

An Adversary Model of Manuscript Review: Further Comments

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Rychlak and Rychlak (1991) make a number of interesting points regarding my proposal for a model of manuscript review that is based on an adversary (i.e., legal) approach rather than on the traditional "empirical assessment" approach (Bornstein, 1991). In this paper, I address three issues raised by Rychlak and Rychlak: (1) how the "burden of proof" should be allocated in the adversary model; (2) what definition of "proof" would work best within the framework of the model; and (3) how the adversary model changes the relationship of author, reviewer and editor during the manuscript review process. Continued discussion of the strengths and weaknesses of the adversary model—and of the traditional manuscript review system—is clearly warranted.

Mahoney (1985, 1987) noted that the vast majority of articles published in psychology journals are actually read by very few researchers. If this is true, then we are forced to conclude—as Mahoney did—that most published research is never integrated into the scientific mainstream, and consequently has little impact on our discipline. This rather pessimistic conclusion is, unfortunately, quite consistent with my personal experience. Most of what I have written seems to have gone unnoticed by large segments of the professional populace. However, my paper entitled "Manuscript Review in Psychology" (Bornstein, 1991) is an exception. It has produced strong reactions from colleagues and critics. Even before the paper was published I received a number of inquiries regarding the manuscript and its proposals. Clearly, the controversial nature of this paper has much to do with the strong (and varied) reactions that it has elicited.

Rychlak and Rychlak's (1991) thoughtful discussion of my paper is most welcome. I am flattered that they found the ideas presented in the paper intriguing enough to comment upon, and I am grateful to have been given

this opportunity to respond to Rychlak and Rychlak's critique. My purpose here is not to defend my ideas or my paper. The original proposal was intended to be heuristic rather than definitive, and therefore suggestions for modifications in that proposal are clearly appropriate.

Before responding to their discussion of my work, I'd like to put Rychlak and Rychlak's commentary into the proper context, by briefly noting one point. My original paper was divided into three broad sections: (1) a psychometric analysis of the manuscript review process; (2) some hypotheses regarding why the reliability and validity of manuscript reviews are often found to be inadequate; and (3) a proposal for an new model of manuscript review that is based on an adversary (i.e., "legal") model rather than a scientific (i.e., "empirical assessment") approach. Rychlak and Rychlak's discussion focuses exclusively on the final third of my original paper. Thus, in this comment I will also confine my remarks to issues having to do with the adversary model.

With this in mind, I'd like to respond to some of Rychlak and Rychlak's points. My commentary will be divided into three sections. First, I will discuss Rychlak and Rychlak's suggestions regarding the relationship of the "burden of proof" to adversarial procedures. Second, I will discuss the nature of "proof" as it is conceptualized in the legal system and in my proposed model. Finally, I will discuss Rychlak and Rychlak's ideas regarding the roles of author, reviewer and editor in the adversary model of manuscript review.

The Burden of Proof and the Risk of Non-Persuasion

Rychlak and Rychlak provide a clear and concise description of the relationship of the "burden of proof" (i.e., the "risk of nonpersuasion") to adversarial procedures in civil and criminal trials. The main point of their argument is this: the party with the burden of proof (i.e., the prosecutor or plaintiff) typically is given a number of procedural advantages (e.g., the opportunity to make the initial opening statement, the chance to make the final closing argument), that afford this party a realistic chance of persuading an impartial third party (i.e., the judge or jury) of the soundness of its argument. Rychlak and Rychlak suggest that because my adversary model does not adopt this same procedure, the reviewer of a manuscript is placed in an impossible position. They note: "Dr. Bornstein, being an author, gave to the author all of the procedural advantages (going first and having the opportunity to rebut), but also cloaked the manuscript with the presumption of publishability With such procedural advantages, and an initial presumption of publishability, most articles should be found 'publishable.' Dr. Bornstein has given the 'prosecutor' an almost impossible case to win" (pp. 472-473).

I disagree. The typical manuscript contains dozens—if not hundreds—of points that may be disputed by a motivated critic. Every conceptual, method-

ological and statistical argument made by the author represents a possible "flaw" in the manuscript. As researchers well know, the identification of a single critical flaw in a paper is sufficient to render the manuscript unpublishable, even if 99% of the author's arguments and reasoning are airtight (a point that I made in my original paper). The critic need not be an expert in the field to discover a critical flaw in a paper; she merely needs to be attentive to the methodology and statistical techniques used in the study, and to be reasonably well-versed in the topic area being investigated.

It is important to note that Rychlak and Rychlak's criticisms in this area apply equally well to the current review system. Under the current system, as in my proposed model, manuscripts are (at least ostensibly) presumed to be publishable unless they are shown to be fatally flawed (APA, 1983). The procedural disadvantages that burden the reviewer in the adversary model (i.e., not making the "opening argument," having a relatively short time to critique the paper, knowing that an author can challenge or appeal a negative review) also burden the reviewer under the current system. My intention in proposing a shift in our conceptualization of the manuscript review process from an "empirical assessment" approach to an adversary model was not to render impossible the task of the reviewer. After all, I am not only an author, but I am a reviewer as well. Rather, my proposal was intended to make the manuscript review system more efficient, to improve the quality of manuscript reviews, and to make explicit the adversarial quality that necessarily and inescapably underlies the reviewer-author relationship when scientific work is being critiqued and evaluated.

Rychlak and Rychlak's suggestion that it might be useful to regard submitted manuscripts as unpublishable until proven otherwise (rather than regarding submitted manuscripts as publishable until proven otherwise) is interesting. This presumption might work well, both in the adversary model and in the traditional empirical assessment approach to manuscript review. Although the policy statements of journals and journal editors are ambiguous with respect to this issue (see, e.g., APA, 1983, pp. 171-173), my understanding of the current system is that it is analogous to my proposed system in this regard. Each submitted manuscript is assumed to be methodologically and conceptually sound upon submission. The manuscript presumably becomes unpublishable only when a fair and impartial reviewer manages to uncover some significant, uncorrectible flaw in the paper (see Bornstein, 1991; Mahoney, 1987). If we continue to utilize the present manuscript review system, perhaps we would do well to adopt Rychlak and Rychlak's suggestion in this area, and make explicit the presumption that each submitted manuscript is, by its very nature, unpublishable. This could be made a standard part of the editor's "receipt of manuscript" letter, and would also be noted in the "Instructions to Authors" section of each psychology journal.

Definitions of Proof

In my original paper, I did not address the question of what definition of proof should be utilized in the adversary model. As Rychlak and Rychlak point out, there are three general definitions of proof utilized in the legal system: (1) "beyond a reasonable doubt" (the most stringent criterion); (2) "by clear and convincing evidence" (a moderately stringent criterion); and (3) "by the preponderance of the evidence" (the least stringent criterion). The choice of an appropriate definition of proof in the scientific and legal arenas is influenced by many factors (see Levine, 1974; Rosenthal, 1990). Clearly, one important consideration must be the perceived "cost" of an incorrect decision. For example, if in the legal setting, the cost of an erroneous acquittal is deemed to be less than the cost of an erroneous conviction, then it is logical to adopt a very stringent definition of proof. If the cost of erroneous acquittal is regarded as greater than the cost of erroneous conviction, then a more lenient definition of proof should be adopted.

In the scientific arena, the question of whether erroneous conviction (i.e., a verdict of "unpublishable" for a paper that actually should have been published) is more costly than erroneous acquittal (i.e., a verdict of "publishable" for a paper that is significantly flawed) is not easily resolved. This rather thorny question can be restated as follows: Is it better to publish some flawed research so as not to miss publishing any sound papers, or should we instead allow some sound papers to go unpublished in order to avoid cluttering up our journals with unsound research? If we choose the former route, psychology journals will publish a substantial number of flawed papers, wasting journal space (which is already at a premium), costing researchers time and money (as they are forced to follow up on research that is seriously flawed), and making the field look bad. On the other hand, if we choose the latter route, we might fail to publish some important and valuable findings, thereby depriving the scientific community of new knowledge and potentially depriving the public of the benefits of scientific, medical and technological progress.

Needless to say, a thorough discussion of these issues is beyond the scope of this paper. Numerous scientific and economic factors will play a role in determining how this question is ultimately resolved (see Bornstein, 1990 for a discussion of some of these factors). Nonetheless, given that there are significant costs involved in erroneous publication decisions as well as in erroneous non-publication decisions, it seems both conservative and reasonable to adopt a middle-level criterion of proof (i.e., "clear and convincing evidence") until the implications of this criterion can be debated more fully. A middle-level criterion of proof would simultaneously minimize the number of "false positives" (i.e., manuscripts that should not have been published but

were), and “false negatives” (i.e., manuscripts that were not published but should have been) during the review process.

The Author–Reviewer–Editor Relationship

Everyone who publishes in scientific journals recognizes that the author–reviewer–editor relationship is, to say the least, peculiar. Reviewer (and sometimes auctorial) anonymity unquestionably contributes to the peculiarity of this relationship, and creates an unusual—if not bizarre—flow of communication among the parties involved in the manuscript review process. Thus, the author knows the identity of the editor, but not that of the reviewer. Conversely, the reviewer always knows the identity of the editor, but often doesn’t know the identity of the author. The editor knows who everyone is, but is not permitted to reveal the identity of reviewer to the author (and vice versa when blind review is used).

The author–reviewer–editor interaction is further complicated by the fact that the role of the reviewer is ill-defined and ambiguous (see Mahoney, 1985, 1987). Specifically, it is unclear whether the reviewer’s primary obligation is to the author (to provide constructive feedback regarding a manuscript), or to the editor (to provide an assessment of the manuscript’s publishability). In fact, over time it seems that the role of the reviewer has shifted from emphasizing the former task (i.e., providing constructive feedback) to focusing on the latter task (i.e., evaluating publishability). This shift began to occur as journal space became more scarce, the population of publishing researchers grew, and competition for scientific resources became increasingly intense (Bornstein, 1990, 1991).

The roles and responsibilities of the participants in a courtroom proceeding are more clearly defined than are the roles and responsibilities of author, reviewer and editor during the manuscript review process. Furthermore, because the identity of each participant in a courtroom interaction is known to all the other participants, each participant is more fully accountable for his or her actions than would be the case if anonymity were the norm. We would do well to follow the legal system’s lead in disambiguating the role of participants in a manuscript review procedure, and in making manuscript reviewers accountable for their actions by removing the cloak of anonymity that encourages a hostile (or lazy) reviewer to make arbitrary, unsupported or *ad hominem* arguments (see Bradley, 1981; Garcia, 1981). An important advantage of the adversary model is that it clarifies the roles of author, reviewer and editor during the manuscript review process, and—as I noted in the original paper—makes reviewer anonymity unnecessary and undesirable.

Rychlak and Rychlak suggest that the adversary model of manuscript review places excessive burdens on reviewers. They write: “Under Dr. Bornstein’s

proposal, the referee is required to put forth a significant effort, under rather tight time constraints, and his or her work is to be judged by the same standards used to judge the manuscript itself. This requires a significant time commitment, with little to show for it" (p. 475).

I agree completely. The work of reviewing manuscripts is rarely pleasant, and reviewers are never compensated directly for their efforts in this area. However, this unfortunate state of affairs characterizes the present review system as well as the adversarial system proposed in my paper. In fact, one advantage of the adversary model over the traditional "empirical assessment" approach is that the reviewer's work is made somewhat easier by virtue of the fact that the reviewer is asked only to critique the paper rather than being asked to both critique the paper and render a judgment regarding its publishability. While I believe that reviewers should somehow be compensated for their efforts (see Bornstein, 1990), I also believe that the task of the reviewer is actually made less demanding—not more demanding—when an adversary system is used.

Conclusions

Rychlak and Rychlak's thoughtful critique makes explicit a number of ambiguities and unresolved issues in my original proposal for an adversary model of manuscript review. The problems that they discuss are not insurmountable (as they point out), and although I do not agree with all of Rychlak and Rychlak's criticisms, many of their suggestions seem to me to be quite reasonable. It is important to note that although we disagree regarding certain aspects of the adversary model (particularly with respect to the details involved in operationalizing and implementing the system), we agree regarding a number of important issues. For example, we clearly share the view that there are many significant flaws in the present manuscript review process. Similarly, we agree that there is a need to make evaluative criteria more explicit, and to clarify the roles of reviewer and editor in the review process. Furthermore, we agree that a review–rebuttal format might enhance the quality of manuscript reviews and stimulate constructive debate regarding controversial issues and findings. Finally, Rychlak and Rychlak and I concur regarding the most fundamental issue of all: we agree that an adversary model has potential value as a tool for increasing the quality of manuscript assessments in our field.

I genuinely believe that some version of an adversary model would work better than the traditional model of manuscript review. However, I do not maintain the illusion that psychology will willingly undertake the massive, complex procedural changes that would be required to implement the adversary approach on a wide scale. Realistically, I hope that eventually one or two journals might be willing to employ an adversary model of manuscript

review on a trial basis in order to evaluate its effectiveness, uncover hidden flaws in the model, and allow for a preliminary comparison of the adversary model and the traditional model of manuscript review. As I noted earlier, however, even if the adversary model is not adopted anytime soon, if the proposal stimulates constructive debate regarding the strengths and weaknesses of the present review system, then it will have served a useful purpose.

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