present psychological knowledge as contingent given the evolving and sometimes ideologically laden nature of the subject matter.

(d) Psychology has been an impressively successful discipline in North America, Europe, and many other parts of the world. The process of *psychologization*, the fact that more and more areas of human existence—from education, industry, the military, the economy, legal and health systems, to society and politics—are understood in terms of psychological categories and theories can be identified as our success. But with regard to the psychologization of public life—from discussions on national intelligence at the beginning of the 20th century, to the psychological contributions in *Brown v. Board of Education* in the middle of the 20th century, to the notion of empathy deficits that President Obama evokes—the public also has a right to know about critical assessments of psychology from within the discipline (Slife, Reber, & Richardson, 2005; Teo, 2005).

The psychologization of everyday life also demonstrates that the categories of psychology are not the same as those in the natural sciences (Danziger, 1997). One important feature that makes psychological concepts different is the evaluative looping effects (Hacking, 1994) that psychological categories evoke. Psychology is applied in the public sphere through concepts or ideas developed in academia (e.g., IQ, emotional intelligence, extraversion, pathological grief, attribution style, the notion that brains are not wired correctly). But the application of these terms in the public sphere changes the public in that a different type of reflection occurs when the public uses these concepts in their self-understanding and in the formation of their identities—and when psychologists rediscover these entities in their research. There exist ongoing looping effects between academic psychology and the public, and psychologists need to study and understand this dialectical interconnection.

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Further Sources of Our Field's Embattled Public Reputation

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In my article on public skepticism toward psychology (Lilienfeld, February–March 2012), I delineated eight reasons why many laypersons are dubious of our field's scientific status. I argued that although some of these sources (e.g., hindsight bias, the illusion of understanding) reflect public misunderstandings regarding the application of science to psychological questions, others (e.g., our field's reluctant embrace of evidence-based clinical practices) reflect professional psychology's failure to uphold rigorous scientific standards. I was gratified to read these three stimulating commentaries, if only because they suggest that my article accomplished its principal aim: to engender thoughtful debate concerning the sources of, and remedies for, psychology's problematic scientific status in the public eye. The authors all noted significant points of consensus with my analysis but also identified areas in which they found my coverage to be incomplete or inaccurate. I welcome these constructive criticisms and address each in turn.

I thank Newman, Bakina, and Tang (2012, this issue) for directing me to a source of public skepticism toward psychology that I had neglected: the fundamental attribution error (FAE). They conjectured that because of the FAE, many laypersons are suspicious of standard social psychological explanations of behavior in the media, which emphasize situational influences. These attributions, Newman et al. contended, are readily perceived by the public as excusing unethical actions. Newman et al. drew on data (e.g., Newman & Bakina, 2009) demonstrating that after undergraduate participants read descriptions of studies that highlight situational rather than dispositional influences on immoral behavior (e.g., cheating, domestic abuse), they view the *researchers* who penned the descriptions as attempting to absolve individuals of responsibility for their actions. If these results generalize to real-world settings, they could imply that many laypersons perceive psychologists, especially social psychologists, as "bleeding hearts" (Newman & Bakina, 2009, p. 269) who are reluctant to hold bad actors to account for their misdeeds. Newman et al. (2012) additionally reported evidence that respondents are less likely to attribute what we might term "exculpatory intent" to investigators when the written accounts of the study accord roughly equal weight to situational and dispositional factors.

I find Newman et al.'s (2012) hypothesis to be worthwhile and intriguing, and I offer two friendly amendments to their friendly commentary. First, I wonder whether participants in these studies were in essence behaving as "implicit Bayesians" when drawing inferences regarding researchers' motives and views. In everyday life, individuals who frequently invoke situational influences when explaining others' unethical behavior (e.g., "The soldier followed the general's orders because he felt he had no choice") may indeed be more likely than other individuals to be attempting to condone it. If so, these participants may be at least partly justified in their conclusions regarding the researchers' intentions. Second, although the FAE is undeniably an erroneous conclusion in certain instances, it probably reflects the overgeneralization of a broadly accurate propensity to perceive traitlike consistencies in individuals across situations (Funder, 1987). Indeed, at times social psychologists have gone too far in neglecting dispositional influences on behavior, as witnessed by efforts to minimize the role of individual differences in accounting for the deplorable behavior of approximately 10 U.S. soldiers during the 2004 Abu Ghraib prison scandal (Donnellan, Fraley, & Krueger, 2007). A meta-analysis of over 25,000 studies comprising more than 8 million participants revealed that the mean effect size of dispositional influences on behavior (r = .19) is

comparable to that of situational influences (r = 22; Richard, Bond, & Stokes-Zoota, 2003), although the sizes of such correlations surely depend on the variances of dispositional and situational variables in each sample. To the extent that social psychologists who communicate with the media at times underplay the potency of dispositional influences on behavior and portray situations as generating a virtually ineluctable drive toward antisocial actions, public skepticism of such proclamations may be warranted.

Tryon (2012, this issue) and Teo (2012, this issue) raised somewhat complementary criticisms, as both maintained that I let psychology off the hook too easily. Specifically, they argued that psychology at large has failed in its mission to construct genuine causal explanations of behavior. For example, Tryon observed that some widely espoused theoretical frameworks within psychology, such as the "biopsychosocial model," consist of scant more than recipes of generic ingredients (e.g., biological, psychological, and social influences) for explaining behavior. Such largely vacuous models may nonetheless display a patina of profundity that lends them credibility in the eyes of many academicians. I concur wholeheartedly with Tryon in this regard and similarly consider these models to be virtually unfalsifiable truisms devoid of much explanatory power. At the same time, I am less persuaded than he is that this problem contributes substantially to the general public's skepticism of psychology, although it may foster such skepticism indirectly by impeding our field's already glacial progress.

Tryon (2012) proposed that connectionist models have the potential to integrate psychology with neuroscience and therefore place our field on firmer scientific footing. I agree, and would add that these models may help to dissolve some of the largely arbitrary distinctions among historically distinct "schools" of psychology (e.g., behavioral, cognitive). My lone quibble with Tryon is his apparent wholesale dismissal of "reductionism." As I observed in my article (Lilienfeld, 2012), we should be careful not to tar all forms of reductionism with the same broad brush. As scientific psychologists, we should embrace constitutive reductionism, which rejects *substance dualism*, or the proposition that mind and body are composed of fundamentally different "stuff." Yet there is ample reason to resist the bandwagon toward eliminative ("greedy") reductionism, which prematurely rejects the possibility of *property dualism*, or the proposition that mind and body are different levels of analysis of the same phenomena. Only the latter form of reductionism, I contend, poses a threat to scientific psychology and risks perpetuating inaccurate beliefs that the psychological level of analysis has little to offer above and beyond the physiological level.

Teo's (2012) commentary is the least sanguine of the three. Teo contended that psychology purports to offer explanations of behavior when in fact it typically provides only interpretations. In his eyes, psychology is problematic as a science and hence merits some of the public opprobrium heaped upon it. I suspect, however, that Teo's distinction between explanation and interpretation is less clear-cut than he implied. I share Teo's view that most psychological theorizing does not provide full-fledged explanations for complex phenomena, such as obedience, violence, or prejudice. Nevertheless, psychology does often offer "explanation sketches": provisional causal accounts that consist of "a more or less vague indication of the laws and initial conditions considered as relevant" and that require "'filling out' in order to turn [them] into . . . full fledged explanation[s]" (Hempel, 1959, p. 351). In an increasing number of domains (e.g., the etiology of schizophrenia), psychology has homed in on many of the key causal variables and on rough outlines of their directional pathways, although the precise steps linking these variables into an integrated etiological explanation remain to be clarified.

Following in the footsteps of Kenneth Gergen and others, Teo (2012) contended that because psychology is a human science as least as much as a natural science, it should not be held to the high standards of the latter. For example, he pointed to the existence of "looping effects," in which the way we conceptualize psychological phenomena (e.g., prejudice) feeds back to influence these phenomena themselves. Again, however, I worry that Teo has imposed a categorical distinction on a dimension. Although looping effects and other distinctively human phenomena surely add a formidable layer of complexity to scientific efforts to explain behavior, they do not imply that human and natural psychology differ in kind rather than in degree. Nor is there any reason that these phenomena cannot themselves be subjected to stringent empirical scrutiny using scientific methodologies.

I conclude on a conciliatory note. Teo (2012) and I very much agree that one source of the average layperson's skepticism toward psychology is our field's vexing and at times exasperating proclivity to promise more than it can deliver. His admonitions regarding the hazards of "a celebratory selling of goods to the public" (p. 807) and the need for "modesty when making generalizations" (p. 807) are well-taken reminders that science, at its core, is a systematic prescription for humility. Science, including psychological science, is our best safeguard against epistemic hubris, as it acknowledges that all of our knowledge is fallible. Yet science also informs us that not all of this knowledge is equally trustworthy and that some conclusions are better supported than others. Ultimately, our field will earn credibility in the eyes of the public not by instilling false hopes but by striving for intellectual honesty. This honesty, in turn, impels us to be circumspect when telling the public what we know—and to openly acknowledge what we do not know.

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