmental structure (such as unsystematic error, unequal sample sizes, skewed distributions) plus an unbiased mind is *sufficient* to produce the phenomenon. Note that other factors can also contribute to some of the phenom-

ena. The moral is not that people never err, but that in order to understand good and bad judgments, one needs to analyze the structure of the problem or the structure of the natural environment.

For example, consider task environments with substantial but unsystematic error, such as when people are confronted with general-knowledge questions, the answers to which they do not know. A typical finding is that when participants were 100% confident of giving a correct answer, the average number correct was lower, such as 80%. This phenomenon was labeled "overconfidence bias" or "miscalibration" and was attributed to confirmation biases or wishful thinking. An analysis of the environmental structure, however, reveals substantial unsystematic error, which *in the absence of any cognitive bias* leads to regression towards the mean: The average number correct is always lower than a high confidence level. Therefore, the environmental structure is a logically sufficient condition for the phenomenon. Now we can ask if there is, in addition, a trace of a real cognitive bias? When Erev et al. (1994) and Dawes and Mulford (1996) plotted that data the other way

round, regression towards the mean produced a mirror pattern that looked like *underconfidence bias:* When participants answered 100% correctly, their mean confidence was lower, such as 80%. They found no real bias. The same unsystematic error is a sufficient condition for two other phenomena listed in Table 1, people's apparent error of overestimating low risks and underestimating high risks (Lichtenstein et al. 1978), as well as the hard–easy effect (see Gigerenzer & Fiedler, 2003; Juslin et al. 2000).

Consider next how stimulus objects are sampled from an environment and a class of phenomena known as "contingency illusions," which were attributed to irrelevant prior beliefs or prejudices against minorities. Versions of the contingency illusion have been claimed in research on self-fulfilling prophecies (Jussim 1991; Kukla 1993), on confirmation biases in hypothesis testing (Snyder 1984), and on alleged memory advantages for negative behaviors in minorities (Hamilton & Gifford 1976; Hamilton & Sherman 1989).

Let me use evaluative judgments of minorities as an illustration. It is an ecological truism that minorities are smaller than majorities, and a recurrent property of social environments is that the rate of positive, norm-conforming behaviors is higher than the rate of negative, norm-violating behaviors (Fiedler 1991; Parducci 1968). When these two ecological assumptions are built into the stimulus distribution presented in a social psychological experiment, participants may be exposed to the following description:

Group *A* (Majority):	18 positive and	8 negative behaviors
Group *B* (Minority):	9 positive and	4 negative behaviors

Note that the same ratio of positive to negative behaviors (18:8 = 9:4) holds for both groups, but people nevertheless tend to conclude that there is significantly more positive behavior in the majority than in the minority: a "contingency illusion." Given the unequal sample sizes, however, an unbiased mind using an (unconscious) binomial test would infer that there are significantly more positive than negative behaviors in the majority group ($p =$ .038), but not in the minority group ($p = .13$). Thus, unequal sample size is a sufficient condition for a class of phenomena labeled "contingency illusions." Again, one can empirically test whether an additional bias exists because of prior knowledge, such as by replacing real groups by neutral labels, to rule out any influence of prior knowledge (Fiedler et al. 1993; 1999).

Table 1 lists two other phenomena that can be deduced from sampling. One has been called "overconfidence bias," and is defined as mean confidence minus proportion correct (many different phenomena have been labeled overconfidence). Note that "miscalibration" does not imply this phenomenon. It can be logically deduced from unrepresentative sampling of stimulus items and an unbiased mind (Gigerenzer et al. 1991). An analysis of 135 studies showed that "overconfidence bias" practically disappears when stimuli are randomly selected from an environment (Juslin et al. 2000). The second phenomenon is that people erroneously judge that there are more English words with a letter (such as "R") in first position than in third position, which has been attributed to "availability" (Tversky & Kahneman 1973). When one uses a representative sample of letters, rather than the five letters selected by Tversky and Kahneman (which are among the few that are more frequent in the third position), people's apparently systematic bias disappears (Sedlmeier et al. 1998).

The other "cognitive illusions" listed in Table 1 can be deduced in the same way for the task structure, including that of social environments (see also Gigerenzer 2000; 2001; Gigerenzer & Fiedler 2003; Krueger & Mueller 2002). An objection to my general argument is, "But people do commit errors!" No doubt, people commit errors; but I am talking about a blunder committed by a research program. The fact that little attention is paid to establishing what is good and bad reasoning cannot be excused by blaming John Q. Public.

Errors might be a window to cognitive processes, but falsely identified errors do not seem to be so, which is consistent with the fact that after 30 years of collecting errors, no model of cognitive

processes has emerged from overconfidence bias, the conjunction fallacy, or any of the other celebrated errors – only vague labels. In contrast, the study of amazing performance seems to be a better window to cognitive processes, such as the less-is-more effect, which led to the discovery of the recognition heuristic (Goldstein & Gigerenzer 2002).

The story is told that there are two personalities among psychologists, optimists and pessimists, who see the glass as half full or half empty, respectively. According to this legend, people like Funder, Krueger, and myself are just kinder and more generous, whereas the pessimists enjoy a darker view of human nature. This story misses what the debate about human irrationality is about. It is not about how much rationality is in the glass, but what good judgment is in the first place. It is about the kinds of questions asked, not just the answers found.

Null hypothesis statistical testing and the balance between positive and negative approaches

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Abstract: Several of Krueger & Funder's (K&F's) suggestions may promote more balanced social cognition research, but reconsidered null hypothesis statistical testing (NHST) is not one of them. Although NHST has primarily supported negative conclusions, this is simply because most conclusions have been negative. NHST can support positive, negative, and even balanced conclusions. Better NHST practices would benefit psychology, but would not alter the balance between positive and negative approaches.

Balance is hard to achieve, but Krueger & Funder (K&F) are right to try. Several of their concrete suggestions for the better balancing of positive and negative approaches in social cognition are constructive and likely to be beneficial. First, the canonical literature is not likely to change overnight, but a more balanced reading of it would help, and the canon is indeed more balanced than the treatment it has received in recent decades. Second, K&F avoid the temptation to implore research that is more likely to support positive conclusions. Their formulation, which is entirely reasonable, is to encourage research in which normative responding is at least possible. Third, it would be of salutary value if more researchers examined modal as well as mean responding. When most participants respond normatively, it is appropriate to model this behavior. This is not intended as a complete list of their constructive suggestions: there are more. However, one of their recommendations, although helpful in other ways, is likely to have no impact on the balance between negative and positive approaches. This is the advocacy of increased skepticism with regard to null hypothesis statistical testing (NHST).

To be sure, the limitations of NHST, as it is currently practiced, are real, and the widespread lack of understanding of these limitations has led to a great deal of ritualistic, often inappropriate, application of NHST (Gigerenzer 1993). Anything that may diminish the misunderstanding and ritualistic, inappropriate application of NHST in psychology is to be applauded.

However, although a more thoughtful approach to NHST by more researchers would help psychology in many ways, it would probably have little impact on the balance between positive and negative approaches in psychology. This is because NHST, whether used thoughtfully or ritualistically, may be used equally in support of positive conclusions as in support of negative conclusions.

It is true that the tools of NHST have mostly been used in social cognition research to promote negative conclusions. But this

is true only because of the predominance of negative conclusions in the literature. Computers and pencils have also been used primarily in the support of negative conclusions, for the same reason, but this does not make them inherently negative tools. Similarly, the use (and misuse) of NHST is equally amenable to supporting positive conclusions as negative ones. NHST can and has been used to promote positive conclusions, to dispute negative conclusions, and to promote balanced approaches and conclusions.

One available tactic in using NHST to support positive conclusions is to install an idealized version of an error in the role of null hypothesis, and then to "show" by rejecting the null hypothesis that the error in question is not real. I will illustrate this with the example of base-rate neglect. Sometimes it is used in a truly balanced way, which is exemplified by its use in the literature of confidence calibration.

The vast literature on base-rate neglect is replete with statistical demonstrations of the effect of just the sort that is criticized by K&F, that is, constructing a normative standard as a null hypothesis, and then showing that mean responding is sufficiently different from that standard to justify rejecting the normative standard as a description of performance. This leads to exactly the problem K&F identify in their Figure 1: pointed (and therefore impossible) rationality alongside a ranging bias.

However, the same tool is available to those of a different perspective. For example, Koehler (1996) drew the influential conclusion that "[w]e have been oversold on the base rate fallacy" (p. 1). One important rationale for this conclusion is that base rates are not, in fact, ignored entirely. Some proponents of the negative approach have come to extreme conclusions, such as "base rate information concerning categories in a population is ignored" (Nisbett & Borgida 1975, p. 935). This has led critics, such as Koehler, to note that people almost never ignore base rates completely. NHST, used to test the null hypothesis that manipulations of base rates have no impact, is guaranteed to show that people are sensitive to base rates, just as tests of a null hypothesis of rationality are guaranteed to show that people are not perfectly rational. Researchers who try to argue that people are insufficiently sensitive (although not totally insensitive) to base rates have been compelled to combat the perception that they are really arguing for a complete lack of sensitivity (e.g., Goodie & Fantino 1996; 2000).

In the literature on confidence calibration, the equivalent tactic would be to report a statistically significant correlation between confidence and accuracy as proof that people are sensitive to their own abilities and limitations. I am glad to be unaware of any research deploying this tactic, although the tool remains available for this kind of misuse. More commonly, though, researchers use NHST for the more appropriate and balanced approach of decomposing Brier (1950) probability scores into components, such as difficulty, calibration, discrimination, and noise (Carlson 1993). Here, NHST is not used in service of either positive or negative approaches, but for quantifying the balance between human achievement and human limitations.

NHST, like any tool, carries the potential for misuse as well as constructive use. It strongly appears to have been used (and misused) more for negative ends than for positive ends, and it is tempting for this reason to suspect that NHST is inherently slanted toward negative ends. But it has served negative approaches predominantly only because negative approaches have been predominant, and have had more occasions to invoke it. It is all to the good to encourage the psychological community to use NHST more judiciously, but this is an improvement that would likely be separate from any shift in the balance between positive and negative approaches to psychology.

Another route to broadening the scope of social psychology: Ecologically valid research

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Abstract: An imbalance is identified in social psychology between controlled experimental studies (which are common) and real-world, ecologically valid studies (which are rare). The preponderance of experimental studies (which provide mere existence proofs and lack realism) helps fuel social psychology's fault-finding focus. Laboratory experiments and ecological studies should be pursued jointly to examine social life in the real world.

Krueger & Funder (K&F) highlight an imbalance in social psychology, persuasively arguing that researchers devote far more attention to how people get things wrong (e.g., bias) than to how people get things right (e.g., accuracy). Here, I highlight another imbalance in social psychology, mentioned only briefly in K&F's article: that between the highly controlled experimental studies that characterize much of social psychological research, and real-world, ecologically valid nonexperimental studies, which are rather rare. I argue that this methodological imbalance contributes to the broader substantive imbalance lamented by K&F.

Thanks in part to the influence of social cognition, modern social psychology has become closely associated with one particular method – carefully controlled laboratory experiments. Although most, if not all, social psychological research is inspired by real-world events, it has become standard practice to control reality out of the picture. As has been argued elsewhere (e.g., Funder 1999; Kenny 1994; Rozin 2001), this over-reliance on laboratory experiments can limit the real-world relevance of the research. Two specific features of experiments fuel the field's fixation on finding faults.

First, experimental research is concerned with existence proofs. That is, laboratory experiments show only what *can* happen under some circumstances, but, without some ideas about the everyday context of the phenomenon, the experiments say very little about what *will* happen in the real world. Laboratory experiments tell us what is possible, not what is likely. To map out where the limits of possibility lie, experiments tend to focus on the points at which processes (e.g., decision making) break down. Consider the research programs on majority and minority influence. Essentially, these programs tell us merely that there exist circumstances under which the majority can influence the minority (e.g., Asch 1956), and there exist circumstances under which the minority can influence the majority (e.g., Moscovici 1980). However, the pinpoint focus of experimental studies on either the majority or the minority allows researchers to study these intrinsically related processes independently, bypassing the incontrovertible fact that in the real world, every time a minority exists so too does a majority. And, by overlooking the real-world contexts in which these social processes typically occur, the limit-testing experimental paradigm draws attention to the boundaries, where things break down, and neglects the question of how often these boundaries are approached by the circumstances of everyday life. If the conditions promoting majority or minority influence occur very rarely, then the negative influence of the majority or the minority could be practically inconsequential. Experiments show us where the boundaries of possibility lie, but social psychologists now need to map the real-world topography within those generous boundaries.

Second, the artificial experimental stimuli in experiments do not permit tests of accuracy. In their reasonable attempts to exert control, experimental social psychologists typically rely on artificial stimuli (e.g., vignettes), so that the variable of interest can be manipulated while keeping all other variables constant. Although useful from the standpoint of identifying causal processes, the

stimuli are not real, so they cannot serve as criteria against which accuracy can be evaluated; thus, in a vignette about Bob, "there is nothing *accurate* you can say about Bob, because Bob never existed" (Funder 1999; p. 15). In our own research on the perception of individuals' personalities based on their bedrooms (Gosling et al. 2002), we could have provided observers with artificial bedrooms, changing just one element at a time (e.g., making the clock fast or slow) to examine the effects of that one element on the observers' perceptions. However, because these bedrooms would not belong to real individuals, we would not have been able to test the accuracy of the observers' perceptions (e.g., were there really differences between people with fast vs. slow clocks in their bedrooms?). To test accuracy (but not to test bias), real targets are needed. Thus, a preponderance of experimental research tends to limit research foci to negative (e.g., bias) rather than positive (e.g., accuracy) findings.

Two points should be acknowledged: Some ecologically valid research is being done in social psychology, and experiments can, in principle, also be used to examine positive processes. However, social psychologists appear to have a preference for control over realism and, as K&F have noted, social psychologists also seem to have a penchant for the negative.

Even if laboratory experiments predispose social psychology to focus on negative rather than positive findings, I do not advocate simply replacing experiments with real-world ecological studies. An over-reliance on either method paints an incomplete picture. The two methods need to be used in concert to identify which causes have an impact and how they operate in the real world. Ultimately, social psychologists need to study social beings in the contexts of their social worlds. K&F propose analytical and theoretical routes to achieving a more balanced social psychology. To these, I propose adding a methodological route, in the guise of a greater emphasis on ecological validity. Bringing at least some of the current research out of the cubicle and back into the street can further broaden the scope of social psychology.

Is social psychological research really so negatively biased?

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Abstract: Krueger & Funder (K&F) overstate the defects of Null Hypothesis Significance Testing (NHST), and with it the magnitude of negativity bias within social psychology. We argue that replication matters more than NHST, that the pitfalls of NHST are not always or necessarily realized, and that not all biases are harmless offshoots of adaptive mental abilities.

Krueger & Funder (K&F) recommend, as an alternative to NHST, a form of Bayesian analysis that incorporates effect sizes. The main advantage of this analysis is that rationality is no longer a null hypothesis vulnerable to rejection with ample N; instead, rationality is accorded a probability of its own that any alternative hypothesis of bias must justly surmount. In principle – and assuming that all terms in the calculus can be plausibly specified – this is a good strategy. However, the fact that the long-flogged horse of NHST is not yet dead suggests that there is some use for the old nag yet (Abelson 1997). K&F criticize NHST for violating *modus tollens.* However, given that statistical inference is a form of induction, should it be expected to conform to the rules of deduction?

Let us explicate what we believe is a solid rationale for NHST. The purpose of NHST is to set standard criteria – collectively agreed upon by members of the scientific community – that must be met by any putative effect before it can be provisionally admitted into the Pantheon of the Real (Insko 2002). By way of analogy, consider a gambler who repeatedly beats the odds at a casino at $p < .05$. He may just be having a lucky streak; logically, there is no way of disproving it. Nor does his performance provide any way of computing the exact probability that he is cheating. Nonetheless, if casino managers adopt the policy of excluding such individuals, they will save money by identifying some genuine cheaters, despite occasionally showing lucky gamblers the door too. The same reasoning underlies NHST. There is no way to compute the exact likelihood of an observed effect being real, given the data. However, if research scientists adopt the policy of accepting only those effects that consistently meet standard stringent statistical criteria, then they will advance knowledge by identifying some genuine effects, despite occasionally seeing illusory order in chance fluctuations too.

Pursuing the analogy further, suppose a revisionist statistician were to recommend to casino managers that they no longer bar gamblers who consistently beat the odds, but instead, bar gamblers who consistently win a lot of money – in other words, that they pay attention, not to statistical significance, but to effect size. The casino managers would likely be unimpressed. They know that, despite some variability across different casino games, beating the odds and winning a lot of money go hand in hand, as the odds of winning fall within a fairly consistent range. Whatever their criterion of suspicion, the long-term outcome will be the same. Similarly, in psychological science, effect size and statistical significance go hand in hand, because, despite some variability across studies, sample size also falls within a fairly consistent range (with alpha levels being fixed by convention). Ultimately, the key to deciding whether an effect is real is whether it can be *replicated,* regardless of whether the effect is authenticated with p-values or standardized magnitudes (whether or not reexpressed in Bayesian terms). This is why most psychologists believe in cognitive bias but not telepathy: effects attributed to the former can be replicated whereas effects attributed to the latter cannot (Milton & Wiseman 1999).

We also wonder whether K&F have been too quick to dismiss cognitive biases as phantom menaces wrought by NHST. Just because NHST *can* lead researchers to overstate cognitive biases, does not mean that all cognitive bias established by NHST *have* been overstated. K&F suggest that cognitive biases generally are in the same league as visual curiosities, like the Muller–Lyer Illusion, that is, that they are nonconsequential artifacts of otherwise overwhelmingly adaptive mental systems. However, other less innocuous parallels might be drawn. For example, pilots are prone to potentially fatal visual illusions when approaching runways under conditions of reduced visibility (Waldock 1993). If such perceptual glitches were to precipitate a plane crash, would the relatives of the dead passengers be consoled by the knowledge that, in a multitude of respects, the pilots' visual systems were miracles of fine-tuned adaptation? The general point is that the specific pitfalls of a cognitive bias are not rendered inconsequential by the general excellence of parent mental systems from which they derive: they are still worth seeking to counteract in contexts where they are likely to cause harm. We believe that many of the biases K&F list in their appendix can, on occasion, prove highly problematic (Belsky & Gilovich 1999; Sutherland 1994).

Relatedly, although K&F are correct that the discovery of any number of biases need not imply that human reasoning overall is defective (because those particular biases need not constitute a representative sample of human reasoning), it does not follow that every cloudy bias *must* have an adaptive silver lining. By way of analogy again, consider two defects in human anatomy: the possibility of choking on swallowed food and the possibility of developing an inflamed appendix. Both are clearly nontrivial risks to survival and reproduction. The former risk is arguably offset by

the benefit of having an oesophagus that facilitates spoken communication; however, the latter risk does not seem to be offset by any particular benefit. True, at some level of abstraction, an inflamed appendix might be construed as part of an otherwise well-adapted food-digesting organism; however, to assert as much is vague and unsatisfying. The same goes for the assertion that a cognitive bias is part of an otherwise well-adapted mind. Might it not be that some cognitive biases are just unmitigated evils, forms of acute mental appendicitis?

The wrong standard: Science, not politics, needed

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Abstract: Krueger & Funder (K&F) focus on an important problem, but they offer a political rather than a scientific remedy. "Balance" is not our problem; systematic, scientific research is. Only that sort of research will ever lead social psychology out of its current malaise that focuses on positive and negative aspects of human behavior.

I find the lopsided character of social psychology no less offensive than Krueger & Funder (K&F) do, and I appreciate their scholarly effort to turn things around. Nevertheless, it appears to me to miss the central target, namely, the unsystematic, nonscientific nature of social psychology today. The authors' remedy applies the wrong standard; it is not merely a question of *balance*, but creating more research that demonstrates the positive capacities of *Homo sapiens*, thus providing roughly equal numbers of positive and negative conclusions regarding the moral and cognitive attributes of this creature. That's a *political* criterion; there is no scientific or naturalistic reason for the necessity of a balance. We shouldn't expect research to be guided by a search for a point of equilibrium where positive findings match negative ones. It is not mere imbalance that ails social psychology, rather, it is the lack of a *scientific* approach to its subject matter. As the authors' note, at present the field lacks the cumulative character of a serious scientific discipline, and that is where the trouble lies. All this was hashed over a few decades ago when the viability of social psychology as a discipline came under serious scrutiny. But it survived, rescued apparently, at least in part, by the excitement generated by all that negative research that threw the field out of "balance."

But suppose the authors get their wish, and suppose we are indeed presented with a new series of positive findings that reverse our contemporary views. Might that not lead to new questions, such as: Is social psychology merely self-referential – consumed with internal political squabbles of little interest to the broader scientific community? Does social psychology merely cycle between producing positive features and negative features? First, a lot of this, and then, a lot of that? And if that's all that the search for balance gives us, we may well ask: Will social psychology *ever* produce systematic scientific work?

The authors recognize this current danger. Their "central recommendation is that empirical work and theoretical modeling address the whole range of performance" (target article, sect. 4.3.1). So they undoubtedly see the point of a systematic scientific approach. Their theoretical suggestions are given with the aim of producing "balance," however, thus diverting their readers, and failing to lead beyond social psychology's internal problems.

As it happens, social psychology did have its systematists who, regrettably, today only a few will remember, or will have encountered. And they were systematists who knew what they were doing, whose contribution to systematic analysis consisted of more than a brave turn of phrase. A half century ago, David Krech and Richard Crutchfield gave us an excellent start with their *Theory and Problems of Social Psychology* (1948), a book that was intended to provide – and did provide – the systematic approach so-

cial psychology needed then, and desperately needs now, and which is called for by K&F. The first sentence of Krech and Crutchfield's Preface made their goals clear: "This book is designed for the teacher and the student who are interested in the *science* of psychology as a systematic, interpretative account of human behavior (Krech & Crutchfield 1948, p. vii, emphasis in original).

But a half century later, all we can say is that, despite the excellence of the effort, it did not succeed. We don't know why it didn't; we now have a scattered, incoherent discipline, filled with disconnected studies. Nevertheless, the effort by Krech and Crutchfield was useful, for it allows us to contemplate the fact that, a half century later, we do not have what is wanted. Perhaps we should simply conclude that, although our sympathies lie with K&F – they are asking many of the right questions – their standard is incorrect; they believe that *balancing* our research will improve matters. But, as I indicated above, that is conceptually mistaken, and now we can see that a half century of empirical evidence also goes against the value of their standard. It appears that social psychology is a discipline that has stumbled onto a series of interesting phenomena that, so far, elude systematic scientific inquiry. But such phenomena will *always* elude systematic scientific inquiry, *as long as we categorize them as we do now.*

Of course, it is easy to call for a new organization of the materials of a discipline, or semidiscipline, but providing that organization is an endeavor that will not be easy, and thus, it is an endeavor this commentator will hastily abjure. (But see Hammond & Stewart 2001, for an even more grandiose attempt.)

So, if we are to achieve a systematic approach, as Krech and Crutchfield did in fact achieve, the reader will have to figure out his or her own new concepts and categories of phenomena that will lead, not merely to a balance, but to a new scientific discipline, which may or may not be called "social psychology." And that is what the reader should be doing; rethinking the concepts and categories that define and guide the social psychology of today, with the aim of developing new ones, rather than conducting research that will restore an unnecessary balance.

Beyond balance: To understand "bias," social psychology needs to address issues of politics, power, and social perspective

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Abstract: Krueger & Funder's (K&F's) diagnosis of social psychology's obsession with bias is correct and accords with similar observations by self-categorization theorists. However, the analysis of causes is incomplete and suggestions for cures are flawed. The primary problem is not imbalance, but a failure to acknowledge that social reality has different forms, depending on one's social and political vantage point in relation to a specific social context.

There is much to like about Krueger & Funder's (K&F's) article. It takes a broad view of the discipline of social psychology and raises timely questions about metatheory and practice. Moreover, some of its more contentious observations are undoubtedly correct. Over the last 30 years, the cognitive branches of social psychology *have* become increasingly fixated on issues of bias, and research into some topics – most notably stereotyping and social judgement – has essentially been defined by the desire to catalogue "basic" cognitive deficits that can be held responsible for pernicious forms of social behaviour.

Like K&F (and Asch 1952; Sherif 1966, before them), we believe that the bias agenda is unproductive and has had a distorting impact on our discipline and on its analysis of social problems (and hence on the remedies it proposes). Indeed, in common with

other self-categorization theorists (e.g., Turner et al. 1994), this belief has informed most of our research for the last twenty years. Accordingly, it was used as a basis for critiquing the large body of social cognitive research into stereotyping and for developing and testing an alternative metatheoretical perspective. This argues that stereotyping and related group processes are not the product of cognitive bias or collective irrationality, but of adaptive sense-making processes which serve: (a) to represent important social realities from the perspective of membership in particular social groups, and (b) to allow those groups to engage in meaningful forms of social action.

An example is provided by research into inter-category accentuation, that is, the tendency to represent distinct groups in black-and-white terms rather than shades of grey (e.g., Krueger & Rothbart 1990; Tajfel 1969). Haslam and Turner (1992; 1998) suggest that, rather than representing a distortion of the individual properties of stimuli, such accentuation serves to represent *veridically* their higher-order properties as members of social groups perceived from a particular vantage point. We argued that such judgments only appear distorted if one accepts the individualistic doctrine that the truth about people resides in their individuality, and if one privileges a single perspective (e.g., that of the researcher or "independent" judge) in one's appraisal of accuracy. In contrast, self-categorization theorists argue that there are higher-order social realities and distinct social and political perspectives, which stereotypes and social judgments *need* to represent, if they are to allow the perceiver to function effectively in the social world (see Oakes et al. 1994; Spears et al. 1997). It is not hard, for example, to see why it would have been problematic – not just psychologically, but also *politically* – for Black South Africans in the Apartheid era to see all South Africans as individuals and to accept the "objective" judgments of the white judiciary, which sought to invalidate perceptions that were consonant with Black Consciousness. Haslam and Turner (1998) used the following court exchange involving the Black activist Steve Biko to illustrate this point:

> *Judge Boshoff:* But now why do you refer to you people as blacks? Why not brown people? I mean you people are more brown than black.
> *Biko:* In the same way as I think white people are more pink and yellow and pale than white.
> [General laughter in the court]
> *Judge Boshoff:* Quite . . . but now why do you not use the word brown then?
> *Biko:* No, I think really, historically, we have been defined as black people, and when we reject the term non-white and take upon ourselves the right to call ourselves what we think we are, we have got available in front of us a whole number of alternatives, . . . and we choose this one precisely because we feel it is most accommodating. (Biko 1978/1988, p. 121)

In treating Biko as a fool, the judge here takes the line of most cognitive social psychologists in suggesting that accentuated judgment (seeing things as black-and-white rather than brown-and-pink) misrepresents reality by exaggerating its true nature. But, providing we share Biko's political goals, we can see that it is the judge who is the fool.

Yet, while there are important points of contact between the work of self-categorization theorists and the arguments of K&F, we would note that there are still significant residual differences. Most pointedly, we do not believe that the bias agenda has arisen primarily as a result of social psychologists' failure to survey a full range of behavioural responses, and hence, that it will be remedied by statistical or other strategies that attempt to correct for this limited sampling. Like social cognitivists, Judge Boshoff was not at fault because he did not have access to enough information of the right sort. Indeed, if he had had more information, it seems likely that (from our perspective) he would have interpreted that incorrectly, as well. Instead, then, the primary problem lies in his very limited interpretation of the data *that he already had access to.* And what is driving this? Problems of negative emphasis? Of non-Bayesian inference? Of lack of balance?

It is none of these. Rather, we can see that the limitations of the judge's perspective were a direct reflection of his in-group's ideology and political imperatives. Likewise, in social psychology, the bias agenda can be traced to ideological priorities which reify a particular definition of social reality – one which sees the truth about people (whether perceivers or perceived) as lying in their status as isolated individuals, rather than as members of functioning social groups who need to act in relation to a specific social context (Oakes et al. 2001; Turner & Oakes 1997).

Significantly too, it is apparent that in K&F's own Utopian future they still retain the belief that there is *a single transcontextual reality,* which can be uncovered by appropriate statistical and behavioral testing. Psychologically, this conviction seems highly questionable. On political grounds, we are generally motivated to favour one version of social reality over another (i.e., ours) and to present this as *the* truth, but in order to do justice to social psychology, we need to understand that the social world is comprised of *multiple* realities. So, although as political agents we may favour Biko's version of reality over Boshoff's, in order to make progress as social psychologists we need to understand that, for the people and groups who endorse such worldviews, there are competing realities here. In short, the path to progress lies in an appreciation of the interplay between psychology and social context that creates these realities, rather than in attempting to achieve some artificial balance in a decontextualized psychology.

The same, incidentally, is true of classic studies of visual perception. To make sense of what happens in an Ames' room, for example, we need to understand that the visual world *really is* different for participants and for detached observers. In research of this form, of course, there is no debate about which of these two worlds to privilege when labeling one set of perceptions "right" and the other "wrong," and so we have no political difficulty achieving a "balanced" psychology of perception. But the social world typically isn't like this – as members of different social groups we have different values, norms, ideologies, and cultures. In other words, we have different *social perspectives.* Moreover, as the history of social cognition research demonstrates, when the differences between these are downplayed, it is the values and perspective of more powerful groups that tend to be privileged in arbitrating over error and accuracy, and the balance between the two (Hopkins et al. 1997; Spears & Smith 2000).

So, as K&F suggest, let us celebrate social psychology as veridical and adaptive, rather than error-prone and error-ridden. But let us accept that this requires an appreciation of differences in social perspective and in associated psychological truths – as well as appreciation of the political and sociostructural reasons for these differences – rather than an a priori commitment to balance. If we do not, we suspect that social psychology will continue to lose its way in an array of baffling conundra and seemingly paradoxical phenomena, and will simply substitute one set of problems for another. For when the labels "truth" and "error" are attached to different phenomena by members of different groups, methodological criteria alone will never resolve the thorny questions of how much balance is enough, and who has the right to decide.

Out of the theoretical cul-de-sac

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Abstract: A key premise of the heuristics-and-biases program is that heuristics are "quite useful." Let us now pay more than lip service to this premise, and analyse the environmental structures that make heuristics more or less useful. Let us also strike from the long list of biases those phenomena that are not biases and explore to what degree those that remain are adaptive or can be understood as by-products of adaptive mechanisms.

Let us waste no more time quarrelling over the diagnosis. Krueger & Funder (K&F) are right. Social psychology and related fields have oversold violations of behavioural and cognitive norms. This state of affairs was foreseeable, and not only with the benefit of hindsight. For instance, back in 1982, Kahneman and Tversky explicitly acknowledged that "although errors of judgment are but a method by which some cognitive processes are studied, the method has become a significant part of the message" (1982, p. 124). Since then, the method has become the most significant part of the message.

It is thus high time the message that human reasoning is "ludicrous," "indefensible," and "self-defeating" be counterbalanced. But balance is not the only reason to rethink social psychology's research agenda. Even more important, as K&F point out, is the fact that the hunt for behavioural and cognitive flaws has led us to a cul-de-sac. Discovering another bias, error, violation, or illusion is a much less original, let alone theoretically fruitful, contribution today than it was 20 or 30 years ago. K&F list a number of promising routes out of the dead end – we add some related ones.

Let us at last pay more than lip service to a key premise of the heuristics and biases program that Tversky and Kahneman (1973) articulated in their original framing of the availability heuristic:

> Availability is an *ecologically valid* clue for the judgment of frequency because, in general, frequent events are easier to recall or imagine than infrequent ones. (p. 209, our emphasis)

Assuming availability is ecologically rational (rather than irrational), how the heuristic reflects the structure of the environment should have been explored, but it was not. Instead, since the heuristic was proposed 30 years ago, countless papers have implicated it in countless biases – illusory correlations, unwarranted optimism, eyewitness identification errors, discriminatory biases, and hindsight bias, to name just a few. To the best of our knowledge, however, not a single paper has fleshed out how the availability heuristic may exploit ecological texture to estimate event frequencies, although this kind of analysis is precisely what is needed to predict the conditions under which it succeeds – and fails. The research program on fast and frugal heuristics demonstrates how the mapping between heuristics and environmental texture can be analysed (Gigerenzer et al. 1999). There is no reason why the heuristics to which many biases have been attributed cannot be subjected to such analysis, even if it requires more clarity about the underlying processes.

There is another, related route to change. This is to examine the long list of cognitive biases by asking the following three questions about each one.

Is the bias really a bias? There are several reasons why a cognitive phenomenon might have to be taken off the list of biases. Take the conjunction fallacy as an example. Virtually no one doubts that Tversky and Kahneman's (1983) Stanford undergraduates violated the conjunction rule when they judged Linda to be more likely a feminist *and* bank teller than only a bank teller. But does that mean that their students committed the conjunction fallacy? No. Semantic and pragmatic ambiguity led many of them not to reason according to the conjunction rule. In particular, the students had to infer what the experimenters meant by semantically and pragmatically ambiguous words such as *probability* and *and.* In doing so, they may have arrived at legitimate meanings that differ from mathematical probability (Hertwig & Gigerenzer 1999) and logical AND (for different views on this issue, see Mellers et al. 2001). It is ironic that while many psychologists continue to interpret the outcome of semantic and pragmatic inferences as evidence of biased reasoning, others struggle to design artificial agents capable of inferring, for instance, which of multiple meanings of a polysemous word is appropriate in a given context. To them, designing systems that can "process language as skillfully as we do will signal the arrival of truly intelligent machines" (Jurafsky & Martin 2000, p. 6).

Is the "bias" a design flaw or a built-in adaptation? Several researchers have recently argued that biases in (social) judgments may be design features rather than design flaws of the human mind (e.g., Haselton & Buss 2003; Nettle 2004). Take, for example, Bjorklund's (1997) argument regarding children's overconfidence in their competence. Children appear to misjudge their abilities on a broad range of cognitive tasks. How might such systematic miscalibration be adaptive? Bjorklund proposed that overrating one's ability has motivational benefits at a point in development at which one's behavioural and cognitive repertoires are extremely limited, and each novel task could be daunting. If children in this situation "rationally" assessed the difficulty of a task and their task-related skills, trying their hand only if they appeared to have the requisite skills, then they would never explore many novel tasks and territories. In fact, by avoiding tasks likely to overtax their skills, children would miss out on important opportunities to learn new things.

Is the "bias" a cheap price to pay for an adaptive mechanism? Even if a bias is not an adaptive feature, it may be a by-product of an adaptive mechanism. Take the hindsight bias as an example: Many researchers have stressed its detrimental consequences (e.g., Fischhoff 1982). In a recent model of the processes underlying the hindsight bias, Hoffrage et al. (2000) suggested that the hindsight bias is a by-product of a memory system that updates information constantly and automatically. Specifically, the model assumes that new information regarding the outcome of an event leads to an updating of the knowledge (cues) on which people's original evaluation of the event was based. When people attempt to reconstruct their original judgment, they access the updated knowledge base, opening the door to hindsight bias.

Knowledge updating is adaptive in that it prevents us from using information that, because of changes in the environment, may be outdated. It has a by-product – the hindsight bias. The bias, however, may be a relatively low price to pay for keeping the knowledge in our limited memory up-to-date. Consistent with this view, Hertwig et al. (2003) found that although updating can result in erroneous memories of past judgments (i.e., the hindsight bias), it increases the accuracy of future inferences.

Admittedly, claims about the adaptive nature of either biases or the processes that result in biases need to be carefully scrutinized. But they serve to emphasize that the design features of the human mind, like those of the human body, reflect trade-offs between benefits and costs. It is high time that we accept this simple truth about human cognition, and at last try to understand these trade-offs, rather than dubbing them biases and calling it a day.

Asch and the balance of values

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Abstract: Values will be central to developing a more balanced social psychology. A nonconformist account of Asch's (1956) experiments is used to illustrate the role of multiple values and to support and extend Krueger & Funder's (K&F's) claims. A balance of values, one that goes beyond accuracy and truth, and that avoids absolutism and relativism, is needed.

Krueger & Funder's (K&F's) call for a more balanced social psychology is a challenge to be welcomed. My comments, intended to support and sharpen their claims, will focus on values, which they suggest will require renewed attention if balance is to be achieved (sect. 5). First, a "nonconformist" account of Asch's (1956) studies will be offered to illustrate K&F's criticisms and recommendations. Second, some difficulties for addressing values will be briefly noted.

Contra K&F (sect. 2.2.2), Asch designed his experiment precisely to counter the view that people are "sheep" (Cialdini & Trost 1998). He thought that if there was unambiguous physical information available, people should and would say what they saw with-

out respect to what others said. Ceraso et al. (1990) have reported that social psychologists of the time were shocked by "early returns" from Asch's studies showing that people "stick to their guns" (p. 8). Even Asch's (1956) final results provide more compelling evidence for truth-telling than for conformity. If Milgram's (1974) basic study had been described like Asch's has been, the focus would be entirely on the 35% of participants who refused to continue. Why do we emphasize the minority responses in Asch and the majority responses in Milgram? K&F's complaint of pervasive negativity is an excellent candidate.

A crucial reason for the story having unfolded the way it has is that Asch adopted a zero-tolerance norm (sect. 2.2.1). Asch (1952) thought he had created a simple moral dilemma between *truth* and *consensus,* in which truth was good and consensus was not. To agree with the unanimous majority on even a single trial was to have erred. But perhaps Asch's imputation of error was rash (sect. 3.1). Campbell (1990) argued that consensus is a good (he calls it *trust*), and that it should be integrated with other values (e.g., truth) to guide behavior in the Asch situation. He proposed that it would be most rational to believe that the majority is correct, but that the most moral action would be to offer one's dissenting view.

Hodges and Geyer (submitted) have suggested that Asch's analysis is simplistic and Campbell's is unrealistic. Interpreting the Asch situation in terms of values and conversational pragmatics, they ask: How does one speak the truth in a complex, tense, and frustrating situation? How does one answer the experimenter's questions in a way that simultaneously honors one's peers, the experimenter, one's own perception, and the situation in which all are embedded? Although any one trial prevents a resolution, over 12 critical trials, the actors in the Asch situation can try to balance as best they can their differing obligations. This means that they might occasionally give an incorrect answer, not because they are gullible, cowardly, or incompetent, but as an implicit signal that they have "heard" the majority and that they are open to further conversation despite the sharpness of the disagreement. By engaging in a local error, actors may be communicating a larger truth about the tension of their multiple obligations and their frustration in realizing all of them.

If this analysis is correct, then there is an irony in Asch's work, which, like the paradox of the fundamental attribution error (FAE; sect. 3.1.3.1), deserves to be "savored like a fine Merlot." It is this: Asch could see that his own deception – the design of the experiment – was part of a larger quest for truth, yet he would not accord his participants the same latitude.

Whatever the merits of the foregoing hypothesis, it illustrates K&F's call for more positive, balanced approaches (sect. 4) to social cognition and action, and for considering whether the norms by which behavior are judged are "incomplete, wrong, or misapplied" (sect. 3.1.2). Furthermore, it attempts to attend to the whole range of behavior (sect. 4). Most explanations of Asch's experiments are so fixated on explaining "conformity" that they overlook Asch's two main results: the preponderance of dissenting responses and the enormous range of responses. Hodges and Geyer (submitted) hypothesized that there might be three different strategies for integrating truth, consensus, and other values, suggesting that together these differing strategies would provide for group survival better than any one strategy alone. Their hypothesis illustrates K&F's suggestion that "multiple norms may need to be considered" (sect. 3.1.2). As Funder puts it elsewhere: Situations are complex, generating multiple motivations, such that "life is a continuous struggle to balance them all and find some kind of workable compromise" (Funder 2001b, p. 23).

Moving toward a more balanced social psychology that understands behavior as guided by multiple values will be difficult. Asch (1990) noted that a "central" theme of his research had been "that there is an inescapable moral dimension to human existence. . . . Yet psychologists have been among the most determined opponents of this claim" (p. 53). Thus, the open discussion of values K&F call for (sect. 5) will not come easily.

K&F briefly acknowledge the difficulty in their reference to the

debates that have emerged about decision-making norms (sect. 3.1.2). Finding the balance they call for (sect. 4.3.2) will require negotiating some middle way between the enlightenment rationalism that tempted Asch (Leyens & Corneille 1999), and the subjective relativism that tempts them (i.e., participants' own goals define what is right; sect. 3.1.2). If values are simple and obvious, no discussion is needed; if they are merely what individual psychologists "consider desirable" (sect. 5), no discussion is possible. Discussions, as Asch realized, require real constraints and real obligations. In fact, his purpose in doing the experiments was to demonstrate that clear physical constraints and real moral obligations make rational behavior possible.

What obligations – which K&F refer to in Lewinian terms as "force fields" (sect. 4.3.2) – frame social relations and provide the basis for judging our actions and decisions (Hodges & Baron 1992; Sampson 2003)? Asch thought truth was our primary obligation. K&F emphasize accuracy. Truth and accuracy are crucial to human survival, but there is more that needs to be included if we are to flourish. For a start, there is compassion (sect. 5).

The goodness of judgment index

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Abstract: Evidence is presented indicating that mainstream social psychology material leads undergraduates to conclude that people are irrational. To help address the problems identified by Krueger & Funder (K&F), a new statistic, the Goodness of Judgment Index (GJI), is presented. A concrete example based on a recent study is used to show how the GJI can be used to bring some balance back to research emphasizing error and bias.

Krueger & Funder (K&F) are right in almost every one of their particulars. People raise decent families, create wonderful works of art, invent computers and cell phones, hold warm and enjoyable gatherings among friends, figure out how to regularly travel at over 600 miles per hour, teach their children how to walk, talk, and ride bikes, create vast universities for educating young adults, and so forth. How could all this possibly occur if people did little more than engage in one immoral behavior after another, and, when not subordinating, torturing, or murdering one another, went about making the dumbest decisions imaginable?

I realize that no social psychologist has ever written anything quite as starkly damning as the prior sentence, but much of the body of work of social psychology leads to an impression consistent with that stark, dark, prior sentence. I recently taught an honors introductory social psychology class – these are among the most intelligent and thoughtful students in psychology. Their readings prior to the midterm included: Myers's (1996) introductory social psychology text's chapter on the self, Aronson's (1999b) *Social Animal* chapter on self-justification, Merton's (1948) classic article on self-fulfilling prophecies, La Piere's (1934) "attitudes do not predict behavior" study, and two chapters each from Cialdini's (1993) book on social influence, and Ross and Nisbett's (1991) book on the person and the situation. These are well-respected and mainstream social psychological writings.

One midterm essay question was, "According to social psychological research, are people mostly rational or mostly irrational?" Three quarters of my students concluded that social psychology demonstrated that people were mostly irrational. See the following examples.

First student, introductory sentence: "Through taking this class, I have come to the conclusion that people are, and have always been, primarily irrational."

Second student, introductory sentence: "People are not rational beings; rather they are rationalizing beings."

Third student, concluding sentence: "I guess that we are probably irrational and spend our lives trying to convince ourselves that we are rational."

This testifies to just how little insight social psychology has provided into how people do things well.

K&F have made many profound points, and I would like to augment one. Specifically, they highlighted how, in most studies, accuracy or rationality is the null hypothesis. It is, therefore, impossible to obtain evidence of accuracy or rationality, because it is impossible to "confirm" the null hypothesis. They are right about this, but there is another problem with null hypothesis testing in such research. The statistical comparison is usually deviation from perfection (whether that perfection means zero difference between experimental groups or deviation from predictions of some sort of normative model).

Unfortunately, however, perfection is so high a standard that researchers rarely apply it when testing their own models, theories, and hypotheses! If the results confirm the hypotheses on two out of three dependent variables in Study One; and then reappear on three out of four dependent variables in Study Two, the paper will most likely get published and the tone will emphasize confirmation of the author's theory.

This situation is explicit in use of goodness-of-fit indices when testing structural equation models. One of the earliest tests of model quality was the chi-square test, which evaluated whether the correlations or covariances predicted by the hypothesized model significantly deviated from the observed correlations. With large samples, the chi-square is almost always significant, however, indicating that almost all hypothesized models are imperfect. It is not often very useful, however, to know that one's proposed model is not perfect. Typically, we want the model to be good, not perfect.

This inspired some researchers (e.g., Bentler & Bonnett 1980) to develop measures of fit that indicate how much the model does explain. One common fit measure is the Normed Fit Index (NFI). For the NFI, one assesses the improvement in fit obtained in one's hypothesized model, as compared to that of the null model – one which assumes no variable is related to any other variable. The NFI is computed by:

$$\frac{(\text{chi-square[null model]}) - (\text{chi-square[hypothesized model]})}{(\text{chi-square[null model]})}$$

If the hypothesized model has a chi-square of 80, and the null model has a chi-square of 1,000, the NFI equals .92. The model may be significantly worse than perfect, but this result can be metaphorically interpreted as meaning that it accounts for about 92% of the covariances among the observed variables.

In the same spirit, I propose a Goodness of Judgment Index (GJI) for studies of error and bias. Most results in studies of judgment, decision-making, and prediction can be readily translated into a 0–1 scale, because such studies use frequencies, percentages, or correlations as their basis for identifying error and bias. The GJI then becomes:

$$\frac{(\text{Maximum possible imperfection} - \text{actual degree of imperfection})}{\text{Maximum possible imperfection}}$$

"Maximum possible imperfection" is the most anyone could possibly be wrong under the circumstances. Imperfection can be operationally defined as errors, discrepancies from predicted values, discrepancies from perfect correlations, and so forth.

The GJI is simple to use and indicates the proportion of improvement of social judgment compared to complete error. Scores above .5 mean that the judgment is closer to complete rationality or accuracy than to complete error or irrationality; scores below .5 mean that the judgment is closer to complete error or irrationality than to complete rationality or accuracy.

Consider the following data from a recent study on bias in social perception (Monin & Norton 2003). During a community water crisis, Princeton students were asked to stop showering for about two days. The researchers surveyed Princeton students, and asked them: (1) How many showers they took during the ban; and (2) what percentage of other Princeton students showered during the ban. I only focus on Day 3 of their results, although the basic pattern holds throughout. During Day 3 of the ban, 47% of their respondents admitted to taking showers. Bathers predicted that 66% of Princeton students took showers; nonbathers predicted that 47% of Princeton students took showers. Interpretation of the results focused almost entirely on error and bias.

Their results, however, provided more evidence of accuracy than of bias. One does not need the GJI for the nonbathers – their prediction of the proportion of bathers was dead-on accurate (47%), and their GJI would be 1.0. Even the predictions of the bathers were more rational than irrational. Because 47% of the students bathed, the greatest possible imperfection would be 53% (which would occur if 100% of the students were predicted to bathe). The observed imperfection among bathers is 66% – 47% = 19%. GJI = (.53 – .19)/.53 = .64. So, the bathers' judgments, which clearly showed a false consensus effect, were closer to complete accuracy than complete inaccuracy.

I hope that K&F's article inspires researchers studying bias to develop tools to estimate, not just the degree to which people make biased and irrational judgments, but also the degree to which they make unbiased and rational judgments. When data are consistently presented in a balanced manner, we will have taken a major step toward a balanced social psychology.

Building an even better conceptual foundation

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Abstract: Krueger & Funder (K&F) spend too much time on their critique of some classic studies in social psychology. They should have spent more time developing their constructive ideas about better methodologies and, especially, better conceptual foundations for the field. We endorse their exhortation to consider social behavior in its ecologically adaptive context, and we present a few ideas of our own about how to develop a more comprehensive conceptual framework.

Krueger & Funder (K&F) are unhappy with two traditions of research in social psychology. They provide a biting and occasionally witty critique of classic demonstrations of misbehavior (over-conformity, over-obedience, and failures to help) and the heuristics and biases approach to social cognition. Many of their insights into weaknesses of these approaches have merit, although their relentlessly negative perspective vastly undervalues the enormous positive contributions of research from these traditions. Any school of behavioral research can be subjected to scathing criticism; indeed, the authors' own *Realistic Accuracy Model* is also limited in many ways.

Acknowledging that there is something to learn from a critical evaluation of these major pillars of the social psychological canon, we are more interested in K&F's affirmative suggestions for improvements in methods and theory. Most of their suggestions are methodological, and we have already expressed our enthusiasm for the correspondence framework for the analysis of degrees of accuracy (see Hastie & Rasinski 1987, for an introduction to the correspondence–coherence distinction, a discussion of "methodological logics" for judgment research, and a comment on the manner in which the null hypothesis test has obstructed the study of social judgment). And, like K&F, we find the Bayesian framework to be conceptually superior to the Neumann-Pearson null

hypothesis testing approach. But, their essay does not help us understand the persistent popularity of the traditional approach. Perhaps, there is an important lesson to be learned from the failure of the Bayesian approach to catch on in any major scientific field.

Because we are in essential agreement with K&F's methodological imperatives, we would like to focus our attention on their suggestions for a stronger conceptual foundation for the field of social psychology. K&F view social behavior as central to human adaptation. They argue that it is essential to place social cognition in an interpersonal context and to evaluate its overall adaptive success by a cost–benefit analysis. For example, referring to Funder's (1995) and Kenny's (1994) frameworks for social judgment and personality perception, K&F emphasize that social interactions are an ecologically indispensable ingredient of social cognition. Social interactions determine what types of information are available and relevant to a perceiver, and prescribe the appropriate standards of accuracy by which to evaluate social judgment. K&F also note that in the two traditions they criticize, "The paradigmatic study presents social stimuli directly to participants, thus bypassing relevance and availability completely, and bypassing the task of cue detection. Traditional studies of social cognition concern the utilization stage exclusively" (sect. 4.3.3.2, para. 4).

We agree that considering interpersonal processes is essential to a more ecologically balanced picture of social behavior and cognition. But, we believe that K&F's recommendation about how to salvage social psychology still does not effectively banish the ubiquitous bias toward the study of "individual minds operating in a social vacuum," which has haunted cognitive social psychology. For example, Funder's Realistic Accuracy Model does not consider the rich context of multiple, partially redundant, substitutable social cues. (Why else would they say that "accuracy is a difficult and remarkable achievement" [sect. 4.3.3.2, para. 4] and indicate that the level of accuracy can be predicted by a simple multiplicative calculation?) Nor is it clear where the promised cost–benefit analysis fits into the framework (see their Fig. 2). General criticisms of such individual-focused frameworks have been raised elsewhere (e.g., Nye & Brower 1996), and we will not repeat them. Instead, let us sketch our conception of a more comprehensive framework for social interdependence that extends K&F's suggestions for a more balanced social psychology.

Everyone agrees that the ultimate goal of social psychology is to provide insights and causal theories of everyday social behavior. No social psychologists question this truism. But "social" seems to mean different things to different social psychologists. For some, "social" means being motivated by the immediate *social policy* implications of the research findings. K&F suggest that this motivation is one reason for the emphasis on biases and social misbehavior in some textbooks (cf. Katzko 2002). For others, like K&F, "social" means that the stimulus that is being perceived and judged is another human being; the most social aspect of the framework is an analysis of the agreement and/or disagreement between two perceivers of a target person. And for still others (including us), "social" means adaptive, strategic interaction in a matrix of enduring and shifting social relationships.

The perceiver–target framework is too limited, and it excludes important factors of social motivation and strategic interaction. Without a broader theory of motivation and social interdependence, we fear research will simply continue to produce lists of "effects" and "biases," which under some conditions *may* materialize in interpersonal perception (cf. Table 1 of the target article). Although K&F do not acknowledge it, the heuristics and biases approach to social cognition did more than simply catalogue biases and errors. The underlying conception of the mind, implicit in this approach, includes a "cognitive toolbox" architecture with optional reliance on alternative heuristic judgment strategies. The strategies are associated with fundamental cognitive capacities (memory retrieval, similarity evaluation, causal simulation) that are responsible for the distinctive signature biases that are byproducts of reliance on each strategy (cf. Kahneman & Frederick

2002). Even some of the harshest critics of the heuristics and biases approach have adopted this basic conceptual framework (e.g., Gigerenzer et al. 1999). But, a cognitive architecture is only part of a comprehensive conceptual framework (cf. J. R. Anderson 1990; N. H. Anderson 1996).

We think that K&F's recommendation to consider the *ecological context* of social behavior should be taken more seriously. Only a few social psychologists have grappled with the adaptive character of social interactions. Indeed, we see little evidence that K&F have seriously addressed these issues. However, this challenge has been accepted by behavioral ecologists who study animal behavior (e.g., Dawkins & Krebs 1978; Hauser 1996). Interaction and communication among animals are often deceptive and manipulative, as well as cooperative. And, even some of the most mysterious animal social behaviors can be understood as solutions to the adaptive problems of securing essential resources, such as food, mating opportunities, social power, and so forth (Byrne 1995). This is no different for humans! Game theory and Evolutionary Game Theory provide truly comprehensive frameworks for understanding the adaptive essence of social interaction (e.g., Gintis 2000; Maynard-Smith 1982). These approaches come with powerful analytic and simulation tactics for theory building, as well as original observational and experimental methodologies. More than 25 years ago, Kelley and Thibaut (1978) attempted to introduce social psychologists to Game Theory, but their effort was unsuccessful. We think social psychology has made a major error by myopically ignoring these important and productive approaches. Without more comprehensive foundations, frameworks like the Realistic Accuracy Model will continue to generate superficial lists of "descriptive patterns," but miss deeper insights into the causes of social behavior.

We can point to a few illustrations of the kind of research we advocate. Camerer (2003) provides an accessible and profound introduction to the aspects of Game Theory most relevant to social psychology (and reading Kelley & Thibaut 1978, is still instructive). Kameda et al. (2003) report on an example study of the development of adaptive social norms, and Kameda and Nakanishi (2002; 2003) report on cost–benefit analyses of social conformity. We applaud K&F's goal of promoting the development of a balanced social psychology. But, we want to exhort social psychologists to take their adaptive theme further. Even limited target–perceiver theories, like the Realistic Accuracy Model, need a more comprehensive foundation that deals with interdependencies among social agents.

One path to balance and order in social psychology: An evolutionary perspective

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Abstract: Consideration of the adaptive problems faced by our ancestors suggests functional reasons why people exhibit some biases in social judgment more than others. We present a taxonomy consisting of six domains of central social challenges. Each is associated with somewhat different motivations, and consequently different decision-rules. These decision-rules, in turn, make some biases inherently more likely to emerge than others.

Social psychologists do often seem obsessed with listing the cute and unconnected stupid errors that people make. There may be reasons for the perennial focus on the negative and unexpected, but we agree that this focus has (1) made it difficult to see the overarching functions underlying these biases and "mistakes," and (2) hindered the development of integrative theories of social bias.

Table 1 (Kenrick & Maner). *A taxonomy of social problem domains, with associated decision constraints, and resultant biases*

Social problem domain	Evolved decision constraints (examples)	Resultant cognitive biases (examples)
Coalition formation	Exchange relationships are ultimately beneficial to the extent that exchange partners (a) share genes, or (b) can bet on future reciprocation	Self-serving biases should generalize to those who share our genes; people should be biased to quickly detect cheating by nonrelatives, and to make strong attributions about cheaters
Status	Owing to sexual selection based in differential parental investment, men tend to compete with one another for status more than women do	Men should overestimate the competitiveness of other men; make dispositional attributions for competitive behavior
Self-protection	Out-group members pose a consistent source of competition and physical threat	People should have low thresholds for detecting signs of anger or threat in out-group members
Mate choice	Mating opportunities are low cost for men, potentially higher cost for women; male commitment is key for female reproductive success	Men tend to overestimate female sexual interest; women tend to underestimate levels of male commitment
Relationship maintenance	There are cost asymmetries associated with sexual versus emotional infidelity for men and women	Men and women might overestimate likelihood of partner's sexual versus emotional infidelities, respectively
Parental care	Parents have lower future reproductive potential than their children do	Parents might overestimate children's contributions/efforts; children underestimate parents' contributions/efforts

Why and when do people exhibit biases? And how might all those wacky biases fit into an organized and cohesive framework? A consideration of social psychological biases in light of evolutionary considerations can do two things: (1) suggest the particular content areas where one would expect to find particular types of bias, and (2) suggest a more integrative taxonomy of the different types of bias.

We have elsewhere suggested that all human beings need to solve a set of adaptive problems in different social domains (Kenrick et al. 2002; 2003). As outlined in Table 1, our ancestors needed to: (1) form and maintain coalitions, (2) strive for status, (3) protect themselves from harm, (4) select mates, (5) maintain romantic relationships, and (6) care for their children. Each domain involves distinct problems, and each is linked to a unique set of evolved decision constraints. Indeed, what social psychologists have traditionally labeled as biases often represent decision-rules that, on average, would have helped our ancestors survive, prosper, and ultimately reproduce (cf. Funder 1987; Krebs & Denton 1997).

Some biases suggested in Table 1 are backed by empirical data. Others are hypotheses based on considerations of the relative costs and benefits people commonly confront within each domain. For example, consider the domain of mate choice. Evolutionary theorists have suggested that because men have a lower level of initial obligatory parental investment than women do, there are relatively lower costs and greater benefits associated with short-term sexual partnerships for men, as compared to women (Kenrick et al. 1990; Trivers 1972). Indeed, for men, the potential reproductive benefits of a short-term sexual partnership tend to outweigh the potential costs. As a result, men often exhibit biases designed to facilitate the procurement of short-term relationship partners. For example, men tend to overestimate female sexual interest (Abbey 1982; Haselton & Buss 2000; Maner et al., under review).

On the other hand, throughout evolutionary history, a woman's reproductive success has hinged on her mate's willingness to commit energy and resources over the long term. For women, mating with a noncommittal man could prove a costly error, indeed. Consequently, a woman should exhibit biases designed to help avoid romantic encounters unless she is relatively sure a man is willing to commit to her. Indeed, evidence suggests that women tend to underestimate men's willingness to commit (Haselton & Buss 2000). Thus, both men and women exhibit biases designed to maximize benefits and minimize potential costs when engaging in short-term romantic partnerships.

Unlike most mammals, otherwise sexually unrestricted human males also tend to maintain long-term relationships and invest heavily in their offspring. In turn, one might expect men who are committed to long-term relationships to exhibit biases designed to help them maintain their relationships. For example, committed men tend to devalue attractive alternatives to their current partner (Johnson & Rusbult 1989). That is, as compared to uncommitted men, committed men tend to judge other women as less attractive. Because excessive exposure to attractive women can undermine commitment (Kenrick et al. 1994), this bias may help men resist otherwise attractive infidelities.

Next, consider the need for protecting oneself from physical harm. Throughout human evolutionary history, members of competitive out-groups have posed a consistent source of threat. As a result, we should expect people to exhibit biases designed to reduce the possibility of harm from out-group members, because failing to identify a possible threat is generally a more costly error than falsely identifying one. Indeed, evidence suggests that when people are in fear-eliciting circumstances, they report more negative threat-related out-group stereotypes (Schaller et al. 2002) and see out-group members as angrier and more threatening (Maner et al., under review).

There are important trade-offs associated with almost any type of social behavior. Highlighting the adaptive costs and benefits associated with particular behaviors can reveal the ultimate functions social biases are designed to serve, as well as the contexts in which they are most likely to occur. An evolutionary framework is particularly useful for organizing biases that would have, on average, ultimately maximized our ancestors' reproductive outcomes. Indeed, merging a functionalist-evolutionary perspective with traditional theories of social bias can pave the way for a more integrated social psychology.

Is there a "People are Stupid" school in social psychology?

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Abstract: This commentary notes the emergence of a "People are Stupid" school of thought that describes social behavior as mindless, automatic, and unconscious. I trace the roots of this "school," particularly in the link between situationism in social psychology and behaviorism in psychology at large, and suggest that social psychology should focus on the role of the mind in social interaction.

The history of psychology is sometimes presented as a contest between various schools, which provided frameworks for theory and research during its early years (Hilgard 1987; Thorne & Henley 1997). These include: the structuralism of Wundt and Titchener, the functionalism of James, Dewey, and Angell, the psychoanalysis of Freud, the behaviorism of Watson and Skinner, the Gestalt psychology of Wertheimer, Kohler, and Koffka, and the humanistic psychology of Maslow and Rogers. Although not so closely identified with particular charismatic leaders, the cognitive, affective, and neuroscientific paradigms that have emerged more recently within psychology at large, and social psychology in particular, may also have some of the qualities of schools. Krueger & Funder (K&F) do not come right out and say it directly, but their target article provides ample evidence of the emergence of yet another school in psychology – one which I have come to call the "People are Stupid" school of psychology (PASSP).

The school consists of a number of theorists who tend to embrace three distinct propositions about human experience, thought, and action. (1) *People are fundamentally irrational:* In the ordinary course of everyday living, we do not think very hard about anything, preferring heuristic shortcuts that lead us astray; and we let our feelings and motives get in the way of our thought processes (e.g., Gilovich 1991; Nisbett & Ross 1980; Ross 1977). (2) *We are on automatic pilot:* We do not pay much attention to what is going on around us, and to what we are doing; as a result, our thoughts and actions are inordinately swayed by first impressions and immediate responses; free will is an illusion (e.g., Bargh 1995; Gilbert 1991; Wegner 2002). (3) *We don't know what we're doing:* When all is said and done, our behavior is mostly unconscious; the reasons we give are little more than post-hoc rationalizations, and our forecasts are invalid; to make things worse, consciousness actually gets in the way of adaptive behavior (e.g., Nisbett & Wilson 1977; Wilson 2002).

As K&F's review suggests, PASSP is heavily populated by social psychologists; curiously, cognitive and developmental psychologists seem less inclined to embrace such a monolithic view of human experience, thought, and action. It is not completely clear why this might be so. K&F may well be right that social psychologists' emphasis on bias and error is to some extent a natural consequence of their emphasis on null hypothesis statistical testing, where rational, conscious, deliberate social behavior is the hypothesis to be confirmed, and lapses from prescriptive norms are valued as evidence of how things actually work. But because everybody engages in null hypothesis significance testing, this does not explain why social psychology fell head over heels for "people-are-stupid-ism." Certainly, a focus on provocative and counterintuitive findings helps social psychologists maintain their course enrollments, and helps distinguish "scientific" social psychology from the common-sense social psychology of our grandmothers.

To some extent, PASSP seems to have arisen in reaction to the cognitive revolution within social psychology, which emphasized the role of conscious, deliberate thought in social interaction at the expense of feelings, drives, and impulses (Langer et al. 1978). As such, it shares its origins with the affective counterrevolution

(Zajonc 1980), which sought to replace cold cognition with hot cognition, if not to abandon cognition entirely in favor of affects and drives. PASSP acquired additional force from the resurgence of biology within psychology. Evolutionary psychology explains human thought and behavior in terms of instinctual tendencies carried in the genes (Buss 1999), whereas social neuroscience (Ochsner & Lieberman 2001) can slip easily into a reductionism that eliminates the mental in favor of the neural – which is one good reason to prefer the term social *neuropsychology* (Klein & Kihlstrom 1998; Klein et al. 1996). There is also something about conscious awareness, deliberation, and choice that seems to make some social psychologists especially nervous. They feel they need to get rid of it so they can have a completely deterministic account of their domain – just like a real science (Bargh & Ferguson 2000).

But there are even deeper roots of social psychology's preference for the thoughtless, the unconscious, and the automatic. Somehow, fairly early on, social psychology got defined as the study of the effect of the social situation on the individual's experience, thought, and action (Bowers 1973). Think, for example, of the classic work on the "Four A's" of social psychology: attitudes, attraction, aggression, and altruism; think, also, about the history of research on conformity and compliance, from Asch and before to Milgram and beyond. In each case, the experimenter manipulates some aspect of the environment, and observes its effect on subjects' behavior. Sometimes there were inferences about intervening mental states, but not very often – otherwise, the cognitive revolution in social psychology wouldn't have been a revolution. Almost inevitably, the emphasis on how people are pushed around by situational factors led to a kind of "Candid Camera" rhetorical stance in which social psychologists' lectures and textbooks focused inordinately on just how ridiculous – how *stupid* – people can be, *depending on the situation* – a situation that, in many cases, has been expressly contrived to make people look ridiculous and stupid.

In turn, the doctrine of situationism in social psychology found a natural affinity with the behaviorism that dominated elsewhere in academic psychology (Zimbardo 1999). Watson and Skinner actively rejected mentalism (Skinner 1990), while classical social psychology mostly just ignored it. Behaviorism, with its emphasis on stimulus and response, did not survive the cognitive revolution, but the "positivistic reserve" (Flanagan 1992) that was part and parcel of behaviorism is still with us. As a result, we grudgingly accept intervening mental states and processes as necessary to the explanation of behavior – but we want them to be as mechanical as possible. We've replaced both the black box and the ghost in the machine with a clockwork mechanism that is as close to reflex activity as we can get and still pay lip service to cognitivism (Ross & Nisbett 1991). In a theoretical environment in which social behaviors are automatically generated by mental states that may be preconscious, and which in turn are evoked automatically by cues in the social situation (Bargh 1990), interpersonal behavior may not be precisely mindless, but it might just as well be. We had a cognitive revolution for this – only to be told that Skinner had it right after all?

K&F suggest that we can solve the problem of social psychology by restoring balance between the positive and the negative, between accuracy and bias, and between accomplishment and error. They also call for an expansion of theory to encompass both positive and negative aspects of social relations. Both suggestions are well taken, but there is another one that might be considered, as well. That is to change the definition of social psychology itself, from the study of *social influence,* with its implication of unidirectional causality from situation to thought behavior, to the study of *mind in social interaction,* with an express focus on the reciprocal interactions between the person and the situation, and between the individual and the group (Bandura 1978; Bowers 1973). In this way, social psychology can link psychology with the other social sciences, just as biological psychology links it to the natural sciences.

Not just a passion for negativity

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Abstract: The Krueger & Funder (K&F) article would gain in constructive value if the authors spelled out what role the heuristics-and-biases approach could play in balancing the field of social cognition, lowering the burden of blame on it, cautioning overly enthusiastic readers from championing the "enough-with-the-biases" movement, and acknowledging that not all biases are caused by minorities.

We agree with Krueger & Funder's (K&F's) main suggestion that cognitive social psychologists should pay greater attention to the full range of cognitive performance, including both achievements and failures, rather than concentrating on the negative side alone. We think that the article would gain in constructive value if the issues presented next were discussed in greater depth.

Where does the balance lie? The "heuristics and biases" (H&B) approach, the main subject of the target article, has rarely received a balanced treatment. On the one hand, it is praised by many as "psychology's leading intellectual export to the wider academic world" (Tetlock & Mellers 2002). On the other hand, it is accused of propagating fictitious "bleak implications for human rationality" (Cohen 1981, p. 317). It has also been described as a conceptual dead end, an empirical cul-de-sac and a surrogate for theory (Gigerenzer 1991; 1998). The target article argues that the H&B tradition has (a) produced a procession of cognitive errors, including the use of erroneous or misapplied norms, (b) is logically, theoretically, and empirically incoherent, (c) has led the social judgment field to theoretical isolation and incompleteness, and (d) has only limited implications. Given this critical view, one may wonder whether the authors see any positive role at all for the H&B approach in the emerging "balanced social psychology"? Can anything be salvaged from the old negative paradigm? At some point, when describing the Realistic Accuracy Model (RAM; Funder 1999), the authors suggest that: "it implies that accuracy is a difficult and remarkable achievement" (sect. 4.3.3.2, para. 4). Some readers sympathetic to the H&B approach might construe this sentence as a compassionate (or, positive) way to pass along a major (negative) insight from the H&B paradigm. After all, it is impossible to recognize how remarkable an achievement occasional accuracy is, without first appreciating to what extent human judgment is prone to error. In any case, an explicit discussion of this point would greatly reinforce their argument.

Burden of blame. K&F attribute the perennial problems of current social cognition research to a passion for the negative. The problems they list are: (1) creation of countless experimental effects (i.e., biases and errors), which are (2) theoretically fragmented and often contradictory, and (3) appeal to the counterintuitive. Clearly, these problems exist in current social psychology, but should the blame fall squarely and entirely on the passion for the negative? (See Kahneman 1991.) In attempting to understand the sometimes uninspiring image of current social psychology, Kruglanski (2001) recently presented a very similar list of perennial problems[1] for *the entire field of social psychology* (including areas of research which are unaffected by the negativity paradigm), but attributes these problems to structural weaknesses in the field, such as the diminishing role of theoretical statements and the retreat from bold theorizing.

Passion for negativity? Does the passion for negativity (or the desire to add new exhibits to the overcrowded "Museum of Incompetence") drive current social cognition research? We still believe (in the methodological spirit of Kahneman & Tversky 1982) that non-normative responses are an excellent tool to shed light on basic cognitive processes that would have gone unnoticed otherwise (although, clearly, this is not the *only* way). We believe that K&F's praiseworthy intent is to encourage researchers to study cognitive achievements rather than deter them from further exploration of non-normative responses (as almost everybody seems to agree nowadays, non-normativeness does not necessarily mean unadaptiveness). However, we are somewhat apprehensive that this artfully written article could be (mis)read as a plug for an "enough-with-the-biases" movement. We fear that a cognitive social psychology that would classify new experimental results into a two file-cabinet system, one labeled: "findings that (apparently) show that we are smart" and the other as "findings that (apparently) show that we are stupid," would not only be intolerant, but also shallow.

A small minority? Finally, a major methodological point in the article is that the use of NHST (null-hypothesis significance-testing) allows for non-normative responses, that is, responses that only a small minority of subjects identify as such, to be declared general biases: "In some cases, this allows biases to reach significance level even when the modal response is identical with the demands of the normative model" (sect. 2.4.2, para. 2). Admittedly, we take this somewhat personally, because the specific example is taken from our own lab: "See, for example, Klar and Giladi's (1997) report on the 'Everyone-is-better-than-average effect.' Although most participants recognized the definitional truth that on average, people are average, the significant minority that erred, erred in the same direction, thereby yielding a difference between the average judgment and the modal judgment" (target article, Note 10)

In fact, Klar and Giladi (1997) asked students from Tel-Aviv University to compare a totally anonymous student to the average student of their university on a number of desirable traits (e.g., friendliness). To demonstrate the scope of the bias, the authors reported, in addition to conventional *p* values, the frequencies of responses. In the female sample, a small majority (53%) indeed responded in accordance with the "definitional truth," but a sizable minority (42%) thought that this anonymous student would be above the group's average (an additional 5% thought that she would be below it). In a follow-up male sample, 61% gave the non-normative response. Hence, the non-normativeness in these studies cannot be dismissed as having been caused by a small minority. Rather, what is even more telling is the fact that 90% of the participants in small intact groups, highly familiar with everyone else in the group and in highly favorable judgment conditions, provided a non-normative overall response when asked to compare their peers one-by-one to the average peer in their small group (Klar & Levi 2003). Thus, we are afraid that K&F chose the wrong example to prove their case (although they might be right in other instances).

NOTE
1. These problems are: (1) "Inventing new (or distinct) names for old (or same) concepts" (p. 873); (2) fragmentation (p. 873); and (3) attraction for "empirical stuff, in particular of the 'cute' variety" (p. 871).

The "reign of error" in social psychology: On the real versus imagined consequences of problem-focused research

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http://www.psych.uiuc.edu/people/faculty/kruger.html
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Abstract: Krueger & Funder (K&F) make the familiar accusation that social psychologists focus too much on what people do wrong, rather than on what they do right. Although there is some truth to their charge, their accusations are overstated and their conclusions are incorrect. The field is far less problem-focused than they suggest, and the proposed consequences of this approach are more imagined than real.

Krueger & Funder (K&F) make the reasonable, albeit familiar (cf. Funder 1987; Krueger 1998c) accusation that social psychologists

focus too much on what people do wrong, rather than on what they do right. Although one could point out, that in making their charge, K&F *themselves* focus too much on what is wrong with the field rather than what is right with it – a paradox one could presumably "savor . . . like a fine Merlot" (sect. 3.1.3.1, para. 4) – the fact remains that the authors are onto something. However, their accusations are overstated, and their conclusions, incorrect.

A biased critique of bias research. The field is far less "problem-seeking" than the authors suggest. A quick glance at any contemporary social psychology textbook or journal will reveal that there is a substantial amount of research with a decidedly positive (or, at the very least, neutral) spin. True, literature searches for the terms "*error*" and "*bias*" yield more hits than the terms "*strength*" and "*virtue*" (target article, Note 7), but the term "*accuracy*" yields more hits than any of those words.[1]

Even work within the heuristics-and-biases tradition is considerably less negative in its conclusions than the authors claim. Rather than succumbing to the habit, common among pre-1896 vision researchers, of interpreting illusions as products of "flawed psychological processes that need to be fixed" (sect. 1, para. 5), researchers in this tradition have instead argued that judgmental shortcomings stem from generally valid and adaptive tools (Nisbett & Ross 1980; Tversky & Kahneman 1974). In fact, the very optical illusion metaphor advocated by the authors has been proposed before – by precisely the researchers the authors accuse of failing to grasp it: "Just as we are subject to perceptual illusions in spite of, and largely because of, our extraordinary perceptual capacities, so too are many of our cognitive shortcomings closely related to, or even an unavoidable cost of, our greatest strengths" (Gilovich 1991, p. 2; see also Nisbett & Ross 1980, p. 14).

Real versus imagined consequences. Even if the field were every bit as problem-focused as the authors suggest, note that social psychology is not only a descriptive, theoretical discipline, but an applied one, as well. As such, the goal is not merely to advance understanding of people, but to help them. And it is what people get wrong, not what they get right, that has the greatest potential practical use for society. In short, K&F are correct to draw an analogy between social psychology and biomedical research (sect. 1, para. 6), because in both fields it is the understanding of when and why problems occur – and thus, how to avoid them – that is of paramount importance.

Why, then, do the authors object to problem-focused research? First, they object on the grounds that it "yields a cynical outlook on human nature" (sect. 1, para. 3). Whether true or not, we wish to point out that whether a finding is flattering or unflattering is hardly a criterion of science.

Second, the authors argue that by focusing on human shortcomings, social psychologists stunt the development of theory. We are curious about the data on which the authors base their claim. Surely, it is not the actual amount of research and theory development engendered by problem-focused research, which is considerable. True, if it were the case that "the typical article shows that people can be induced to do something objectionable or think in a way they should not" and "stops there, short of asking *why* such a behavioral or cognitive tendency exists, or what general purpose it might serve" (sect. 1, para. 4), then we might share the authors' concern. But this is hardly the case. Indeed, the theoretical paper cited in the pages of the *Journal of Personality and Social Psychology* (*JPSP*), more than any other (according to a recent meta-analysis by Vidal et al. 2003), asks precisely this question (Taylor & Brown 1988), a fact of which the authors are presumably aware, given that one of them is a well-known critic of this work (Colvin et al. 1995). It is paradoxical, given the authors' thesis, that, whereas Taylor and Brown emphasized the positive implications of judgmental errors, Funder and colleagues emphasized the negative implications.

Finally, the authors criticize problem-focused research for touting "contradictory biases," as if doing so is a logical fallacy (such as Kruger & Dunning's [1999] argument that the unskilled overestimate themselves, whereas the highly skilled underestimate themselves). This is perhaps the most suspect charge of all. Most coins, after all, have two sides. Some people work too much, others too little. Some people are optimists, whereas others are pessimists. And, yes, some people overestimate themselves, whereas others underestimate themselves. The existence of one tendency does not, as the authors suggest, imply the lack of existence of the other. What is particularly curious about the charge is the fact that so-called contradictory biases typically lead to the investigation of moderating variable(s) and underlying processes that explain them (e.g., Blanton et al. 2001; Epley et al. 2002; Klar & Giladi 1997; Kruger 1999) – precisely the sort of theory development the authors claim is lacking.

Final thoughts. Although we have been critical of the target article, we wish to emphasize that we agree with the authors on several points. There probably *is* a negative research emphasis in social psychology, and we agree that merely cataloging errors with little consideration of how they fit within a broader context would be problematic. That said, we cannot help but wonder what the field would look like if social psychologists actually took the authors' advice. No longer would the field focus on norm violations or counterintuitive findings. No longer would we fear "bubba psychology" and "golden fleece" awards - instead, we would embrace them. We are reminded of the frequent charge that the news media focuses too much on what's wrong with the world instead of what's right with it, which begs the question, would you really want to read a report titled "*This just in . . . everything's super!*"? We invite readers to ask the same question of social psychology.

NOTE
1. According to a PsycINFO abstract field search, July 3, 2003.

Accuracy *and* error: Constraints on process models in social psychology

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Abstract: In light of an historical obsession with human error, Krueger & Funder (K&F) suggest that social psychologists should emphasize the strengths of social perception. In our view, however, absolute levels of accuracy (or error) in any given experiment are less important than underlying processes. We discuss the use of the process-dissociation procedure for gaining insight into the mechanisms underlying accuracy and error.

In February of 1999, four New York police officers ordered West African immigrant Amidou Diallo to freeze in a darkened alcove. Shortly thereafter, the police officers shot and killed Diallo, believing that he had waved a gun at them. They were mistaken. The object that Diallo held up was not a gun at all, but rather, his wallet. Most people, including Krueger & Funder (K&F), would certainly agree that human beings are capable of making egregious errors – such as those that occurred in the Diallo case – and that it is important for psychologists to study them when they occur. Nevertheless, K&F believe that social psychologists have overemphasized the degree to which people are inaccurate. Should we support their plea to develop research paradigms that are better able to permit the investigation of accuracy?

On the importance of studying accuracy and error. We do not believe that one should be forced to choose between investigating errors *or* investigating accurate judgments. Rather, we are interested in the processes underlying the two types of judgment, which requires that one should study errors in combination with correct responses. Consider an example that is much more mundane than the Diallo case. Two students take a multiple-choice test with instructions to not respond to a question unless they are sure that they know the correct answer. One student produces more

correct responses, but also produces more erroneous responses than the other student does, simply by virtue of responding to more items. Which student knows most about the studied material? To answer that question, one cannot look only at errors, or only at accurate responses. Rather, accurate responses and errors must be *jointly* considered to separate the contribution of knowledge from that of bias, willingness to guess, and, perhaps, a preference for some particular alternative (e.g., alternative "C" on a multiple choice) when guessing. Similarly, we have combined correct responses and errors to estimate the contributions of accurate perception and bias. One "bias" that has been of interest to us is a bias toward racial stereotypes.

In one representative paradigm (Payne 2001; see also Lambert et al. 2003), an image either of a gun or of a small hand-tool is presented, and participants are asked to correctly identify the object by pressing the corresponding key (marked GUN or TOOL). In this paradigm – and unlike many social perception experiments – there is an objective criterion for accuracy. Just before the target object appears on the screen, participants are randomly "primed" with a picture of either a Black or a White face. We find that participants are biased to make stereotype-consistent errors. For example, they are more likely to mistake a wrench for a gun when primed with a Black face, as opposed to a White face.

An important element of this paradigm is that it allows use of Jacoby's (1991) process-dissociation procedure to estimate the relative roles of cognitive control and automaticity in driving behavior. Cognitive control in this paradigm corresponds to participants' ability to respond to the veridical properties of the target, ignoring information from the nonpredictive racial cues. The other parameter, accessibility bias, is relevant to how participants respond in the *absence* of cognitive control. It is here that automatic reactions come into play, determining whether the *gun* response is likely to be chosen differentially, dependent upon racial cues, when participants are unable to fully control their responses (Payne et al., in press).

In this, as well as other, paradigms it is virtually meaningless to ask whether people are accurate or not. Indeed, we have found that overall accuracy rates can be varied greatly by simply changing the parameters of the task (e.g., giving participants less time to respond). Of greater importance, analyzing the *pattern* of accuracy and errors permits us to address process-level questions. For example, Lambert et al. (2003) used process dissociation in order to shed light on a decades-long debate as to why people show greater reliance on well-learned responses in public settings, otherwise known as a social facilitation effect (Zajonc 1965). Rather than being the result of strengthening of bias (Hull 1943), such effects were caused by a loss of cognitive control.

Whereas the absolute levels of accuracy may change from person to person or from context to context, the basic processes are likely to remain the same, varying only in magnitude. K&F advocate a Bayesian approach as a way of accounting for both accuracy and bias. However, this is a rather descriptive approach, much like the null hypothesis testing it is meant to replace. Process dissociation is one kind of model aimed at quantifying the mental processes at work behind observable outcomes. Other process models, such as signal detection theory, multinomial models, and connectionist models, have recently made entries into the social psychological literature as well. The advantage of process models is that once the basic processes are understood, one can predict and interpret both accuracy and bias naturally from the same underlying framework.

Conclusion. K&F's article is important and timely, and we are largely, although not entirely, in agreement with their main points. K&F charge that social psychologists have devised clever paradigms that paint people as inappropriately foolish. Should social psychologists endeavor to "balance the score" by devising clever paradigms to show higher levels of absolute accuracy? We are not sure that this represents a productive line of inquiry. Social psychologists should not have to choose between emphasizing accuracy *or* errors. The important question is not whether humans

should be portrayed as noble or foolish. Instead, we might do better to focus on models of the processes driving human judgment, and let the portraits emerge as they will.

People actually are about as bad as social psychologists say, or worse

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Abstract: Experimental studies are not representative of how badly people function. We study people under relatively innocuous conditions, where their self-interests are very low. In the real world, where people's self-interests are much higher, people are much worse a good deal of the time (some illustrations are cited). This is often "adaptive" for the perpetrators, but that doesn't make it "good" behavior. That people function so badly in our experiments, where self-interest is relatively minimal, is what is really terrifying.

The overall thrust of Krueger & Funder's (K&F's) article is really "are people as bad, morally and cognitively, as social psychologists say or imply?" They want to say no, the present literature is unbalanced. I agree with many of K&F's analyses of the extant social psychological data; their calls for greater balance and completeness seem well justified. But in some major ways, they are wrong. First, the experiments are unrepresentative, in a way not considered by K&F: In these experiments, very little self-interest is actually at stake for the subjects; in the real world, much more is typically at stake. Consider the subjects in the famous Asch or Milgram experiments (cf. Asch 1956; Milgram 1963; 1974). They won't have to continue to live with the other people in the experiment afterwards. They won't receive promotions, demotions, or firings from them; they won't be accused of heresy or treason or witchcraft by them; they aren't friends they could lose; they won't be cast out to starve. What is so shocking about the Asch and Milgram experiments is that there was so much conformity and cruelty, *given how little the subjects had at stake.*

In real life, people have real self-interest and real passions at stake. The results are quite often horrible. I will only cite a few historical and current examples of the multitude available. None of these concern terrible behavior in wars or massacres, or the Holocaust, which might be (wrongly) written off as "exceptions."

My first example is polygamy: As soon as there were surplus agricultural resources, men in most societies took up hoarding women for themselves, perhaps two or three or four, or more (e.g., harems) if they could. This women-hoarding is "adaptive" for the favored men, but is hard on other men, who then lack mates; it often has made more miserable lives for the women. It is ordinary unkindness.

Also ordinary is the horrible behavior that has been used to control women. Take, for example, the practice of footbinding in China which consisted of crushing, for years, the feet of young girls to keep them small, and unable to sustain walking. X-rays of the feet are horrifying. The practice started with Chinese emperors who wanted to control their harems, but soon spread to prosperous men with multiple wives; it continued to spread throughout society as a requirement for upwardly mobile marriage. By the early twentieth century, in some large areas, *half* the girls were footbound. Everyone accepted the results as "attractive," and mothers argued it was also *healthy* for the girls (it isn't). Another example is the practice of clitorectomy. In modern Africa, millions of girls are clitorectomized to control them sexually; their mothers claim that it is healthy (it isn't). And, of course, killing unfaithful wives has been commonly accepted everywhere.

Slavery lasted for centuries in ancient Greece, with very few moral objections from those who benefited; the Church did not declare slavery immoral. Conditions were often horrible; in the

Laurion silver mines, which supported Athenian democracy, male slaves typically died in 2 to 3 years. People's supporting cognitions were appropriately flawed. Aristotle argued that slavery is justified because slaves come from losers in wars, and losing wars shows that the losers are inferior in merit. Aside from whether "merit" should mean merit in warfare, and whether this "merit" should spread to a man's wives, women, and descendants, consider the following: Is every man on a losing side actually inferior in manly "merit" to every man on the winning side? Of course not. By this logic, the great Trojan Hector "deserved" to be a slave to any warrior on the winning Greek side. Aristotle benefited from slavery, and this corroded his reasoning, making it, I believe, "contemptible."

The examples proliferate. Doctors, through the centuries, were one of the best-educated classes, but, as Montaigne wrote, they did not use formal operational thought. For example, for more than two thousand years, doctors followed the practice of bleeding people, which killed many and cured none; during these centuries, no doctors (an educated class) tested whether bled people actually recovered better than non-bled people; no one proposed it, either, apparently. Self-interest (a doctor has to have something to do) impaired cognition, as it always does.

Until unions formed, employers always paid employees as little as possible, just enough to get workers, and to have surviving children as laborers (the "iron law of wages"). When the English government passed laws against children working for a shift longer than ten hours, manufacturers employing child labor invented the "split shift" (e.g., dinner ends one shift; a new one begins). These (usually evangelical Christian) manufacturers generally thought God wanted them to prosper this way. In much of Asia today, if someone is raped, or steps on a land mine, or is a permanent social leper (untouchable), you don't have to pity them, or help them. They did something in a former life to deserve this (Karma); religious cognition obviates the burden of sympathy. On Wall Street, according to Scott Paltrow in "Heard on the Street," scandals occur and will continue to occur because (1) there's no money in playing straight with small investors (commission regulations); (2) there's money in helping big guys; (3) you're almost never caught; (4) big executives nearly always negotiate no punishment for themselves as part of the settlement with the government (e.g., Sandy Weill, Citibank); and (5) small investors are viewed as contemptible suckers who deserve it (Scott Paltrow, "Heard on the Street," *Wall Street Journal*).

Very few westerners ever trouble themselves seriously over the poverty-stricken conditions of the third-world people whose cheap labor helps support their lives.

These and many other everyday things are, of course, all "adaptive" for the perpetrators; but K&F think "adaptive" for the self and its favored associates means, somehow "generally good." This is K&F's second major mistake, one that evolutionary theorists do not make.

I don't rest my case (or refine it; unfortunately, there isn't enough space for that here). Self-interest makes people worse, and the real world is full of it, *much more* so than in our pallid experimental situations; that people commonly act or think so badly in these experimental situations, only adds to the terrible knowledge we have of ordinary people and human nature in the real world.

Proper experimental design and implementation are necessary conditions for a balanced social psychology

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Abstract: We applaud the authors' basic message. We note that the negative research emphasis is not special solely to social psychology and judgment and decision-making. We argue that the proposed integration of null hypothesis significance testing (NHST) and Bayesian analysis is promising but will ultimately succeed only if more attention is paid to proper experimental design and implementation.

We do subscribe to the basic message of Krueger & Funder (K&F), that there is a negative research emphasis in social psychology and judgment and decision-making, and that this negative research emphasis hinders theory developments, such as programs that try to understand to what extent seemingly maladapted heuristics in laboratory settings may be quite reasonable in real-life settings (e.g., Gigerenzer et al. 1999).

K&F persuasively lay out the allure of such a negative research emphasis. Indeed, it is much more interesting (and, we submit, on average easier, faster, and less expensive) to generate violations of norms or conventions than to explain why they have arisen in the first place. Although we are as surprised as the authors that the persistent emphasis on norm violations has not yet decisively eliminated its allure, we do see evidence that, at least in psychology, the tide is turning (e.g., Gigerenzer 1991; 1996b; Gigerenzer et al., in press; Juslin et al. 2000; Koehler 1996). The target article strikes us as yet another good example of that encouraging trend.

Curiously, but maybe not surprisingly, although the unbalanced view of humans as cognitive misers seems slowly but surely on its way out in social psychology and judgment and decision-making, the heuristics-and-biases program, which seems mostly responsible for the unbalanced view, has during the past decade invaded economics with little resistance (e.g., Rabin 1998; see Friedman 1998 for an early and lone attempt to stem the tide), amidst outrageous claims. To wit, "mental illusions should be considered the rule rather than the exception" (Thaler 1991, p. 4). Sounds familiar?

It is easy to see why the widespread practice of taking the predictions of canonical decision and game theory as an explicit or implicit null hypothesis (e.g., the predictions of no giving in standard one-shot dictator, ultimatum, or various social dilemma games), has facilitated this development. Although the simplistic rational actor paradigm surely deserves to be questioned – and experimental evidence questioning it has generated some intriguing theory developments recently (e.g., Goeree & Holt 2001) – the rational actor paradigm is often questioned by perfunctory reference to the various "anomalies" that psychologists in the heuristics-and-biases tradition claim to have discovered. This negative research strategy nowadays often goes under the name of behavioral economics and finance.

Alleged errors of judgment and decision-making, such as the overconfidence bias or the false consensus effect (or any other choice anomaly of the list provided in Table 1 in the target article), are taken to be stable and systematically replicable phenomena.[1] Rabin (1998), whose article has become the symbolic reference for most self-anointed experts in the areas of behavioral economics and finance, is particularly explicit about it when he says, "I emphasize what psychologists and experimental economists have learned about people, rather than how they have learned about it" (Rabin 1998, p. 12).

Of course, there is no such thing as an empirical insight per se; each and every empirical result is a joint test of some (null) hypothesis about the behavior of people and of the way the test was designed and implemented. Think of the giving behavior in dicta-

tor, ultimatum, or various other social dilemma games, and how it can be systematically affected by social distance (e.g., Hoffman et al. 1996), or think of the dramatic effects that real versus hypothetical payoffs (e.g., Holt & Laury 2002) can have on choice behavior. Or, take the false consensus effect (FCE) that figures prominently in the K&F narrative. Mullen et al. (1985) argued that there was overwhelming evidence in the psychology literature that such an effect existed and that it was rather robust. Dawes (1989; 1990) already questioned the meaning of the FCE as defined then. Interestingly, he found that a more appropriate definition (one which calls a consensus effect false only if one's own decision is weighed more heavily than that of a randomly selected person from the same population) often (but not always) shows just the opposite of what the old definition led to.

Most recently, Engelmann and Strobel (2000) tested the false consensus effect in the way it arguably should be done – with representative information and monetary incentives – and found that it disappears. Similar issues of representativeness of information and selected sampling of problems (as in the context of overconfidence), as well as more fundamental issues of the benefits and costs of certain experimental practices, are at the heart of the controversy surrounding the question of the reality of cognitive illusions (e.g., Gigerenzer 1996b; Gigerenzer et al., in press; Hertwig & Ortmann 2001; Kahneman & Tversky 1996) and, more generally, the negative research emphasis that K&F persuasively attack.

An acknowledgment of the central role of experimental practices for the move towards a balanced social psychology, is curiously absent in K&F's list of suggestions that might get us back to balance. We therefore propose that thinking about methodological issues would be an appropriate addition, for both economists and psychologists, to their two empirical suggestions to de-emphasize negative studies and to study the range of behavior and cognitive performance.

We fully agree with the authors' critique of NHST (see also, Gigerenzer et al. 2004) and find promising the authors' suggestion of integrating NHST with Bayesian concepts of hypothesis evaluation. We caution, however, that the success of such a strategy is crucially dependent on aspects of proper experimental design and implementation, such as the proper construction of the experimental (learning) environment (e.g., appropriate control of the social distance between experimenter and subjects, representativeness of information, and learning opportunities), proper financial incentives, and unambiguous and comprehensive instructions that facilitate systematic replication, among others (Hertwig & Ortmann 2001; 2003; Ortmann & Hertwig 2002).

NOTE
1. The fact that pretty much each and every bias enumerated in Table 1 has a contradictory sibling has escaped the attention of almost all economists.

Multi-process models in social psychology provide a more balanced view of social thought and action

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Abstract: Krueger & Funder (K&F) describe social psychology as overly consumed with maladaptive heuristics and biases. This characterization fails to consider multi-process models of social thought and action. Such models, especially with respect to attitudes, have outlined the situational and individual difference variables responsible for determining when thoughts and actions are relatively thoughtful versus when they are more reliant on mental shortcuts.

In a provocative article, Krueger & Funder (K&F) have outlined what they think is wrong with contemporary social psychology. In their view, the field is too focused on the maladaptive aspects of human thought and action. Among other evidence, they characterize social psychological work as overly focused on the use of mental shortcuts (heuristics and biases) to the exclusion of rational and adaptive thought and action. In this sentiment, they join the positive psychology movement, which aims to focus on human capabilities and talents. Notably, and appropriately, however, the authors caution that an exclusive focus on either the adaptive or the maladaptive is limiting. Thus, they join Spinoza in calling for research on the full range of human thought and action. This is an important point, and one with which I agree. However, the authors have downplayed research traditions within social psychology where such balance is present – at least more present than readers of this target article might suspect.

In making their critique, the authors have captured mainstream work on heuristics and biases fairly well. But, social psychology is more than social cognition, and social cognition is more than work on heuristics and biases (e.g., see the burgeoning work on implicit processes). The authors are aware of this, as they describe numerous "behavioral" effects to help make their point. But, they have largely excluded work that seems inconsistent with their relatively narrow characterization of the field. For example, they imply that the dominant view in work on attitudes and social influence is that attitudes are rationalized after the fact, rather than based on careful thought, and that people often mindlessly go along with the majority view (conformity).

First, consider whether attitudes are invariably rationalized, rather than based on thought. Ever since Gordon Allport (1935) called attitudes the single most indispensable construct in social psychology, researchers have considered both relatively thoughtful and non-thoughtful processes of influence (e.g., see Kelman & Hovland 1953). Indeed, one of the most prominent models of attitudes and behavior is Fishbein and Ajzen's (1975) theory of *reasoned* action. This model, based on subjective utility theory, holds that people's evaluations are determined by the underlying information people have regarding those objects. The popularity of this "reasoned" approach is evident in the fact that Fishbein and Ajzen's 1975 text has been cited over 3,500 times since its publication (similar to the over 3,000 times that the Kahneman et al. [1982] edited reader on heuristics and biases has been cited).

Second, consider whether social influence research has emphasized mindless conformity to the will of the majority. In fact, research has demonstrated that majority influence is not necessarily a mindless endeavor. Rather, hearing what others think can motivate issue-relevant thought that results in changed opinions (e.g., see Burnstein & Vinokur 1975; Harkins & Petty 1987). Thus, conformity to a majority sometimes represents a simple heuristic process, but can also represent an effortful and more reasoned cognitive process. Furthermore, there is a rather large literature documenting the sometimes powerful effects that *minorities* have (e.g., see Wood et al. 1994). Researchers in this area have celebrated the benefits of the divergent thinking that is inspired by minorities, rather than the convergent thinking induced by majorities (Nemeth 1986).

Of course, not all behavior is thoughtful or rational. Sometimes people rely on mental shortcuts and merely conform to majorities. This flexibility is recognized in many contemporary social psychological theories, which postulate that different psychological mechanisms determine judgments and behavior in different situations (moderated mediation). As Fiske and Taylor noted in their 1991 *Social Cognition* text, the field has moved beyond viewing individuals as "cognitive misers," who are inevitably prone to various errors and biases that stem from their limited cognitive capacity, to a model of the individual as a "motivated tactician," who is a "fully engaged thinker who has multiple cognitive strategies available" (Fiske & Taylor 1991, p. 13).

In fact, current multi-process models in social psychology emphasize that behavior and judgment are sometimes based on relatively simple cues and heuristics, but at other times result from an effortful evaluation process.[1] For example, in one study (Petty

& Cacioppo 1984), when students read about a proposed policy that did not affect them personally, they were influenced by the mere number of arguments presented but not by the quality of the arguments. Reliance on a numerosity heuristic led to maladaptive evaluations when the arguments were weak – the more weak arguments there were, the more the students favored the proposal. However, when the same proposal was characterized as impacting the students directly (i.e., of high personal relevance), the process of evaluation changed. Now, increasing the number of arguments was effective only when the arguments were strong. When the arguments were weak, presenting more arguments led to less favorable evaluations – a more rational reaction. Numerous situational and individual difference variables have been shown to moderate the extent of information processing activity in this manner (Petty & Wegener 1998).

These multi-process models (e.g., ELM, HSM, MODE, etc.) were recently compiled in one volume by Chaiken and Trope (1999), but *none* of these more "balanced" approaches is mentioned by K&F. These models are of interest because they can account for seeming paradoxes in the literature. As one example, K&F note that some researchers have demonstrated that judgments can be flawed when people rely too much on individuating information at the expense of useful category information, whereas other researchers have shown that people can be overly reliant on category information. The multi-process models provide an integration of these perspectives by identifying conditions under which people rely on each type of information (e.g., see Fiske et al. 1999).

In sum, K&F have presented an accurate, but incomplete, snapshot of work in social psychology. To be sure, there are numerous studies that point to humans as fallible – especially within the heuristics and biases tradition. But there are other longstanding literatures in the field that present a more complex picture of human thought and action. Consideration of these areas will lead to a more balanced view of the current state of social psychology.

NOTE
1. It is important to note that just because a judgment is thoughtful, it does not mean that it is rational or accurate. Just as mental shortcuts can provide adaptive responses in some situations, so too can thoughtful decisions be tainted with bias.

Social psychological research isn't negative, and its message fosters compassion, not cynicism

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Abstract: Krueger & Funder (K&F) correctly identify work on conformity, obedience, bystander (non)intervention, and social cognition as among social psychology's most memorable contributions, but they incorrectly portray that work as stemming from a "negative research orientation." Instead, the work they cite stimulates compassion for the human actor by revealing the enormous complexity involved in deciding what to think and do in difficult, uncertain situations.

We do not recognize current social psychology in Krueger & Funder's (K&F) indictment. For many years we have taught a "mainstream" introductory social psychology course, and we cover the topics to which K&F devote most of their energies. We begin the course with work on social facilitation, which asks the most basic of all "social" questions: What effect does the presence of others have on behavior? We then move on to social comparison, which addresses the impact of others' opinions, abilities, and emotions, on self-assessments. We also discuss persuasion, compliance, interpersonal attraction, altruism and prosocial behavior, prejudice and racism – the usual list. Although the content of a few of these

topics might be considered "negative" (particularly, prejudice), most are not.

We also teach the "big three" on K&F's list of "disproportionately negative" behavioral topics, but even these are "negative" only in the narrow sense that the behavior of some participants would be criticized by naïve observers. Some people conform in the Asch situation (Asch 1956), and obey orders in Milgram's paradigm (Milgram 1963). At first, this seems very surprising; we agree with K&F that part of the fame of these demonstrations stems from their counterintuitiveness. But what are we to make of these surprising results? No social psychologist of our acquaintance, and certainly neither Asch nor Milgram themselves, drew the "negative" conclusion that people behave badly, and left it at that. Instead, most analysts have tried hard to understand the predicament that the experimental participants experienced, and the conflicting forces operating on them.

Understanding the pressures in the Asch situation as deriving from "normative social influence" (Deutsch & Gerard 1955) in a situation fraught with ambiguity (Ross et al. 1976) makes sense of and humanizes behavior that initially seemed bizarre. Similarly, Milgram's extensive experimental variations (Milgram 1974) lead to a very Lewinian take, one that renders his participants' behavior understandable and not simply "maladaptive." Personally, we favor an account that focuses less than Milgram's on the obedience manifested by participants and more on their difficulty in finding a way to disobey effectively. But the bottom line is the same: Participants were in a very difficult predicament with powerful situational and dispositional forces in play. We do not see here a "negative" view of human nature, but, instead, a nuanced, compassionate one that pays serious attention to both people and their situations.

The work on bystander nonintervention, research conducted with the express purpose of casting doubt on the negative portrayal of bystanders as "apathetic" (Latané & Darley 1970), is caricatured in the target article. Darley and Latané show that the probability that a research participant will intervene to help another is sensitively attuned to a variety of situational variables, all of which make sense. In particular, a person is relatively unlikely to intervene unless the situation is actually defined as an emergency (passive onlookers diminish this likelihood), and the person feels responsible for the outcome (less likely as the number of potential helpers increases). What is "negative" about any of this? Late in the target article, K&F claim that "no theoretical account of a range of behavior is complete without a cost-benefit analysis." But as a direct result of the bystander intervention experiments, most analysts portray the potential helper as facing a sequence of decisions, very much including a calculation of the costs and benefits of intervening or not (Aronson et al. 2002; Brown 1986).

When we turn to K&F's characterization of social cognition work as showing "a focus on inferential shortcomings and errors" (sect. 2.3, para. 1), we can agree that this is descriptively correct. But what is the point of this work, and what conclusions are to be drawn from it? Kahneman (2000) puts it succinctly: "Contrary to a common perception, researchers working in the heuristics and biases mode are less interested in demonstrating irrationality than in understanding the psychology of human judgment and choice" (p. 682). Exactly by analogy with research on visual illusions (as advocated by K&F themselves) so-called errors and biases are regarded as phenomena that yield particularly rich insight into the basic processes of intuitive judgment. In our view, any analysis (Kahneman & Frederick 2002) that finds unity in such diverse phenomena as the conjunction fallacy, duration neglect, and what legal scholars regard as problematic punitive damage awards, is a truly positive contribution indeed.

K&F claim that Tversky and Kahneman "characterized human judgment as 'ludicrous,' 'indefensible,' 'self-defeating'" (sect. 2.4, para. 2). This would be seriously "negative," if true. But a look at the paper in which these "characterizations" appear shows a very different state of affairs (Tversky & Kahneman 1971). What is characterized as "ludicrous" is an "extension of the representation

hypothesis," which would be required to justify participants' erroneous expectation that patterns of experimental results would replicate almost in their entirety, regardless of sample size. What is termed "indefensible" (on logical grounds) is not "human judgment" as a whole, but a very particular (indeed, indefensible) response to a difficult question about how to interpret a partial replication of results. And what is "self-defeating" is the practice of choosing research designs with very low statistical power. These strong adjectives were used, in other words, not to tar the human inferential system in general, but to describe very specific responses to very difficult questions. The point, that people seem to believe in a "law of small numbers," remains true. But to accept this point does not require a broad characterization of the inferential system in negative terms. What it does require is an attempt to understand why such problems are so difficult, and what can be done to ameliorate matters.

K&F call for "a more balanced, full-range social psychology" that might result in "a more realistic and thus a more compassionate view of human nature" (sect. 5, para. 1). But we suggest that a realistic, compassionate view is just what emerges from an understanding of the complexities of situations in which people (sometimes) conform, obey unreasonable commands, fail to intervene in emergencies, and overuse judgmental heuristics. It is difficult to think straight and act right in complex situations; we now understand a great deal about why that is so, and what might be done about it.

Errors of judgment and the logic of conversation

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Abstract: Experimental procedures routinely violate the cooperative principle of conversational conduct by presenting irrelevant information in a way that implies its relevance to the task at hand. This contributes to an overestimation of the prevalence of judgment errors relative to natural contexts. When research participants are aware that the usual norms of conversational conduct do not apply, the emerging errors are attenuated or eliminated.

Krueger & Funder (K&F) highlight social psychologists' fascination with judgmental biases and note that the processes underlying inferential errors in the laboratory may often be adaptive in daily life. This commentary draws attention to one of the variables that contribute to this asymmetry, namely, experimenters' violation of conversational maxims (Grice 1975) that govern cooperative communication in daily life.

Tacit norms of cooperative conversational conduct imply that "communicated information comes with a guarantee of relevance" (Sperber & Wilson 1986, p. vi), entitling listeners to assume that the speaker tries to be informative, truthful, relevant, and clear. Listeners interpret speakers' utterances "on the assumption that they are trying to live up to these ideals" (Clark & Clark 1977, p. 122). Bringing these assumptions to the research situation, participants assume that every contribution of the researcher is relevant to the aims of the ongoing conversation. Yet, the researcher may deliberately present information that is neither relevant, nor truthful and informative – and may have carefully designed the situation to suggest otherwise. Missing this crucial point, participants treat presented "irrelevant" information as relevant to their task, resulting in judgmental errors relative to normative models that consider only the literal meaning of the utterance, but not the implications of the conversational context. These errors are attenuated or eliminated under circumstances that either conform to conversational norms or allow the insight that the usual conversa-

tional maxims do not apply (for extensive reviews, see Hilton 1995; Schwarz 1994; 1996).

For example, Kahneman and Tversky (1973) described a man, said to be randomly selected from a sample of engineers and lawyers, who "shows no interest in political and social issues and spends most of his free time on his many hobbies which include home carpentry, sailing, and mathematical puzzles." Participants predicted that this person is most likely an engineer, independent of whether the base-rate probability for any person in the sample being an engineer was .30 or .70. Clearly, they relied on individuating information of little diagnostic value at the expense of more diagnostic base-rate information, violating Bayesian norms. Does this imply that they did not notice that the description was uninformative? Or did they infer that the researcher wanted them to consider it – or else, why would it be presented to them in the first place? An extended replication of this study supports the latter possibility (Schwarz et al. 1991). When the personality description was provided as a narrative allegedly written by a psychologist, participants again concluded that the person is an engineer, independent of the base-rate. But when the same description was presented as a random sample of information about this person, allegedly selected by a computer from a larger file assembled by psychologists, participants relied on the more diagnostic base-rate information to make a prediction. Thus, participants considered normatively irrelevant information when it came with a conversational "guarantee of relevance," but not when this implied guarantee was called into question.

Similar analyses apply to other judgmental biases that involve reliance on normatively irrelevant information, ranging from the fundamental attribution error, the dilution effect, and the conjunction fallacy to misleading question effects in eyewitness testimony and numerous context effects in self-reports (for a review, see Schwarz 1996). When explicitly asked, participants usually seem aware that the normatively irrelevant information is of little informational value (e.g., Miller et al. 1984), but proceed to use it in making a judgment because the sheer fact that it has been presented renders it conversationally relevant in the given context. Once the "guarantee of relevance" is undermined, the impact of normatively irrelevant information is eliminated or attenuated (Schwarz 1996, Chs. 3–4). Increasing individuals' motivation to "get it right" rarely attenuates reliance on normatively irrelevant information, but merely increases participants' efforts to find meaning in the material presented to them (e.g., Tetlock & Boettger 1996).

Because of these conversational dynamics, the field's favorite procedures foster an overestimation of the size and the pervasiveness of judgmental biases. This analysis does *not* imply, however, that violations of conversational norms are the *sole* source of judgmental biases. Like most robust phenomena, judgmental biases are likely to be overdetermined. If we are to understand their operation in natural contexts, however, we need to ensure that their emergence in experiments is not driven by determinants that may not hold in daily life, where cooperative communication is likely and listeners are often aware of conditions that call the assumption of cooperativeness into question.

From disorder to coherence in social psychology

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Abstract: Krueger & Funder (K&F) presuppose that the base rate for social cognition is more rational than is indicated by research, and that a focus on cognitive errors and behavioral shortcomings is responsible for the fragmented nature of social psychology. Insight concerning both issues is forthcoming from advances in evolutionary psychology and the adaptation of dynamical systems theory to social psychology.

Preparing a commentary on Krueger & Funder's (K&F) article represents something of an irony. Their thesis is that social psychologists emphasize people's shortcomings (mental errors and biases, behavioral weaknesses) to the relative exclusion of people's impressive (fair, insightful) cognitive abilities and proclivity for doing wonderful (moral, purposeful, self-directed) things. Our task as commentators is to identify shortcomings in this thesis, focusing on the questionable aspects of a cogent and well-documented argument. To compound the irony, our basic argument is that the negativity bias in social psychology highlights the adaptive nature of human functioning and provides the basis for coherent theory construction.

Humans clearly represent a successful species. The accomplishments characterizing our comparatively brief tenure in the animal kingdom are testament to our enormous cognitive skills and capacity for acting in a fashion that enhances both personal and group survival. Against this backdrop, it's not surprising that laypeople and scientists alike are fascinated (and often chagrined) by what appear to be lapses in reasoning and weaknesses in conduct. Apart from their figure–ground appeal, perceived shortcomings in people's cognitive and behavioral tendencies draw attention for two reasons, both of which are of value to science and society.

The first reason is practical: By exposing error-prone and undesirable aspects of human functioning, science and society are in a position to minimize their frequency of occurrence or their consequences. The situation is analogous to the disproportionate concern with illness in medical research. Most people don't get cancer, but if we were to let the relatively low base rate dictate research activities, we would not discover means for preventing or curing this affliction. In like manner, social psychologists are professionally concerned with apparent human foibles, such as irrationality and susceptibility to social influence, because these tendencies are associated with personal and social ills (e.g., poor decision-making, racism, social violence).

As K&F note, focusing on problems to the exclusion of normal operation provides a skewed image of people. This brings up the second rationale for emphasizing people's shortcomings: By looking at the ways in which people err mentally and display weakness behaviorally, we can gain insight into the mechanisms that produce apparent lapses in thought and action. The analogy to medicine is telling here, as well. Research spawned by the AIDS epidemic, for instance, has yielded insights into the immune system that might not have been appreciated otherwise. With respect to social psychology, were it not for research into such phenomena as cognitive heuristics, dissonance reduction, groupthink, and deindividuation, theories of mental and behavioral processes might not appreciate basic mechanisms that operate in different ways under specified circumstances. Thus, research on cognitive heuristics and cognitive dissonance has underscored the press for efficiency and evaluative consistency in cognitive processes – tendencies that are responsible for effective decision-making and judgment much of the time. The work on groupthink and deindividuation, meanwhile, illustrates people's penchant for social co-ordination – a feature of human nature selected for in our ancestral environment and crucial to social harmony and efficiency in contemporary society.

K&F express concern that a focus on the ways in which people can go wrong promotes fragmentation in social psychology, with independent mini-theories devoted to separate, narrowly defined shortcomings. A concern with the lack of theoretical synthesis in the field has been voiced in various quarters in recent years (e.g., Buss 1995; Kenrick et al. 2003; Vallacher & Nowak 1994). This very fragmentation, however, has fueled efforts to achieve theoretical synthesis and has resulted in several promising meta-theories. Two purported syntheses in particular – evolutionary psychology and dynamical social psychology – are noteworthy. Both perspectives confirm the functional nature of human thought and action by focusing on apparent exceptions (i.e., nonrational or undesirable manifestations).

Evolutionary psychology (cf. Buss 2004) is explicitly concerned with people's success in meeting adaptive challenges, both interpersonal and environmental. But insights into evolved mechanisms have stemmed in part from research exposing aspects of human nature that seem dysfunctional. For example, the tendency to favor in-group members and to derogate out-group members, revealed in research on social stereotyping and conflict, is a manifestation of a proclivity for forming social bonds and alliances in one's local group that has beneficial (or at least benign) consequences most of the time. In similar fashion, although some unsavory consequences of sexual jealousy, such as spousal homicide, may have received disproportionate attention relative to their base rate occurrence, this research has highlighted the evolved design of human psychology.

The dynamical perspective emphasizes the tendency for systems of interacting elements to achieve higher-order coherence as well as the expression of this self-organization tendency in specific personal and interpersonal contexts (cf. Vallacher et al.). The juxtaposition of specific thoughts and memories promotes the emergence of coherent global judgments on the part of individuals, for example, whereas social interactions in a group promote the emergence of group-level beliefs and values. The failure to achieve personal and interpersonal coherence is distressful and is associated with a host of problems (e.g., ambivalence, in-group conflict). But, although a press for higher-order coherence is functional, it also may qualify as a fundamental principle underlying a wide range of cognitive and behavioral shortcomings. In the attempt to achieve and maintain coherence, people distort or suppress information, show irrational susceptibility to influence, and ostracize or derogate others with different notions of social and physical reality.

The emergence of higher-order coherence is a fundamental (and hence, unifying) feature of complex systems in all areas of science (cf. Strogatz 2003), and there is reason to think that this feature underlies the adaptive and apparently maladaptive aspects of human nature. Laypeople strive for mental coherence, groups of interacting individuals strive for social coherence, and scientists strive for theoretical coherence. In each case, the press for coherence is fueled by disorder and complexity in the system's components. From this perspective, the laundry list of human foibles that K&F decry may provide the elements for a unified view of social thought and behavior – a view that emphasizes our strengths and capabilities as well as our weaknesses and limitations.

Goodness has nothing to do with it: Why problem orientation need not make for parochial theory

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Abstract: Social-cognitive psychologists' problem orientation is, in itself, no threat to the generation of normatively neutral general theory. What would put general theory at risk is, rather, the reliance on a valence-balancing explanatory heuristic. Fortunately, social-cognitive research communities have resources to override this heuristic and utilize more epistemically effective cultural tools.

Psychologists worry about doing their science right. Krueger & Funder (K&F) are worried that their colleagues' fondness for studying (putatively) contranormative phenomena stands in the way of theories capable of explaining "the [entire] range of human behavior, not just the negative end" (sect. 2.2).[1] K&F's epistemic objective is unlikely to raise eyebrows. Despite arguments that "mental competences" are, in principle, beyond psychologists' explanatory reach (Hamlyn 1957, quoted in Fodor 1968; Ryle 1949, p. 326), inquiry has never been restricted to "mental impotences."[2] Even an outspoken critic of the Edinburgh strong program finds its famous principle of explanatory symmetry uncontroversial when read as requiring nothing more than "use [of] the same . . . principles . . . to explain the causation of all beliefs . . . true or false, rational or irrational" (Sokal 1998, p. 16).

What is more problematic than K&F's goal is why, having endorsed the irrelevance of normative evaluation to theoretical explanation, they take it to be an influential factor in theory formation. Why, one might wonder, should the (putative) niceness or nastiness of the phenomena from which a theory was derived matter to its eventual applicability across a range of positively and negatively evaluated cases? Lacking an answer, it is hard to tell how worried social-cognitive psychologists should be about their taste for deviance.

My own hunch is that what K&F have in mind as an obstacle to general theory is not just one-sided concern with (putatively) nonnormative thought and action, but rather, a pairing of this concern with systematic attribution of undesirable outcomes to negatively valenced causes. The combination of negative (−) *explananda* and valence-matched *explanantia* is, indeed, a recipe for theory applicable only to "the negative end" of human activity. Although K&F do not explicitly identify such a heuristic, they do single out for unfavorable comment what might well be seen as an instance of its use – interpretation of (putative) norm violations (−) as resulting from "flawed (−) psychological processes" (rather than as reflecting the operation of adaptive positive (+) mechanisms).[1] It would not be surprising were K&F taking for granted the operation of such a heuristic. Valence matching of cause and effect conforms nicely to the tenets of balance theory (Heider 1958; Insko & Schopler 1972), which is not only a plausible theory about how minds work, but also a fair approximation to how people believe that other people's minds work (Morrissette 1958; Rodrigues & Newcomb 1980).

If what really worries K&F is what happens when a problem orientation is yoked to a valence-matching explanatory heuristic, then giving more attention to admirable aspects of human thought and action is not the answer. Consider: If social-cognitive psychologists looked only at positively evaluated phenomena, but still relied on valence matching in formulating their explanations, they would still produce parochial theory – this time, theory applicable only to "the positive end." Nor would attention to a nicely titrated mixture of (−) and (+) instances do any better under these circumstances: Conjoined with valence matching, it would simply deliver a body of normatively segregated theory. What is wanted is not just theory that explains (+) and (−) instances, but theory that can explain them with the same concepts and principles.

What precludes development of such normatively neutral general theory is reliance on valence matching as an explanatory strategy. The question is, therefore, whether social-cognitive psychologists are wedded, not to the investigation of "problems," but to reliance on a valence matching heuristic.

The answer is fairly cheering: Word has been out on the streets for a while that, under some circumstances at least, adaptive-but-fallible cognitive heuristics can be (and often are) overridden[3] (e.g., Gilbert et al. 1988).[4] Among other devices, professional socialization, critical colleagues, and familiarity with the research literature can facilitate – even enforce – reliance on more costly but more reliable strategies. Halo effect is alive and well in everyday life; methods texts teach standard ways to evade it. Journal referees demand effect sizes; graduate students learn to compute them. Members of research communities have at their disposal a host of cultural tools that complement those conferred by Mother Nature. Hence, social-cognitive psychologists can, and not infrequently do, invoke the same sorts of explanations for favored and disfavored thought and action (e.g., Wilson 2002), and textbook authors not untypically urge undergraduates to invoke the same basic factors in explaining "our propensity to hurt and to help" (Smith & Mackie 1999, p. 581).

Because social psychologists do not go against the world singlehanded and bare-brained, they do not have to rely on a valence balancing explanatory heuristic. Because they are not limited to a strategy that mandates different explanations for vice and virtue, there is no reason why their traditional concern with "problems" presents a threat to the construction of normatively neutral, generally applicable, theory. K&F have done a service in drawing attention to a variety of ways in which the practice of the social-cognitive communities may fall short of their own epistemic standards, but problem-orientation, in and of itself, does not seem to be one of them.

NOTES
1. They are also concerned that the phenomena in question are not as contranormative as claimed, a mistake in judgment that leads to unwarranted gloominess about human nature. This is an interesting but independent issue.
2. Not least because psychologists were typically unaware of the philosophical literature in which these claims appeared.
3. Sampson (1971) suggests that balance theory describes a tendency with substantial adaptive advantages, including provision of "noncontradictory guidelines to action" (p. 131).
4. Were this not the case, K&F themselves would have reason to be profoundly pessimistic about human capacities. It takes more than recognition of their historically adaptive function to make one less gloomy about human hankerings for fat and sugar. You need to know that there are ways to outwit one's craving for crullers.

Balance in psychological research: The dual process perspective

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Abstract: Krueger & Funder (K&F) are right that various imbalances characterize social psychology, but I question whether they are characteristic of psychology or cognitive science as a whole. Dual-process theories, popular in the latter fields, emphasize both processing biases and the adaptiveness of human cognition in a more balanced manner.

It is hard to fault many of the prescriptions in this wise and nuanced target article. Krueger & Funder (K&F) have done an immense service for social psychology with their analysis. Their generic prescriptions for the field, as well as their specific recommendations (such as emphasizing Bayesian approaches over null hypothesis significance testing [NHST]), are, no doubt, overdue. As a prescription for social psychology I can find little to fault.

How much their prescriptions apply to psychology or cognitive science, as a whole, is, however, a much different issue – but a real issue, because the conclusions from this target article will doubtless be generalized beyond social psychology. In general, psychology and cognitive science (as opposed to social psychology specifically) are now, as opposed to a decade ago, much more balanced in their approaches. Work within the framework of the classic heuristics-and-biases approach still continues (e.g., Gilovich et al. 2002), but it is now more than balanced by several flourishing research traditions. Adaptationist and rational modellers, starting with Anderson (1990; 1991) and continuing in the work of Oaksford and Chater (1998; 2001; Chater et al. 2003), have brought many more phenomena under their purview. Evolutionary psychology has produced many prolific research programs (Cosmides & Tooby 1996; Fiddick et al. 2000; Klein et al. 2002; Pinker 1997) and even several textbooks (e.g., Barrett et al. 2002; Buss 1999) highlighting their findings (which, of course, stress the adaptiveness of human cognition). Finally, work on fast and frugal heuristics has resulted in no less than five articles in *Psychological Review,* a *BBS* treatment (Todd & Gigerenzer 2000), and several notable books (e.g., Gigerenzer 2002; Gigerenzer et al. 1999). Cognitive science, in general, as opposed to social psychology, in particular, is characterized by much more of the balance that K&F are seeking.

In various places in their target article, K&F call for research frameworks that (a) assume that "the heuristics that drive human inference are more likely to be part-and-parcel of adaptive cognition than arbitrary design flaws" (sect. 4.3.3.1, para. 2), (b) research that ceases to assume that "human judgment is fundamentally erroneous" (sect. 4.3.3.1, para. 2), and (c) research that seeks greater convergence between social cognition and evolutionary psychology. I think that in cognitive science, in general, such trends are already underway in the development of two-process theories, which have spanned many sub-areas of psychology (e.g., Epstein 1994; Evans & Over 1996; Evans & Wason 1976; Sloman 1996), including social psychology (Chaiken & Trope 1999). My own work on these theories (Stanovich 1999; 2004) has put particular stress on showing how such models help to integrate the insights of evolutionary psychology and of the classic heuristics-and-biases approach.

Such theories stress the evidence from cognitive neuroscience and cognitive psychology indicating that the functioning of the brain can be characterized by two different types of cognitive systems: the autonomous set of systems (known as TASS; see Stanovich 2004) and the analytic system. The autonomous set of systems (TASS) can operate in parallel with each other and with the analytic system. Included in TASS are processes of implicit learning, overlearned associations, processes of behavioral regulation by the emotions, and the encapsulated modules for solving specific adaptive problems that have been posited by the evolutionary psychologists.

In contrast, the analytic system is a serial system which, using working memory as a temporary storage area, retrieves goals and knowledge from long-term memory as well as stored procedures for operating on the goals and knowledge and causing the selection of action. The analytic system and TASS sometimes (because the latter can operate in parallel) provide outputs relevant to the same cognitive problem.

When both systems provide outputs relevant to a decision situation, the outputs most often reinforce each other. However, sometimes TASS responses that represent overgeneralizations must be overridden by the analytic system. For example, there are instances where the responses primed by Darwinian modules represent adaptive responses from conditions no longer present, and thus, they must be overridden by the analytic system if optimal goal satisfaction is to be obtained (Kahneman & Frederick 2002; Slovic et al. 2002; Stanovich 1999; 2004). This way of framing two-process theories provides almost complete rapprochement between the perspectives of evolutionary psychology and the heuristics-and-biases approach.

Evolutionary modules may deliver the correct decision 99% of the time, but for the one percent of errors it may still be critically important to have the correct procedures and strategies for the analytic system to implement – because the one percent of cases may be in domains of unusual importance (financial decisions, personal decisions, employment decisions; see Stanovich 2004 for a detailed development of this argument). The modern world tends to create situations where some of the default values of evolutionarily adapted heuristic systems are not optimal. For example, many heuristic systems biases serve to contextualize problem-solving situations. In contrast, modern technological societies continually spawn situations in which humans must decontextualize information – that is, they must deal abstractly and in a depersonalized manner with information. Such situations require the active suppression of the social, narrative, and contextualizing styles that characterize the operation of the heuristic system (Evans 1982; 2002; Oatley 1996; Stanovich 1999; 2004). For example, many aspects of the contemporary legal system put a premium on detaching prior belief and world knowledge from the process of evidence evaluation. These situations may not be numerous, but they tend to be in particularly important domains of modern life – and they tend to require TASS heuristics to be overridden. This is where there is room for the traditional emphasis of the classic heuristics-and-biases approach – sometimes fast-and-frugal systems lead us astray, because we are in one of the minority of situations where a slow, analytic solution using an explicitly applied procedure is required.

The role of learning in normative and non-normative behavior

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Abstract: There are good reasons why social psychologists have emphasized the negative side of human reasoning. They are simply following humans' tendency to pay particular attention to unusual occurrences. Attempts to refocus attention onto a wider range of behavior should include the influence of learning on both normative and non-normative behavior.

Krueger & Funder (K&F) argue persuasively that social psychology has gone too far in focusing on the negative side of human behavior. Their list of the errors of judgment identified by social psychologists is a lengthy one and certainly makes clear that bias and error are major preoccupations for many of those working in the field. However, as the authors themselves point out, there are good reasons for this preoccupation. Psychologists are subject to the same influences as those who participate in their studies. Perhaps we should not be surprised to observe some of the same biases in their judgments.

As acknowledged by the authors, it seems to be human nature to be more interested in unusual events than in ordinary events. For example, driving between office and home, our attention is captured more by the driver who is rude and cuts us off than by the many drivers who follow the road rules and behave as we expect them to. Unusual events are counted as more informative in a variety of judgment and reasoning tasks (see, e.g., Klayman & Ha 1987; McKenzie & Mikkelsen 2000; Oaksford & Chater 1994). Likewise, we receive attention for research results that violate expectations. Findings that support a popular theory may be published and valued by other researchers in the area; however, they are unlikely to result in the work being featured in *The New York Times.*

The authors suggest that judgments that violate experimenter-imposed norms reflect flawed reasoning no more than displaying perceptual illusions like the Müller-Lyer reflects flawed vision. Perhaps a more apt comparison is with ambiguous figures rather

than with perceptual illusions. Although, in our culture, the Müller-Lyer illusion occurs invariably, there is nothing invariable about the scores of judgment errors listed by the authors. Whether or not judgement errors occur, and which version of the ambiguous figure will be seen, both depend critically on the immediate and historical context in which the task is embedded. We bring rich histories to the task and these histories affect what we see and how we judge. In our view, an acknowledgment of the role of learning is a necessary complement to the authors' explanatory framework.

For example, why do participants in psychology studies demonstrate behavior that is irrational or violates norms? All behaviors, whether labeled normative or non-normative, are influenced by learning. That is, insofar as we use biased strategies for making judgments, it is because we have learned to use those strategies. For example, Goodie and Fantino (1996) demonstrated that a past history of matching (e.g., matching pictures with words, as children learn to do from early childhood) influenced the tendency to neglect base-rate information on a matching-to-sample analog of Tversky and Kahneman's (1982) taxicab problem. When stimuli with no previous association were substituted, performance on the task improved significantly. The base-rate task may be viewed as one involving multiple stimulus control, that is, competition for control by the case-specific cues and by the base rates. Depending on the subject's history, one or the other of these will dominate. The conjunction effect (or conjunction fallacy) also provides a good example of the effect of learning history on a judgment task. For example, a number of investigators (e.g., Abelson et al. 1987; Fantino et al. 1997; Zizzo et al. 2000) have suggested that the effect often results from application of a weighted averaging strategy, or a variant thereof, that people have learned to use in many other judgment tasks (see Anderson 1981). This describes what research participants are doing when their judgments reflect the conjunction effect. However, what influences them to answer normatively? Fantino et al. (2003) report a study in which students performed a repeated-trials conjunction task. In the first phase, participants answered 20 conjunction questions presented in either a likelihood or a frequency format or in mixed presentation. As expected, based on the findings of Hertwig and Gigerenzer (1999), participants were significantly less likely to show the conjunction effect on questions given in a frequency format than on those given in the more typical likelihood format. However, students who received the likelihood and frequency questions in a mixed order apparently were able to carry over the normatively correct approach they used to answer the more transparent frequency questions when they answered the likelihood questions. That is, the performance of the mixed presentation group equaled that of the frequency group. The importance of learning history is also apparent in work on the "sunk cost" effect (also known as persistence or escalation of commitment), an example of non-optimal economic behavior (see Arkes & Blumer 1985). Goltz (1992) manipulated participants' past experience of success or failure on a simulated investment task by controlling returns on the investments they made. She found greater persistence in failing investment strategies to be a function of participants' past rate and magnitude of reinforcement. In addition, she found that participants whose previous reinforcements (i.e., returns on investments made) had been unpredictable persisted longer than those whose investments had previously paid off continuously or had not paid off at all. These results, of course, implicate learning and the subject's prior history. But they also remind us to revisit the importance of situational cues, as emphasized by the authors. Persistent behavior is as likely to be adaptive ("If at first you don't succeed . . .") as maladaptive ("throwing good money after bad"). Depending on the contextual cues and the participant's history, behavior will persist (and will or will not be adaptive). When maladaptive, the behavior may be viewed as the misapplication of learned strategies that have been successful in the past.

In conclusion, the authors take social psychologists to task for emphasizing the negative. Behavioral research on the informa-

tion-seeking behavior of college students suggests that information about negative outcomes is ordinarily avoided (e.g., Fantino & Case 1983), at least when it cannot be used to improve the outcome. From this perspective, one can commend social psychologists for accentuating the negative. Perhaps their willingness to do so reflects their confidence that, not only are these phenomena unusual, but that studying them can lead to improved understanding of human behavior.

Why is ain't ought, or: Is *Homo sapiens* a rational humanist?

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Abstract: Although the critique of disputable norms is largely legitimate in the cognitive realm, the role of social norms is a different one. Darley, Zimbardo, Milgram, and CNN have compellingly demonstrated that humans are not always humane. But the very cognitive ability to distinguish between "is" and "ought" shows that there is behavioral plasticity, and space for education, inoculation, and learning.

> *"What's optimism?" said Cacambo.*
> *"Alas!" said Candide," it is the mania of insisting that all is well when one is miserable."*
> —Voltaire (1759), *Candide ou l'Optimisme*

Krueger & Funder (K&F) have presented a fine article that strikes me with a holism paradox. Although I subscribe to most of the single points they make, such as their rebuttal of a hunt for cognitive biases gone wild in judgment and decision-making research (Kahneman et al. 1982; Tversky & Kahneman 1974), or their warning of mindless application of Null Hypothesis Significance Testing (NHST; cf. Glück & Vitouch 1998; Gigerenzer et al. 2004), I do not like the overall take-home message emerging from their article. How can this be?

"Cognitive norms" are not the same, neither genetically nor functionally, as social norms. Although some of the effects from the heuristics-and-biases program seem to be substantial, others result from highly specific instructions or setups, almost similar to trick questions. This "bag of tricks" has been rightfully criticized (e.g., Gigerenzer 1996b; 2001; Gigerenzer et al. 1999; Mellers et al. 2001). To what extent these effects really count as "norm violations," and which norms rationally apply, is a question of language ambiguities (e.g., "if" vs. "if and only if"), of conventions of formal logic, of different concepts of probability (from frequentist to subjectivist), and so forth. It therefore is a justifiable standpoint that many of those biases live more in the heads of researchers than in the heads of their subjects.

Social norms are of a different kind. Be they moral, legal, or religious, their function is to regulate the living-together of people in the modern world. It is clear that some of these standards (e.g., "Thou shalt not kill") are essential for guaranteeing peaceful life in a society, and they seem to be cultural universals. Research about violations of social norms is much more *ecologically valid* than research about violations of cognitive norms. The classic experiments of "negative" or "problem-seeking" social psychology were stimulated by actual events, not by dark perverted fantasies. Milgram's (1974) motivation, in his careful experimental work on obedience to authority, was to explain how the Holocaust was possible; Darley and Latané (1968; Latané & Darley 1970) wanted to understand how the "public murder" of the 28-year-old Queens woman Kitty Genovese in 1964 could happen, and how such "unresponsive bystander" phenomena can be avoided. Zimbardo, in his Stanford prison experiment (Zimbardo 1971; see http://www.prisonexp.org), wanted to understand how roles, uniforms, and authority, and more generally, powerful situations,

make people behave in ways that, in retrospect, seem strange even to themselves. All this has led not only to a better understanding of the spectrum of human behavior in social contexts, but has also enabled us to develop prevention and intervention strategies.

Together with the "biases" literature from social-cognitive psychology, K&F put all this into one common pot of allegedly negativistic social psychology. They are certainly correct that there has been a "bias towards biases" in the literature, and the list they present in Table 1 is truly outrageous. But experimenter-imposed zero-tolerance norms or shortcomings of NHST cannot be blamed for the 40% of participants giving the strongest shock (450 V, labeled "XXX") in the Milgram (1974) study, being in the same room together with the student/victim, and the additional 25% who gave intense shocks (> 240 V) in this condition before they refused to continue. And Zimbardo's (1971) results show that even when a majority behaves decently, a minority that does not (such as the guard "John Wayne") can easily get the upper hand.

We need not, however, resort to laboratory classics and textbooks of social psychology, but can look into CNN, quality newspapers, and history books. Just consider behavioral reports from the recent (civil) wars in former Yugoslavia and in several African countries. The "field" tells the story. And history repeats: There are evocative parallels between the Austrian-German war against Serbia, which triggered WWI, and the recent U.S. war against Iraq. The parallel starts with the formulation of an unacceptable ultimatum by the super-power (luckily with much less tragic consequences in the latter case). It ends with American patriots fiercely requesting that *French fries* be renamed *freedom fries* because of the anti-war policy of the French, just as the Viennese demolished the windows of shops and cafés with foreign names some 90 years before, in 1914 (Kraus 1922 [I, 1]); and with my e-mail box getting spammed with Saddam jokes and war remembrance ads.

Taking an evolutionary stance, it is clear that a specific social behavior (e.g., unconditioned obedience to authority) can be beneficial in one context, and maladaptive in another. It is also clear that hypothetical human social adaptations to "then" (the social environments of our ancestors) are not necessarily beneficial now. Evolutionary thinking should never lead us into normative biologism, or into Hume's (1740) *naturalistic fallacy:* "Is" and "ought" do not naturally relate. It may be well understandable *why* socially situated humans act in a certain way, and their behavior may even be an adaptation. But this does not mean that behavior is completely inflexible, and that the "is" dictates the norms.

I am skeptical about an evolution-inspired Panglossian paradigm (Gould & Lewontin 1979) for social psychology, in the sense of Dr. Pangloss's tragicomic stance, that "All's for the best in this best of all possible worlds" (Voltaire 1759). Although K&F advocate a balanced agenda, to some extent they fall prey to their own optimism. They sometimes seem to suggest that in-group/outgroup effects, stereotypes, and so forth only exist in the minds of researchers. Although a more balanced view of the positive and the negative, and a more integrated picture of "human nature," may prove to be helpful for the field, I cannot see how this implicit denial of real effects should be useful. Of course, glasses can be either half-full or half-empty; but a generalized "I'm OK – you're OK" attitude does not automatically promote social psychology.

So, is *Homo sapiens* a rational humanist?[1] Often, the easiest way to react (e.g., to obey) is neither the most rational nor the most socially desirable one. But I am an optimist, too: I believe in the power of education, insight, culture, and learning. I believe that informed human beings, who know what can happen, are better able to avoid bad outcomes. (That's why history is taught in schools.) People can *learn,* also from social psychology, to behave differently. For example, they can learn to disobey when obedience may have fatal consequences.[2]

It was a central point of the Enlightenment that not everything is for the best in our world, and that humans do not always act humanistically. It remains a legitimate task of social psychology to explain why.

NOTES
1. This is, at least, what philosophers believed for the longest time. Many other metaphors have been proposed, such as *Homo economicus,* the selfish and corrupt guy who you would not necessarily like to live next door to.
2. In order to endorse such (extraordinary) courageous behavior, the Austrian empress Maria Theresia (1717–1780) instituted a high military decoration for justified and victorious disobedience to an order.

Social cognitive neuroscience: The perspective shift in progress

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Abstract: Krueger & Funder (K&F) describe social cognitive research as being flawed by its emphasis on performance errors and biases. They argue that a perspective shift is necessary to give balance to the field. However, such a shift may already be occurring with the emergence of social cognitive neuroscience leading to new theories and research that focus on normal social cognition.

Krueger & Funder (K&F) present a reasoned argument that much of social cognitive research – particularly, decision-making, judgment, and reasoning – is flawed, as it focuses on errors that people make. They suggest, quite reasonably, that these errors and biases may reflect adaptive cognition that is appropriate to real-world situations and leads to errors only in the somewhat artificial laboratory environment. They express a desire for errors, biases, and normal behavior to be considered in the same theoretical frameworks. I agree that research and theories should address normal behavior and not just errors. Further, I believe that there is a growing body of social cognitive research that tests hypotheses about social cognition by studying the range of performance, rather than focusing on "abnormal states" – K&F review some of these studies in their article (e.g., Ekman 1991/1992; Stanovich & West 2000).

Social cognitive neuroscience is a synthesis of social psychology and cognitive neuroscience, and the emergence of this field has brought new integrative theoretical approaches. Although in its infancy, I would argue that this field meets the challenges of K&F. There are several theories of social cognition that address different aspects of normal social cognition (e.g., decision-making, social judgment, intuition, theory-of-mind, attitudes, stereotypes, emotional processing, reasoning) (Adolphs 2003; Cacioppo 2002; Cacioppo et al. 2000; Damasio 1996; Greene & Haidt 2002; Haidt 2001; Lieberman 2000; Ochsner & Lieberman 2001; Wood 2003). Recent social cognitive neuroscientific research has explored moral judgment and moral reasoning to establish how people make moral decisions (e.g., Greene & Haidt 2002; Greene et al. 2001; Haidt 2001; Moll et al. 2002a; 2002b). Damasio and his colleagues have explored social decision-making and demonstrated that people are able to make good decisions in the absence of awareness of experimental contingencies (e.g., Bechara et al. 1997; 2000). These theories and research meet K&F's criterion of considering error and accuracy in the same experiments and theoretical frameworks.

Even within more traditional reasoning research, it has been shown that people who fail classic reasoning tasks, such as Wason's selection task (Wason 1968), can perform accurately if the stimulus materials are familiar rules that are presented in a familiar real-world context (e.g., Cheng & Holyoak 1985; Griggs & Cox 1982; Johnson-Laird et al. 1972; Wason & Shapiro 1971). In addition, it has been argued that failures on traditional reasoning tasks result from the comparison of everyday reasoning strategies with "an inappropriate logical standard" (for a recent review, see Oaksford & Chater 2001).

In summary, K&F's take-home message is that social psychol-

ogy needs a shift in perspective to "a more balanced, full-range so-cial psychology." The purpose of their review is to stimulate such a shift. K&F eloquently argue that there is much research demon-strating a large number of different behavioral and cognitive bi-ases in social cognition. This is true; however, there is also a large body of research meeting their criterion, that is, the need to study a range of behavior and cognitive performance (some of these are presented above). In my opinion, therefore, a "perspective shift" is already in progress. Research and theories have been published, and are continuing to be published, that address normal social cognition and behavior without proposing that we reason or be-have in error-prone ways. That said, K&F's article provides a timely reminder that we should seek to understand behavior as a whole and not simply focus on the ostensibly abnormal or unusual.

Authors' Response

Social psychology: A field in search of a center

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Abstract: Many commentators agree with our view that the prob-lem-oriented approach to social psychology has not fulfilled its promise, and they suggest new research directions that may con-tribute to the maturation of the field. Others suggest that social psychology is not as focused on negative phenomena as we claim, or that a negative focus does indeed lay the most efficient path to-ward a general understanding of social cognition and behavior. In this response, we organize the comments thematically, discuss them in light of our original exposition, and reiterate that we seek not a disproportionately positive social psychology but a balanced field that addresses the range of human performance.

In the target article, we argued that modern social psychol-ogy is characterized by an abiding preoccupation with trou-blesome behavior and flawed cognition. We traced this state of affairs to an underlying value orientation that ac-cords primacy to negative phenomena and to the rigid way in which these phenomena tend to be cast in experimental design and statistical analysis. In conjunction, these prop-erties of social–psychological research have impeded the development of theories with explanatory power and the ability to generate novel and nontrivial hypotheses. Our suggestions for a re-orientation were not radical. Instead, we sought to highlight several existing trends in both theo-rizing and methodology that could benefit the field if pur-sued more vigorously. Many of the commentators echo our concerns about the history and the current status of the field; they constructively elaborate on many of the pro-posed remedies, and they suggest new ones. Others defend the traditional view, arguing that social psychology should continue to focus on misbehavior and flawed judgment. We are indebted to all commentators for their carefully rea-soned contributions. In this response, we highlight what we perceive to be recurring themes, and we delineate how the commentaries have shaped our thinking. As could be ex-pected, we give more detailed consideration to commen-

taries that challenge important components of our original argument.

The relevant themes can be organized to parallel the or-ganization of the target article. First, there is the question of *diagnosis.* Because we stressed the importance of study-ing the accuracy of social perception, it is only fair to ask whether our assessment of the state of the field is itself ac-curate. Second, there is the question of *methodology.* Our claim that the routine applications of null hypothesis sig-nificance testing contribute to the negative outlook turned out to be controversial; comments concerning moderator variables raised pertinent issues; and our proposal that re-search be oriented to examine the entire range of perfor-mance, rather than just the negative end, was in some cases misunderstood. Third, there are issues of *theory* and the kind of research most likely to help theory develop, which lie at the heart of the search for a balanced paradigm.

R1. An accurate diagnosis?

R1.1. Selectivity

There is no consensus among the commentators on whether social psychology is predominantly negative. Al-though many agree with our assessment that it is (**Hertwig & Wallin**, **Jussim**, **Kihlstrom**, **Ortmann & Ostatnicky**, **Schwarz**), others object (**Darley & Todorov**, **Gregg & Sedikides**, **Regan & Gilovich**, **Petty**, **Vitouch**). Still oth-ers feel that there is a negative orientation, but that this is as it should be (**Epley, Van Boven & Caruso [Epley et al.]**, **Friedrich**, **Klar & Levi**, **Shackelford & Vallacher**, **Stolarz-Fantino & Fantino**), or even, that this orientation is insufficiently negative (**Maratsos**). How then is one to ar-rive at a reasonably accurate negativity score? Database searches for relevant keywords such as *accuracy* or *bias*, of the kind provided by us or by **Kruger & Savitsky**, are only suggestive because they sample across diverse psychologi-cal subdisciplines and do not fully capture the impact of in-dividual publications.

Our case for the overall negative orientation of social psy-chology traced its roots to an enduring ideological commit-ment that began with the idea that social groups are more likely to corrupt individuals rather than allow them to flour-ish (e.g., Allport 1924; Le Bon 1895). Although some later work (especially in the Lewinian tradition) examined effec-tive leadership and heightened group performance, these topics faded from view as the cognitive revolution renewed interest in the psychology of stereotyping and prejudice. We also noted some of the rhetoric employed in the litera-ture, which has included the characterization of human judgment as "ludicrous," "indefensible," and "self-defeat-ing." **Regan & Gilovich** claim that in context these partic-ular terms were justified. Besides questioning whether de-scribing human behavior with a term like "ludicrous" is appropriate in *any* scientific context, we would note that these three terms were drawn from a longer list of exam-ples of negative rhetoric. To quote another prominent ex-ample, none of the commentators claimed that the com-ment "How could people be so wrong?" (Ross & Nisbett 1991, p. 139) was either justified or quoted out of context. It would be hard to deny – and we are not certain whether Regan & Gilovich intend to deny – that overall the rhetoric of the heuristics and biases literature has been both re-markably negative and effectively attention-getting.

We further noted that in this literature only negative and not positive effects become reified. The list of biases, errors, mistakes, illusions, and fallacies presented in Table 1 is a sobering illustration. If this sample were unduly biased, it ought to be possible to draw up an alternate list of positive effects. No commentator took the opportunity to do so, and we suspect that this is because it would be futile to try. With rationality (or accuracy) defined as a *point-hypothesis,* there are many ways to detect departures, but none to confirm the null hypothesis.

Finally, we argued that the heuristics-and-biases paradigm, as spearheaded by Kahneman and Tversky, was congenial to the pre-existing negative value orientation in social psychology, and its emergence coincided with the cognitive revolution. The adaptation of the heuristics-and-biases paradigm fueled the search for irrationalities, and the cognitive revolution introduced a variety of new places to look (e.g., automatic responses). The new hybrid paradigm achieved inordinate impact and public recognition as it swept over the field. As shown by the title of their review, Tetlock and Mellers (2002; cited by **Klar & Levi**) credited Kahneman and Tversky with stirring up "the great rationality debate." The work they reviewed was mainly concerned with exposing the limitations of Expected Utility Theory. In this, Kahneman and Tversky had great success, in part because they offered Prospect Theory as an alternative. The assumptions of Prospect Theory enjoy empirical support because they are grounded in well-established principles of the psychophysics of sensation and perception.

The derivative work within social psychology had no such general target, because social psychology lacks any overarching paradigm (such as Expected Utility Theory) that assumes behavior and cognition to be rational. As a result, Kahneman and Tversky's work only lent further ammunition to social-psychological research that was already premised on the pre-theoretical idea that people are socially and cognitively inept. Pinpoint hypotheses of rational thought popped up adventitiously only to be knocked down by the data. But these demonstrations of norm violations could not be regarded as anomalies calling for theory revision because there was no theory to be revised.

Still, many investigators, including some of the present commentators, endorse the idea that the study of anomalies yields the greatest theoretical benefits because errors open windows to the mind (**Epley et al.**, **Friedrich**, **Klar & Levi**, **Kruger & Savitsky**, **Shackelford & Vallacher**, but see **Gigerenzer**). This, indeed, is the key objection to our analysis. Our skepticism about this idea should now be clear. Experimentally demonstrated anomalies are informative only if there is a well-articulated theory mandating ethical behavior or rational thinking, and if the outcomes of experimental tests are not foregone conclusions. Neither condition is typically satisfied in social psychological work. Commonsense expectations of proper behavior and thought often pass for theory, and the sensors of data analysis are tuned to significant departures from narrowly defined norms.

It is astounding with what facility the idea that anomalies are most informative was imported to a field that had no general theory against which specific data could be judged to be anomalous. Although several commentators endorsed this idea in almost exactly the same words, no one presented a compelling case for *why* or *how* research on anomalies

yields deeper insights than other research. One would think that this debate offered a splendid opportunity to convince skeptics of the theoretical value of errors such as "The Big Three" (the false consensus effect, the fundamental attribution error, and self-enhancement). If these phenomena open windows to the mind, what do we see when we look through them? That this opportunity for clarification should have been foregone is perhaps the most troubling outcome of the present exchange. Until further arguments are presented, we are inclined to think that the interest in errors and biases is indeed the kind of infatuation that we diagnosed it to be. We cannot resist quoting from Lessing's *Nathan the Wise* on the matter (Lessing 1779/1923, p. 139).

> NATHAN (to his maid Daya):
>
> And yet though it might sound but natural,
> An every-day and ordinary thing [. . .]
> Would it be less of a miracle?
> To me the greatest miracle is this,
> That many a veritable miracle
> By use and wont grows stale and commonplace.
> But for this universal miracle,
> A thinking man had ne'er confined the name
> To those reputed miracles alone
> Which startle children, ay, and older fools,
> Ever agape for what is strange and new,
> And out of nature's course.
>
> DAYA:
>
> Have you a mind
> With subtle instances like this to daze
> Her poor o'erheated brain?

Fiedler claims that "for a scientific contribution to be accepted as original, it has to deviate from established laws," and points to the work of Copernicus, Einstein, and Kahneman as prominent exemplars. According to this argument, demonstrations of errors are both original and theoretically progressive. We believe that they are neither. By definition, deviations from prior beliefs may qualify as original when first observed, but their originality should wear off with repetition (and Bayesian belief revision). This did not happen in social psychology, apparently because of an enduring commitment to the pre-theoretical idea that only the hypothesis of rationality needs refutation. Moreover, Kahneman and Tversky's work was original and impactful in relation to Expected Utility Theory, but the derivative work on social heuristics and biases was accepted precisely because it was *not* original. Instead, it was seen as a great fit with a great idea (a pre-theoretical, poorly justified one at that). The field slumped into a Trotskyite dystopia, in which the conduct of normal science (Kuhn 1970) was mistaken for a permanent revolution.

R1.2. Omissions

We did point to a few exceptions to the predominant value orientation in social psychology, and we thank the commentators who discussed research directions that we had neglected, such as dual-process theories (**Petty**, **Slater**, **Stanovich**), social neuropsychology (**Wood**), social learning (**Stolarz-Fantino & Fantino**), and strategic interaction (**Hodges**, **Kameda & Hastie**, **Kenrick & Maner**, **Kihlstrom**).

Two specific omissions deserve further comment. The first and more general omission concerns cooperative behavior. From the perspective of orthodox game theory,

cooperative behavior in non-zero-sum games such as the prisoner's dilemma appears anomalous and irrational. If rationality is defined in terms of self-interest, rational players are expected to defect, which leads to poor outcomes for everyone. Yet, many players cooperate (Komorita & Parks 1995; Sally 1995). **Darley & Todorov** imply that this finding can be counted as a positive contribution of social-psychological research, but we wonder how. Experimental games were first devised and studied by mathematicians and economists (e.g., Luce & Raiffa 1957). Subsequently, investigators in a variety of social-science disciplines observed levels of cooperation that were unexpectedly high from the game-theoretic perspective. The question is whether social psychology has succeeded where game theory failed. We do not think that it has (yet). It is true that some factors have been identified that increase levels of cooperation. Some of these require additional information about the other player. For example, people are more likely to cooperate with an attractive opponent (Mulford et al. 1998) or with someone belonging to a social ingroup (Kiyonari et al. 2000). The former effect seems irrational when observed in the anonymity of a one-shot game. The latter effect can be attributed to generalized expectations of reciprocity. Other effects fall near the boundary of social and personality psychology, such as the finding that people with a prosocial attitude (or disposition) are more likely to cooperate (van Lange & Semin-Goossens 1998). This effect too may be understood as a consequence of expectations of reciprocity (Acevedo & Krueger 2004; Krueger & Acevedo, in press). We think that generalized expectations of reciprocity will be a significant element in any social psychological account of cooperation in dilemmas (Baker & Rachlin 2001; Krueger 2003b; Krueger & Acevedo 2002). Perhaps ironically, this approach owes much of its potential to previous research on social projection, and thus the "false-consensus effect" (Acevedo & Krueger 2004; Krueger & Acevedo, in press; Orbell & Dawes 1991).

Dunning claims that another significant omission is our failure to consider further evidence relevant to Kruger and Dunning's (1999) study of the "unskilled and unaware" syndrome. We accorded this study a prominent place in the target article because it exemplifies many of the troublesome features of the heuristics-and-biases tradition. The focus of the research was unabashedly negative. Participants who performed poorly on a test (any test!) were charged with the dual failure of being unskilled and unaware of it, and the findings were heavily publicized as revealing deep failures of social cognition (see also Dunning et al. 2003). To elaborate on our foregoing discussion, we note four points.

First, regression to the mean guarantees that errors by low scorers will tend be overestimates, whereas errors by high scorers will tend to be underestimates (Krueger & Mueller 2002), but regression works in the other direction as well (Campbell & Kenny 1999). When actual test scores are regressed on the estimates, high estimates are associated with overestimation, whereas low estimates are associated with underestimation. The unskilled-and-unaware pattern is thus equivalent to over- and under-confidence. The latter pattern is well known, raising the question of whether the unskilled-and-unaware pattern is a new discovery.

Second, Kruger and Dunning did not demonstrate mediator effects of individual differences in metacognition. The mediator hypothesis assumes that individual differences in metacognitive skill (i.e., insight into one's per-

formance) are correlated with test scores. The negative correlation between test scores and estimation errors should then be attenuated when differences in metacognitive skill are controlled. A study testing this hypothesis was unsuccessful (Krueger & Mueller 2002). This may not be surprising because the presumed metacognitive skill involves the ability to discriminate individual test items on which one did well, from those on which one did poorly. Such a skill is a matter of sensitivity, whereas the overall estimate of one's own performance is a matter of bias (or threshold of optimism). These two facets of judgment are conceptually distinct (Swets et al. 2000), and there is no particular empirical reason to believe them to be related (**Lambert, Payne & Jacoby** [**Lambert et al.**]).

Third, **Dunning** asserts that we ignored Studies 3 and 4 in the Kruger and Dunning (1999) article, which he claims established the validity of the mediator hypothesis. Study 3 showed that low scorers do poorly when evaluating the test answers of others. This was construed as an indication of low metacognitive skill, but it is hardly a parsimonious account. The finding may reflect the low scorers' limited ability to solve the test problems. If it cannot be shown that metacognition involves skills that are conceptually distinct from those needed to do well on the test, there is no need to invoke them as variables mediating the correlation between performance and estimation errors. Study 4 showed that low scorers' overestimation errors disappeared when these participants were trained to do well. Kruger and Dunning seemed to realize that they could not train participants to acquire better metacognitive skills. Instead, they manipulated the original predictor variable (i.e., actual performance), which reconfounds test-taking and metacognitive skill even while it restricts the range of the outcome variable.

Fourth, one might ask precisely what normative standard is violated by the "unskilled and unaware" effect. One possibility is that people should predict their own performance perfectly. A standard of perfection is unrealistic, however, if only because both estimated and actual performance scores are inevitably affected by random error components. Interestingly, if the correlation between the two were close to perfect, there would be no need to score psychometric tests. We could simply ask people how they think they did. In an alternative ideal universe, individual differences in test performance could be diminished (as was attempted in their Study 4, when low scorers were trained to score high). In the limiting case, measurement would again become superfluous. More realistically, statistical regression would still assert itself. As Galton put it, "the deviations from the average – upwards towards genius, and downwards towards stupidity – must follow the law that governs deviations from all true averages" (Galton 1892, p. 28).

R1.3. Interpretation

Some commentators, especially **Darley & Todorov** and **Regan & Gilovich**, suggest that we portrayed major social psychological work as more negative than it really is. Particularly with regard to the classic behavioral research, these commentators emphasize how the findings can be construed as showing how normal people try to cope with difficult situations. This perspective is indeed more compassionate than many secondary treatments. Thus, we only partially disagree with these commentators. As we noted in the article, the original researchers went to great lengths to

map out the conditions under which social pressures sway people. Nevertheless, the nature of these pressures was typically directed toward non-normative, or even offensive, behavior. Countervailing situational forces (e.g., the liberating effects of accomplices in the Asch paradigm) played the role of reducing the prevalence of negative behavior. In a related vein, **Klar & Levi** maintain that error research implies a compassionate stance because "it is impossible to recognize how remarkable an achievement occasional accuracy is, without first appreciating to what extent human judgment is prone to error." We disagree with this assessment. Accurate judgment is difficult because the context is difficult; the necessary information is often missing, ambiguous, misleading, complex, and confusing. Rather than being prone to error, human judgment is adapted for accuracy under difficult conditions. When information is missing, false, or ambiguous, the accuracy of social judgments suffers without implying the operation of faulty mental processes.

Darley & Todorov claim that work on bystander nonintervention was not meant to imply normative or ethical failure. As we see it, the social-psychological experiments were designed to see if *simple* social situational variables could produce the same kind of nonbehavior seen among the bystanders in the murder of Kitty Genovese. The inaction of these bystanders was a brute fact, and the question was whether it could be attributed to some particular characteristic of them, such as the exceptional callousness of New Yorkers. The studies by Darley and Latané (1968) suggested that this was not so because under certain conditions, prosocial acts could be inhibited among normal, randomly selected people, not from New York. This made the implications more universal and therefore even more jarring. Now, Darley & Todorov suggest that the passive bystanders in the experimental emergency paradigm "had not decided *not* to respond" and remained "in a state of indecision and conflict concerning whether to respond or not." This is perhaps a more sympathetic construal than sheer callousness, but not by a wide margin.

With regard to the Good-Samaritan study (Darley & Batson 1973), **Darley & Todorov** suggest that an unspecified proportion of participants were "in conflict between stopping to help the victim and continuing on [their] way to help the experimenter." From this perspective, all of the participants' behavior was both altruistic and unresponsive, depending on whether the victim or the experimenter was seen as the beneficiary. The "help-as-a-zero-sum-game" hypothesis is only credible when the predicaments of the two parties in need are equivalent. This was hardly the case in the Good-Samaritan study, and the predicaments were even more strikingly different in the Epileptic-Seizure study (Darley & Latané 1968). Moreover, the hypothesis would vitiate the relevance of the bystander studies to the Kitty Genovese murder. In that gruesome situation, who would benefit from a person not calling the police? Darley & Todorov seem to recognize that the two targets of help are out of balance when they suggest that "people can train themselves to resist these forces." Such a decision to improve one's behavior presupposes a valid judgment of who needs help more urgently.

Fiedler raises a question concerning the interpretation of social-cognitive research that parallels **Darley & Todorov**'s argument regarding the classic behavioral work. He suggests that our argument boils down to a one-sided, negativistic interpretation of the findings when, instead, many presumably negative effects can easily be recast as positive. It seems to us, however, that when biases such as overconfidence or false consensus take on a more positive look, it is not a matter of a changed interpretation, but a change in the normative model or the context of the task (see **Gigerenzer** for examples, or Fiedler's [2000] ecological approach to social perception).

Another interpretive issue arose in the wake of the "Merlot metaphor." When it dawned on us that in the situationist paradigm it is paradoxical to see the fundamental attribution error (FAE) as a disposition of research participants, we expected vigorous objections. There were none. Indeed, it is remarkable that of 35 commentaries, *not one* defended the common interpretation of this putatively "fundamental" phenomenon. We suggest this is because the FAE falls apart under close scrutiny, and continues to be famous only because it seldom receives any.

Instead of defending the FAE, some commentators (**Fiedler**, **Kruger & Savitsky**) try to turn the paradox around. After saying that we "point out, correctly, that FAE researchers themselves commit the FAE," Fiedler suggests that *we* "commit the FAE by blaming researchers rather than the scientific situation." What we argued was this: If one considers statistically significant effects to be products of the experimental design, one should credit the experimental situation as the FAE's cause. The attributional zero-sum logic then implies that FAE-making dispositions cannot be the cause. Ergo, attributing the FAE to such a disposition is an instance of the same. Alternatively, one could relinquish the claim that statistically significant effects stem from the power of experimental situations. In the case of the FAE, this would mean that a situation that was designed to eliminate dispositional inferences failed to do so. This too is an inconvenient conclusion for a situationist paradigm. In the Jones-and-Harris design this imbalance means, distressingly, that the researchers had greater success changing behavior (i.e., getting compliance for the writing of counterattitudinal essays) than changing judgments (i.e., from the dispositional to the situational). Our point is that one cannot have it both ways: professing the power of situations, and blaming the dispositions of research participants when that power falls flat.

Fiedler's suggestion that our pointing up this paradox is itself an example of the FAE can mean one of two things. It might mean that all dispositional inferences are paradoxical and thus invalid. We see no support for this extreme view, either in attribution theory or elsewhere. Or it means that the FAE paradox can only be stated at the cost of superimposing a second layer of the same paradox. This view would then, of course, also apply to Fiedler's own suggestion that we too have a disposition to err on this matter. The prospect of an infinite regress here is not an appealing one.

Perhaps, as in the analysis of behavior more generally, it is not fruitful to try to separate dispositional from situational factors when making sense of the conclusions drawn by individuals or groups of investigators. Any particular study simultaneously reflects the beliefs and preferences of individual researchers and the ideology prevalent in the field. We may then agree with **Fiedler** that the problem lies, in part, "in ideological constraints imposed on research" (also **Haslam, Postmes & Jetten** [**Haslam et al.**]). Ideologies are represented in individual minds, often with a great deal of consensus. They are both personal and cultural. Ross's

(1977) statistical criterion of detecting dispositions notwithstanding, to define the personal only as that which is unique (e.g., Karniol 2003), is too limiting (Krueger 2003a). By Ross's definition, claims about the FAE could be attributed to individual researchers (or research participants) only if these researchers (or participants) held a minority view. If that were the case, the FAE would hardly be regarded as "fundamental."

R2. An "aye" for methods

A central theme of our target article is that the conventional methodology of social psychology can produce misleading results and has helped to create a literature that exaggerates misbehavior and flawed judgment. We offered suggestions for increased methodological sensibility, and several commentators took these ideas further. **Borkenau & Mauer** and **Gosling** elaborate on the need to study individual differences in social judgment, and to take advantage of the Brunswikian framework (**Hammond**). Other commentators propose improved experimental designs (**Ortmann & Ostatnicky**) or model-fitting techniques (**Jussim**). In addition, we would urge greater awareness of the role of random error in judgment and the concomitant regression artifacts (**Gigerenzer**). We are encouraged by the increasing popularity of methods that decompose judgment into theoretically meaningful components (**Lambert et al.**; Swets et al. 2000). The specific appeal of these methods is that they guard against the temptation to treat any significant sign of bias as an indication of inaccuracy (see also Hastie & Rasinski 1987; Wright & Drinkwater 1997). For a recent compendium of further methodological advances (e.g., simulations, connectionist modeling, meta-analysis), we recommend the handbook published by Reis and Judd (2000).

Three specific methodological issues arise from our article and the commentaries. The first concerns the pitfalls of null hypothesis significance testing (NHST) and our suggestions for the refinement of statistical inference. The second issue concerns the role of moderator variables in research on misbehavior and flawed judgment. The third issue is our suggestion that research be reoriented to study the whole range of human performance, not just the negative end.

R2.1. The NHST bugaboo

Our article pointed out several of the increasingly often-recognized pitfalls of null hypothesis significance testing, leading some commentators to respond that NHST "has not caused the current crisis" (**Fiedler**, see also **Chow**, **Goodie**, **Gregg & Sedikides**). But we made no such claim. Instead, we pointed to the consequences of NHST in the context of narrow, unforgiving, point-specific norms of good behavior or rationality. The target article was not the place for a full or decontextualized discussion of NHST. Chow presents some general arguments, and for a full review and discussion of his ideas, we recommend an earlier exchange in this journal (Chow 1998). Gregg & Sedikides downplay the significance of NHST by arguing that the acid test for inductive inferences is the replicability of the findings. We agree, but not totally. As shown elsewhere, the p value obtained from significance testing is a valid cue towards replicability. Its validity increases inasmuch a replication study closely resembles the original work (Greenwald et al. 1996; Krueger 2001). NHST proscribes the use of prior probabilities of the hypotheses, whereas Bayesian analyses make explicit use of them. Thus, we disagree with Fiedler, who suggests that the Bayesian perspective offers no qualitative improvement because different statistical indices (p values) can be converted into each other.

Our main recommendation for reform was to ask researchers to begin to think within a Bayesian framework (see also Rorer 1991). In doing so, we attempted to dispel the view that Bayesian thinking and NHST are incompatible. Often, the two frameworks are viewed as a choice between objective and subjective data analysis. When framed this way, who would not prefer the objective approach? We strove to show that Bayesian calculations of inverse probabilities (i.e., the probabilities of certain hypotheses given the empirical data) can be grafted on the better-known procedures employed within NHST. This idea has been expressed by several writers within social psychology (Krueger 2001; Trafimow 2003) and elsewhere (Cohen 1994; Hagen 1997). Most notably, the American Psychological Association's task force on statistical inference came to endorse this view (belatedly) in its rejoinder to commentaries on its original set of recommendations (Task Force on Statistical Inference 2000).

R2.2. Salvation in moderation?

The search for moderator variables has a long tradition in social psychology (Greenwald et al. 1986). As we noted, the classic work on conformity, obedience, and bystander behavior was designed to identify important situational constraints on the basic effects. **Kruger & Savitsky** extend this approach to the study of cognitive-perceptual biases, suggesting that "so-called contradictory biases typically lead to the investigation of moderating variables." This may be so in some cases, but we suggest that much more common is their safe mutual isolation in independent literatures employing different jargon. For example, we have yet to see research reconciling the "hot hand" with the "gambler's fallacy," or the overuse of stereotypes with the underuse of base rates, and in general the errors in Table 1 are treated as general and not moderated phenomena. Even when moderation is pursued, the results may be less than wholly illuminating.

For example, numerous studies of the false consensus effect (FCE) have explored ways the effect could be made larger or smaller, yet failed to produce a coherent view of how social projection comes to operate in the first place. Instead, the FCE became overdetermined as more and more causal factors were "ruled in" rather than ruled out (Krueger 1998b). The impasse was broken not by the discovery of further moderator variables, but by the introduction of a new normative model with a fresh view of how projection can produce predictive accuracy (Dawes 1989; Hoch 1987). Significantly, no reliable moderator variable was identified that would predict the occurrence of the opposite of the FCE (i.e., the "false uniqueness effect," Krueger 2000b). A similar critique applies to research on the fundamental attribution error (FAE). The more factors are identified that increase people's propensity to make dispositional attributions, the less we know about how and when these attribu-

tions might be correct, or what dispositional inferences are good for.

Self-enhancement has also seen some moderator research. When conceptualized as a motive, for example, situational variables can be found that trigger or inhibit it (Shepperd et al. 1996). Kruger (1999) identified task difficulty as a moderator variable. His analysis showed that people self-enhance when tasks are easy, but that they self-diminish when tasks are difficult. This finding suggests that self-enhancement is not an immutable, built-in bias, but can be made to disappear or even reverse depending on the selection of tasks.

The task difficulty effect on self-enhancement can be examined further from a regression perspective. With regard to the "unskilled-and-unaware" hypothesis (Kruger & Dunning 1999), we began with the empirically based expectation that actual performance (A) is positively correlated with estimated own performance (E). It follows that A is negatively correlated with estimation errors (E-A). This analysis holds regardless of whether A refers to individual differences in ability or to differences in task difficulty. As tasks get easier, the probability of success and the probability of underestimation errors increase. At the same time, people self-enhance *more.* How can that be? Kruger (1999) suggested that, taking the subjective experience of task strain as a cue, people infer low and high performance, respectively, from difficult and easy tasks. A second reasonable assumption is that people will project their own experience on the average other person (O) (Kelley & Jacoby 1996)**.** The correlation between these O ratings and actual task difficulty (A), because it is mediated by E, will then be lower than the correlation between E and A. It follows that self-enhancement, when construed as the "better-than-average" effect, or E-O, will be positively correlated with the probability of success (A) (see Asendorpf & Ostendorf 1998 for derivations). In short, opposite patterns can be obtained depending on whether self-enhancement is construed as an overestimation of reality or as a favorable social comparison. What is more, neither pattern reflects a genuine moderator effect, which requires two predictor variables, whose cross products contribute to the prediction of the outcome (Aiken & West 1991).

The question of moderator variables has larger, paradigmatic importance in social psychology. Over the last decade, the study of culture and cultural differences has received increased attention. This is an important and potentially fruitful development. In the context of the present discussion, however, we note that a good deal of research continues to rely on ostensibly established phenomena from the heuristics-and-biases paradigm. Culture is thereby conceived as a moderator variable that tells us whether certain errors or biases are more or less prevalent in one culture or another. Nisbett and his colleagues, for example, found that the effect sizes of many of the standard phenomena rise or fall depending on whether studies are conducted with participants from individualist or collectivist cultures (Nisbett 2003; Nisbett et al. 2001). East Asians show a stronger hindsight bias but a smaller fundamental attribution error than do Americans. Although these findings are intriguing, their interpretation will likely spawn some controversy. Nisbett and colleagues take a relativistic stand with regard to cognitive norms, noting that most are of Western, and specifically Aristotelian, origin. Yet, these norms are used to evaluate the performance of East Asians,

whose Taoist or Confucian framework has little use for such concepts of logic or rationality. Thus, it remains to be seen whether cross-cultural work can complement the heuristics-and-biases paradigm as hoped.

R2.3. Taking the Bad with the Good

A few commentators read our paper as prescribing research that "congratulat[es] whatever positivity is out there already" (**Dunning**; also see **Epley et al.**, **Regan & Gilovich**) and even as seeking to establish that "everything's super" (**Kruger & Savitsky**). This is a fundamental misreading of our intention, and while we appreciate that other commentators did recognize that we do not advocate "an imbalanced focus on the 'sunny side' of social behavior" (**Figueredo**, **Landau & Sefcek** [**Figueredo et al.**]), some further clarification might be in order. To reiterate, we argued in section 4.1.1 that

> a one-sided research emphasis on positive behavior, perhaps complete with null hypotheses where *bad* behavior represents the null to be disconfirmed, might eventually generate problems parallel to those besetting the one-sided emphasis on negative behavior. We recommend that the *range* of behavior be studied, rather than showing that behavior is bad – or good – more often than people would expect.

In this vein, we agree with **Gregg & Sedikides** that heuristics and biases do sometimes lead to harmful outcomes, but problems arise when these biases are assumed to cause harm in *all* contexts and when correcting them across the board would cause more harm than good. By the same token, some heuristics might be mental *appendixes*, that like the gastrointestinal kind cause occasional harm and no good, but we will move closer to identifying which these are only when we stop assuming (or acting as if) they *all* are. Fine-tuning the analysis of the implications of heuristics is exactly what we would support.

We believe that the suggestion that our target article fails to describe "in more detail and precision what such a [balanced] psychology would look like, even by example" (**Dunning**) seems to overlook section 4, which addresses this topic and occupies almost a third of the article's length. Specific and detailed examples are included in sections 4.1.2 and 4.3.3.1. The central intention of the Realistic Accuracy Model (section 4.3.3.2) is to "point to four specific stages where efforts to improve accuracy might productively be directed." Efforts to improve accuracy, of course, presume that not everything is "super."

A related misunderstanding is evidenced by **Kruger & Savitsky**, who identify what they see as an inconsistency in the prior work of one of us. They note that "whereas Taylor and Brown [1988] emphasized the positive implications of judgmental errors, Funder and colleagues emphasized the negative implications." The Taylor and Brown thesis is based on research showing that people who have positive views of themselves and their prospects generally have good outcomes. Funder's misgivings stem from the characterization of this effect as being due to illusions, when it seems reasonable to expect that well-functioning people will develop positive and *accurate* self-views. The possibility that "adaptive illusions" might not be illusions at all is supported by findings that overly positive "narcissists" have negative outcomes (e.g., Paulhus 1998; Robins & Beer 2001). In a broader perspective, our point of view does not

seek to defend inaccuracy; it does in many cases lead us to doubt claims that inaccuracy (a.k.a., "errors," "illusions") has been found.

We take extra space to reiterate this point because we suspect this is precisely where misunderstanding of our intention is most likely to occur. Our critique of the negativity of behavioral and cognitive social psychology is not meant to lead to a new and positive social psychology, but rather to a *balanced* field of research where both across and within studies the entire range of performance would receive attention. This is why, for example, we endorse **Gosling**'s call for more research in real-world contexts – not necessarily because accuracy is more likely in such contexts, but because, unlike in some experimental situations, accuracy is at least *possible.* We believe it is axiomatic – if still not universally appreciated – that demonstrations of the circumstances that promote error will be most informative when they can be contrasted with the circumstances that promote accuracy, and this cannot occur unless both are studied.

R3. Theory

Unlike some other social sciences, social psychology does not have a master theory. Instead, there are numerous theories of small or intermediate range (**Brase**, **Hammond**). There are unifying themes, but these are rather pre-theoretical. We think that there are two reasons why these pre-theoretical commitments have been so influential. One is Asch's observation that "there is an inescapable moral dimension to human existence" (cited by **Hodges**). The other is that a great part of the phenomena relevant to the field are also the kinds of phenomena that people can observe in their own lives and form opinions about. The confluence of the moral dimension and common sense provided an epistemological framework that has given the field a moralistic aspect. At the risk of exaggerating this point, one might say that the move from the classic behavioral work to social-cognitive work on heuristics and biases was a move from the "crisis of conscience" metaphor to a psychology of "dumb and dumber" (**Kihlstrom**).

The present exchange points to a variety of theoretical developments that can go a long way to detach social psychology from its pre-theoretical premises, and thus to restore balance. The Realistic Accuracy Model (RAM) was offered as one way to incorporate both error and accuracy within a single framework, and to extend the analysis of social judgment beyond the "utilization" (cognitive) stage to include interpersonal and contextual influences. **Kameda & Hastie** are correct to observe that RAM remains a basic schematic for research and theorizing, rather than a complete account. RAM claims that accurate judgment can occur only if relevant information is available to a judge, who then detects and correctly utilizes that information. Although this claim adds three steps to the usual analysis of accuracy, and has generated new research, the bare-bones Figure 2 of the target article does not explicitly address how multiple sources of information arrive in an interacting stream, while their meaning changes according to context. But to add these elements would require simply a straightforward extension of the basic model (and a much messier Fig. 2). A more significant omission is any representation of the judge's goals. The purpose of RAM is to explain the necessary steps towards accurate judgment, where accuracy is defined as a veridical correspondence between the distal

properties of an object and the understanding of that object achieved by an observer. Whether accuracy itself is a worthy goal, for example, according to a cost-benefit analysis, is a separate (and interesting) issue that at present lies outside of the model.

In forecasting future theoretical developments, a conservative estimate is that some progress will be made by importing theoretical advances from neighboring fields. One example is the increasing popularity and generativity of evolutionary theory. The relevance of this theory was first pursued in the area of mate attraction and selection. As **Kenrick & Maner** show, its influence is now spreading. Similar developments are under way in the area where social psychology overlaps with the psychology of judgment and decision making (**Gigerenzer**, **Hertwig & Wallin**, **Ortmann & Ostatnicky**) and with behavioral economics (Colman 2003; Hertwig & Ortmann 2001). The latter development appears to be particularly promising in focusing attention on the dynamics of strategic interpersonal behavior, as noted by **Kameda & Hastie**, **Hodges**, and **Kihlstrom.**

We do not expect any of the various new approaches to take over the field, but rather, to offer considerable integrative momentum. Perhaps, the coexistence of these approaches will put too much strain on the field, leading researchers and students to feel they must commit themselves to one metatheory or another. We do not think, however, that this is a grave risk. At least one popular introductory textbook was written by three researchers representing strikingly different theoretical orientations (Kenrick et al. 2005). These authors have set an example of how the field can be jointly illuminated by the evolutionary, cognitive, and cultural-behavioral approaches.

In conclusion, we wish to reaffirm our hope that social psychology will benefit from reform. As we hope the target article made clear, we expect that certain epistemological core values can be retained, such as a realist approach to the subject matter, a vigilant dedication to the principles of theoretical coherence and parsimony, and a continued effort to demystify social realities.

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Letters "a" and "r" appearing before authors' initials refer to target article and response respectively.

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