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-

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