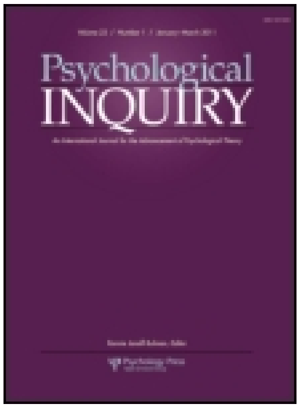

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Scientific Utopia or Scientific Dystopia?

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Scientific Utopia or Scientific Dystopia?

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I am grateful for the opportunity to comment on Brian A. Nosek and Yoav Bar-Anan's (this issue) article. I applaud their willingness to challenge the status quo of scientific communication, and I find myself in agreement with many of their bold proposals. In particular, I second wholeheartedly their calls for digital communication, open access to all published research, publication of peer review, and ongoing peer review. I concur with them that each of these reforms would enhance the openness of scientific communication (see also Mahoney, 1985). As Merton (1942) noted, "communalism"—the tenet that knowledge belongs to all members of the scientific subculture—is one of the cornerstones of science. The more we can ensure that scientific knowledge is accessible to all players within that community, the better.

Nevertheless, I am less persuaded than are Nosek and Bar-Anan that their proposed step of disentangling publication from evaluation will be beneficial to long-term scientific progress; I actually worry that it might prove harmful. In this commentary, I lay out a few of my reservations regarding their daring vision of a scientific utopia. On a more constructive note, I also delineate several provisional criteria for ascertaining whether their proposals will be effective in enhancing scientific progress.

Diagnosis and Treatment

It may well be my training in clinical psychology, but I view it as crucial to offer a diagnosis prior to prescribing a remedy. If I understand Nosek and Bar-Anan's diagnosis correctly, they view the central "disorder" afflicting the world of scientific publication in psychology as inefficient communication, including file-drawer effects (Rosenthal, 1979), unduly lengthy publication lags, and inability to correct many blatant errors in published articles. Their proposed "treatment" follows directly from this diagnosis. I share many of their concerns with the often glacial pace of communication in our field, and suspect that each of the aforementioned problems plays some modest role in psychological science's malaise.

Nevertheless, I have one friendly point of disagreement with Nosek and Bar-Anan's diagnosis. In contrast to them, I do not view inefficient scientific communication as the underlying disorder per se. To the extent to which inefficient communication is a problem, it is at most one contributor to this disorder. Instead, I see the disorder itself as a far deeper and more encompassing problem, namely, what Meehl (1978) termed the "slow progress of soft psychology" (p. 806; see also Lykken, 1991; Miller, 2004). Meehl lamented that the traditionally soft fields of psychology, such as clinical, counseling, social, personality, and developmental psychology, usually lack the cumulative character of the traditional "hard" sciences, such as chemistry and physics.[1] Progress in much of soft psychology is often desultory, with numerous fits and starts. Short-lived fads abound (Dunnette, 1966), and most are replaced by other, equally unscientific fads.

Moreover, what we might term the "jettison rate" of dubious scientific models in soft psychology is troublingly low. As Meehl (1978) noted, most poorly supported theories and techniques in soft psychology fit General Douglas MacAuthur's sad description of old soldiers: They never die, they just fade away. In some respects, Meehl may have actually been overly charitable in his appraisal, as numerous theories and techniques in soft psychology remain disconcertingly vibrant in the conspicuous absence of theoretical or research support. In my own field of clinical psychology, there remain more than 500 different brands of therapy (Eisner, 2000), many of them still widely administered (e.g., attachment therapies, transpersonal therapies, energy therapies, sensory-integration treatments) despite precious little empirical backing. The same holds for a plethora of poorly supported assessment techniques. To take one example, witness the continued popularity of the Rorschach Inkblot Test in many clinical and some research quarters in the face of consistent evidence that it is a grossly suboptimal instrument on numerous psychometric grounds (Lilienfeld, Wood, & Garb, 2000). More than four decades ago, Jensen (1965) commented wryly that the "rate of scientific progress in clinical psychology might well be measured by the speed and thoroughness with which it gets over the Rorschach" (p. 509). By this admittedly fallible metric, the state of progress in clinical psychology leaves a good deal to be desired.

[1]Although I adopt the terms "soft" and "hard" sciences for ease of communication, I agree with E. O. Wilson (1998) that the so-called soft sciences typically address more difficult problems (e.g., the nature of consciousness) than do the hard sciences.

Ultimately, inefficient scientific communication matters only to the extent to which it impedes the bottom line of scientific progress. The central question, then, is not whether Nosek and Bar-Anan's recommendations would improve the flow of scientific communication. It is whether they would enhance long-term scientific progress, a point to which I return.

The Law of Unintended Consequences

As T. D. Wilson (2012) observed, the histories of clinical and educational psychology, among other fields, remind us that it can be exceedingly difficult to tell in advance whether an intervention will serve its intended purpose. As he pointed out, some interventions that appeared on their surface to be "sure bets," such as crisis debriefing for trauma-exposed victims or certain widely disseminated diversity training programs for reducing prejudice, have been found in controlled studies to be ineffective and perhaps even harmful (see also Lilienfeld, 2007). The often forgotten lesson here is that our commonsense intuitions regarding the likely success of interventions are often woefully inaccurate. For example, societal reforms that seemed virtually guaranteed to work have followed the law of unintended consequences, and have yielded unanticipated negative effects (Leaf, 2005). The apparent paradoxical effects of bans on texting while driving, which may have contributed to drivers texting more surreptitiously and hence more dangerously, come to mind as one recent example (Husak, in press). Hence, some of my disagreement with Nosek and Bar-Anan stems from their apparent certainty that their proposals for reform will eventuate in a scientific utopia. I am certainly open to this possibility, but I am less convinced than they are.

Admittedly, my diagnosis for what I see as wrong in psychological science seems to differ in at least one critical respect from that of Nosek and Bar-Anan. They accord nearly exclusive emphasis to the need to remove barriers to efficient scientific published communication. Moreover, they appear to place considerably more stock than do I in the power of publications to facilitate scientific progress. To be sure, published articles are essential vehicles for sharing and transmitting new scientific information among scholars. But they are merely one key ingredient in the formula for scientific progress.

Instead, I agree with my late Ph.D. mentor David Lykken (1991; see also Wachtel, 1980, 2007) that most of us in academia overvalue greatly the importance of publication, especially relative to good teaching; good mentoring; and, most of all, thinking deeply about difficult scientific questions. I could be mistaken, but I would prophesize that scientific progress in psychology would be enhanced if we all (myself included) published at least 50% less and used the lion's share of our remaining time to *think*, and to spend more time discussing ideas with our colleagues and students. For example, when reading Kahneman's (2011) magisterial book, *Thinking, Fast and Slow*, it is difficult not to be struck by the extent to which many of Kahneman and Tversky's seminal ideas regarding heuristics and biases emerged from lengthy and leisurely conversations between these two geniuses over walks and meals. This model of extended armchair idea generation seems almost unimaginable in the frenetically paced, publication- and grant-driven world of today's modal research-oriented university psychology department.

Why is my admittedly anecdotal hypothesis concerning the importance of "thinking time" relevant? I fret that Nosek and Bar-Anan's proposal to allow all research to be published will open the floodgates, leading to a massive outflow of even more publishing and even less pondering. I suspect that at least in the short term, many academicians who would otherwise be reluctant to publish scores of preliminary or low-quality investigations will now feel free to do so. In some departments, they might feel compelled to do so. One need not be a Skinnerian to forecast this outcome. Given the premium placed by university administrators on raw numbers of publications, most faculty members will probably be encouraged by their departments to publish still more. They may also be less likely to think deeply about complex psychological problems prior to conducting studies, analyzing data, interpreting findings, and submitting manuscripts. The increasingly mindless arms race of publication productivity endemic in much of academia could escalate still further.[2]

In fairness to Nosek and Bar-Anan, my worries may be unfounded. Perhaps they would respond to my concern by contending that in the long term, their proposal would actually result in *less* emphasis on sheer bean counting, because academicians and university administrators would eventually come to value peer reviewers' ratings of quality—and citation counts—over numbers of publications. If so, I hope they are right. Yet given the academic world's remarkable propensity for institutional inertia, I worry that it could take many years, if not decades, for attitudes concerning the importance of raw scholarly productivity to change. Publication productivity is the coin of the realm in much of academia, and we may need to be prepared to wait a long time for cultural norms to shift.

[2]As one example of just how out of hand this arms race has become, we (the psychology department at Emory University) recently conducted a search for an *assistant professor* in cognitive neuroscience. Of the four candidates who were brought in for job talks (all of whom were superb), the number of publications on their vitae ranged from 11 to 18, and many of the other candidates in the final running for the position had well over 25 publications.

How Can We Evaluate Scientific Progress?

If Nosek and Bar-Anan's provocative article has a significant omission, it is the short shrift they accord to the question of how they would ascertain whether their proposals are effective. As I argued earlier, enhancing open scientific communication is, ceteris parabus, a laudable goal, but it matters only if it helps to combat the slow progress of psychology. How could we determine whether Nosek and Bar-Anan's recommendations would enhance scientific progress in psychology? In closing, I outline a few provisional criteria in the interests of stimulating further discussion; I make no pretense that this list is exhaustive. Nor do I endeavor the challenging task of proposing quantitative metrics for such criteria. These criteria, it is worth noting, could also assist us in ascertaining whether my concerns regarding the unintended consequences of Nosek and Bar-Anan's proposals are justified.

1. *Enhanced rates of replicability of key findings*. Needless to say, Nosek has played a laudable leadership role in highlighting our field's misguided neglect of the importance of replication (see Young, 2012). If Nosek and Bar-Anan's proposals enhance scientific progress, we should begin to observe higher rates of independent replications of major published findings and fewer widely cited but nonreplicable findings (e.g., see Ritchie, Wiseman, & French, 2012).
2. *Higher jettison rate of bad ideas*. Many subfields within psychology, including clinical and counseling psychology, remain mired in pseudoscientific and otherwise questionable theories and practices (Lilienfeld, Lynn, & Lohr, 2003). If opening up the lines of scientific communication increases scientific progress, we should witness a more rapid rate of discarding blatantly false or poorly supported ideas.
3. *Increased capacity to generate accurate predictions, such as point predictions and range predictions* (the latter being predictions regarding the range of a parameter's value; Lykken, 1968). Most of the softer fields of psychology seem content with making directional predictions, such as predictions that a correlation will be positive rather than zero or negative or that the mean scores of one group will be higher than that of another (Lilienfeld, 2004). Such hypotheses, which entail minimal theoretical risk, usually offer at best feeble corroboration of psychological theories (Meehl, 1978). One mark of scientific progress is a field's capacity to generate more precise predictions, which in turn should allow for more efficient filtering out of inaccurate models (Popper, 1959).
4. *Greater integration across multiple levels of analysis*. Most psychological phenomena can be conceptualized at multiple levels of analysis, ranging from biochemical and physiological at the lower rungs to social and cultural at the higher rungs. Yet far too often, psychological investigators examine phenomena from only their preferred level of analysis. One indicator of scientific progress in psychology would be the extent to which we observe an increasing trend toward explanatory integration of psychological phenomena at multiple levels of the explanatory hierarchy (Kendler, 2005).
5. *More effective and efficient translation of basic into applied research*. Another sign of scientific progress is the extent to which basic scientific findings enhance human welfare. In my field of clinical psychology, scientific progress should become evident in larger effect sizes for psychological interventions, such as psychotherapies, derived from basic work in the laboratory.

Concluding Thoughts

Nosek and Bar-Anan are to be congratulated for drawing the field's attention to the plodding pace of scientific communication in psychology, and for offering constructive proposals for remedying it. They have initiated a much needed debate, and despite my reservations, I very much hope that our field gives their thoughtful recommendations the consideration they deserve.

Note

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