

See discussions, stats, and author profiles for this publication at: <https://www.researchgate.net/publication/232486259>

Comment on Summary Report of Journal Operations

Article in *American Psychologist* · July 1986

DOI: 10.1037/0003-066X.41.7.836

CITATIONS

7

READS

2

1 author:



Henry Roediger

Washington University in St. Louis

303 PUBLICATIONS 25,937 CITATIONS

SEE PROFILE

Some of the authors of this publication are also working on these related projects:



Question Order Effects on Quizzes [View project](#)

Comment

Contents

Roediger on the Summary Report	836
Kendler on Howard	837
Howard Replies	838
Snyder on Hyland	839
Kelly on Miller	839
Philp on Rollin	841
Glenn on Rollin	841
Moriarty and Allen on Rollin	842
Lansdell on Rollin	842
Rollin Replies to Lansdell	843
Einhorn on Gibbs and Schnell	843
Kirsch on Critelli	844

Comment on Summary Report of Journal Operations

Henry L. Roediger III
Purdue University

The interesting "Summary Report of Journal Operations" appears in the *American Psychologist* each year, most recently in the June 1986 issue on pages 700-701. The report includes basic information on submitted manuscripts, printed pages, and subscriptions for the 17 primary APA journals and the three special journals for the previous year. The data provide a wealth of information about the health of the journals and, indirectly, the fields that they represent. In this comment I discuss two unrelated features of the report: rejection rates and number of subscribers.

Probably the most remarked-upon figures from the Summary Report are the rejection rates, which average 74% for the 17 primary journals and vary up to nearly 90% for some journals. Some despair of publishing in APA journals with rejection rates running so high. However, the method used by APA to calculate rejection rates has a large inflationary factor built into it. True rejection rates are much lower than those published every year.

The most plausible interpretation of the rejection rates is that they represent the number of rejected articles divided by the total number of different articles submitted, multiplied by 100 to obtain a per-

centage. However, the denominator—the number of submitted papers—is not simply the number of different papers submitted. The reason for this is the manner in which manuscripts are counted, as put forward in the *Guidelines for Journal Editors*. Imagine that a paper arrives in the editorial office and is given a number. Following the review process, the editor takes the formal action of rejecting the current manuscript but inviting a resubmission after further analyses or experiments. This revision will, according to the rules, be given a new manuscript number when it is resubmitted. The same process could occur again, with a third manuscript number ensuing. If an editor were to follow this process for every paper and eventually accept them all, then the journal would still have a rejection rate of 67%, for only one of every three manuscripts would have been accepted. Even though this example is farfetched, this method of counting does serve to inflate rejection rates. (The reason for it is to determine the number of manuscripts that must be subjected to review for each journal, to aid in estimating costs for budgetary purposes.)

More subtle differences in editorial practices make it difficult to compare rejection rates across journals. For example, if a paper may be published with revisions, one editor may reject the paper and request a resubmission, whereas another might accept the paper contingent on the revisions. When resubmitted, the paper would be assigned a new number by the former but not the latter editor, with obvious consequences for rejection rates.

Beginning in 1986, editors of APA journals will begin keeping records counting both the number of total manuscripts handled (the old way) and the number of unique manuscripts received (the way most people erroneously assume data are currently figured). A comparison of rejection rates calculated both ways will be difficult on a yearly basis, because many submissions will not have received final action until much later. (The "Number Pending" in the Summary Report now provides a measure of editorial efficiency, for it indicates the number of manuscripts submitted the previous calendar year—say

1985—that have not been acted on when the June issue of the *American Psychologist* goes to press in April.)

Although the rejection rates probably receive the most attention, for me the most interesting figures have always been the number of subscribers. These figures are broken down, for each journal, into subscriptions of APA members and non-members, as well as institutional subscriptions. The *American Psychologist* is received by all members; according to figures in the June 1986 issue, *AP* went to 69,623 members in 1985. (This figure probably includes Associate members and student members, too.) One can then use this total to determine the percentage of APA members who subscribe to other journals. The figures are most disheartening, by almost any account. Some specialized journals have very low numbers of subscribers, but perhaps this is to be expected. More remarkable is the fact that even the journals that should be of general interest to all psychologists have pitifully few subscribers among APA members as a whole. For example, *Psychological Bulletin* and *Psychological Review* are taken by only 8% and 4.9% of the membership, respectively. Perhaps, one might guess, these are journals that appeal only to academic types and not to practitioners. However, the journals that should be of the most direct interest to practitioners are not heavily subscribed, either. *Professional Psychology* is taken by only 4.5% of all members; the *Journal of Consulting and Clinical Psychology* tops the list of the 17 primary journals by having 8,044 subscribers in 1985, but this only represented 11.6% of the membership.

These numbers of subscribers for the primary journals are dismal enough, but if anything the situation is really worse than they indicate. The total number of subscriptions of APA members to all APA journals (that is, totaling subscriptions to all 17 primary journals) was 42,404 for 1985, which is some 27,000 short of the total membership. But when the question is asked as to what percentage of the membership subscribes to none of the 17 primary journals, the answer is 70%. Although these data are not available from

the summary report, Michael Pallak (personal communication, August, 1985) reported that in 1984 only 19,000 of some 65,000 members subscribed to one or more of the primary journals.

Probably one can draw no firm conclusions from these figures, but to my eyes they portray a rather grim picture of the knowledge and competence of the profession as a whole. Apparently most APA members feel no need to keep up with work in the primary journals, which register the progress in the field since their graduate school days. Perhaps we have, to borrow a phrase, a field in which two-thirds of the members are "terminal PhDs."

REFERENCE

American Psychological Association. (1986). Summary report of journal operations. *American Psychologist*, 41, 700-701.

Let's Look at the Record

Howard H. Kendler

University of California, Santa Barbara

Howard (March 1985) tried to convince his audience, and I suspect himself, that the psychologist has the right, perhaps an obligation, to prescribe suitable values for society and individuals. His argument demonstrated first that the scientific enterprise is not value free. This is easily done. Science is a social effort that demands that scientists abide by the ethical principles of honesty, responsibility, cooperation, and so forth. Howard also acknowledged that value judgments enter into the core of science: the scientist's search for understanding. Properly, he recognized that scientists disagree among themselves about the "litmus test" for understanding (e.g., predictive accuracy, breadth of unifying power).

Once having admitted subjectivity into science, in the form of value judgment and research strategies, Howard tried to swing open the gates to allow subjectivity free rein in the scientific enterprise. He assigned Good-Housekeeping-type seals of approval to the subjectively oriented arguments of philosophers who support his views while conveniently ignoring those who do not. But even these endorsements are not sufficiently compelling. Finally, to close his case, Howard justified his subjective and value-oriented stance by concluding that psychology has unique characteristics (e.g., reflexivity) that are not shared by the physical sciences. Gradually, but surely, Howard led himself to the value-impregnated position that knowl-

edge should be sought that could "translate into desired results for humans" (p. 263). But who judges what is desirable, and how is the verdict arrived at? Let us look at the record to observe the possible consequences of the subjectivity that Howard encouraged.

Wilhelm Wundt knew what kind of society is desirable. His analysis of ethnic (social) psychology led him to postulate that evolutionary principles govern cultural development. Cultures evolve in a sequence of stages from primitive tribal organizations to the highly structured states of the present. In each culture, a distinctive group mind is created. Germany, he concluded, had achieved a truly organic society in which the needs of the state and individuals were properly balanced to produce a culture that emphasized heroism, duty, and spiritual ideals. In contrast, and much less desirable, were British and American societies, which were weighted toward the individual with excessive concerns with materialism, pragmatism, and commercialism. Wundt was essentially sponsoring social values of the Germany of his day within a presumed empirical relationship about the evolution of societies.

Abraham Maslow, like Wundt, thought that psychology could prescribe human values. In his research on the highest goal that humans seek, self-actualization, "the desire to become more and more what one is, to become everything that one is capable of becoming" (Maslow, 1954, p. 99), Maslow discovered a scientifically confirmed set of values. He accomplished this feat by first selecting the "best of mankind," individuals who were self-actualized (e.g., Beethoven, Eleanor Roosevelt, some of Maslow's acquaintances). Maslow identified their personality characteristics as spiritual in the humanistic sense, accepting of themselves, spontaneous, philosophical but not hostile sense of humor (poor Groucho Marx was never actualized!), and so on. From such evidence, Maslow (1961) inferred the "ultimate values which are right for mankind" (pp. 5-6). His empirical operations are transparently subjective (H. Kendler, 1980). By intuitively knowing who is self-actualized, Maslow employed the authority of psychology, which in this case was himself, to construct a so-called valid ethical system. M. Brewster Smith (1978) neatly summarized Maslow's effort: "Maslow has built no bridge between values and facts, not even a shaky one. He has stayed in the closed circle of his own values" (p. 190).

More recently, an ideological attack has been launched by Lewontin, Rose, and

Kamin (1984) against "biological determinism." The scientific status as well as the ethical justification of behavior genetics was challenged. Their suspicion of this research area stemmed from their Marxist bias, which they refreshingly admitted: "We share a commitment to the prospects of a creation of a more socially just—a socialist—society" (p. ix). This orientation encouraged them to believe that all psychological facts are embedded in a social context from which they cannot be separated. Within this orientation, they were encouraged to believe that positive findings in the field of behavioral genetics (e.g., genetic involvement in schizophrenia) would be used to attribute the social and economic inequities of capitalism to genetic differences among people. For them, the presumed social consequences of hereditary influences were more important than the truth value of the research findings. As a consequence of their ideological commitments, they made every effort to discredit research that supported a strong genetic component in the etiology of schizophrenia. Today, the evidence is so clearly overwhelming that the research problem has become one of identifying the genetic mode of transmission as well as environmental risks (K. Kendler, in press).

At the root of Lewontin, Rose, and Kamin's resistance to so-called "biological determinism" is the murky, holistic Marxist assumption of an "essential dialectical unity of the biological and social, not as two distinct spheres, or separable components of action, but as ontologically coterminous" (p. 76). This doctrine, therefore, justifies the dismissal of all research designed to uncover genetic influences even with an interactionist position. The authors cited Mao Tse-Tung as a practitioner and theorist of their Marxist approach, but unfortunately they failed to mention its relevance to the Cultural Revolution which, it should not be forgotten, operated with an antihereditary bias.

Understandably, psychologists would like to help improve society and the lives of individuals, and Howard is to be commended for his concerns. But a tough-minded and clear-headed approach that resists the conflation of facts and values, of the objective and the subjective, can be more useful in achieving "humanistic" goals than an approach that dictates so-called "humanistic" values.

Howard's methodological analysis erred in not fully pursuing the implications of Goodson and Morgan's (1976) recognition that all forms of understanding in the last analysis are subjectively determined. Acknowledging the subjective foundation of the act of understanding