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.

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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

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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 Kreuger & 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 social 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