

JOURNAL of  
COMMUNITY  
PSYCHOLOGY

ISSN 0090-4382

Journal of  
Psychol

ational

JOURNAL of  
CLINICAL  
PSYCHOLOGY  
Founded In 1945 by F. C. Thorelli

ISSN 0022-0061

of the  
of the  
Sciences

AN AUTHOR'S GUIDE  
TO PUBLISHING  
BETTER ARTICLES  
IN BETTER JOURNALS  
IN THE  
BEHAVIORAL SCIENCES

JOEL KUPFERSMID  
AND  
DONALD M. WONDERLY

PSYCHOLOGY IN

CPPC

Published Quarterly by  
Clinical Psychology Publishing Co., Inc.  
4 Conant Square  
Brandon, Vermont 05733



AN AUTHOR'S GUIDE  
TO PUBLISHING  
BETTER ARTICLES  
IN BETTER JOURNALS  
IN THE BEHAVIORAL  
SCIENCES

Joel Kupfersmid  
and  
Donald M. Wonderly



Clinical Psychology Publishing Co., Inc.  
4 Conant Square  
Brandon, Vermont 05733

Copyright © 1994 by Clinical Psychology Publishing Company, Inc., Brandon, Vermont.

All rights reserved. No portion of this book may be reproduced in any form, by photostat, microfilm, retrieval system, or by any means now known or later devised, without prior written permission of the publisher. Permission is not necessary for brief passages embodied in critical reviews and articles.

Library of Congress Catalog Card Number: 93-71124

ISBN: 0-88422-123-7



4 Conant Square  
Brandon, Vermont 05733

Cover design: Sue Thomas

Printed in the United States of America.

AN AUTHOR'S GUIDE  
TO PUBLISHING  
BETTER ARTICLES  
IN BETTER JOURNALS  
IN THE BEHAVIORAL  
SCIENCES

# CONTENTS

Chapter 1: Dissatisfaction with Research Reporting .....	1
The Problem .....	1
The Motivation for Publishing .....	5
“The Big Picture” .....	8
Chapter 2: Causes of Dissatisfaction: Hypothesis Test and Negative Results Bias .....	11
Statistical Significance Bias .....	11
Negative Results Bias .....	22
Chapter 3: Causes of Dissatisfaction: Replication Bias and Fraud ..	27
Replication Bias .....	27
The Quantity/Quality Problem .....	34
Fraud .....	35
Chapter 4: Writing Better Articles .....	45
Focus on Significant Issues .....	45
Overcoming the Problem of Statistical Significance Testing .....	48
Motivation and Knowledge of the Publication Process .....	53
Chapter 5: Getting Published in Better Journals .....	71
Citation Impact and Journal Rankings .....	72
Impact of Subjective Prestige and Journal Rankings .....	74
Impact of Journal Circulation and Journal Rankings .....	80
The Combined Impact Factor and Journal Ratings .....	84

Journal Ranking by Subfield .....	84
Using Author's Guides .....	89
Chapter 6: Recommendations for Systemic Change.....	95
Recommended Changes for University Personnel .....	95
Recommended Changes for Editors/Reviewers .....	98
Recommended Changes for Authors .....	107
A Fantasy .....	108
References .....	113
Index .....	127

# 1 DISSATISFACTION WITH RESEARCH REPORTING

This book addresses the problem of dissatisfaction experienced by many who publish in psychology and education journals. It is assumed that most behavioral scientists desire to write quality papers, documents that will be noticed and respected by others in the field. Our purpose is to attempt to reduce the proportion of articles that are considered to be inept. The audience should be—primarily—behavioral scientists who have published and want to improve the content of their articles, or who have not yet published but want to author papers with substance.

This book is not intended for beginning researchers or others who are unfamiliar with experimental techniques, conducting literature reviews, and writing manuscripts in APA style. There are a variety of good books and articles in each of these areas. Neither is it intended for those professionals who must publish, regardless of quality, in order to be hired, keep their jobs, and/or attain tenure or promotion.

## **The Problem**

Data from a variety of sources suggest that both practitioners and experimental psychologists are displeased with the content of articles in their professional journals. Lindsey (1977) asked: “How is it that so much triviality, illiteracy, and dullness is yearly entered into the scientific publication stream?” Mahoney (1976) claimed that “many scientific journals have seriously retarded our progress by absurd, inefficient, and often prejudicial policies.” Nelson (1982) added that “most published

manuscripts will be soon forgotten by editors, reviewers, and the general reader because they are eminently forgettable" (p. 229). Evidence supporting Nelson's position comes from studies on an article's "impact factor" (i.e., the extent to which a paper is referenced in other articles).

Garvey and Griffith (1971) reported that 50% of all research articles are read by approximately 1% of psychologists. These same researchers (1979) noted that in their investigation of over 200 research studies conducted by psychologists, only one study in seven originated from the investigator's reading a journal article or listening to a research presentation. Similarly, Matson, Gouvier, and Manikam (1989) found that the mean citation rate is less than one (0.92) per published article across psychology journals. One journal reviewer lamented that "one would like to think that if scientists were content with what is currently being published, they would pay more attention to it (cite it) when they write their own papers" (anonymous personal communication, September 11, 1987). Mahoney (1985) concluded that "the motivation to read seems to fall far short of the motivation to publish" (p. 31).

Surveys of mental health practitioners' attitudes toward the quality of what is published display considerable inconsistency. On the positive side, when asked what the best source of research information useful to their work is, mental health administrators and research members of grant review committees ( $N = 147$ ) rated professional journal articles at the top of the list (Weiss & Bucuvalas, 1980). Similarly, when psychologists employed at V.A. hospitals, medical schools, state hospitals, mental health centers, and in private practice ( $N = 224$ ) rank ordered the usefulness of eight sources of information, research articles were rated third on the list (Cohen, 1979).

However, more recent studies indicate that many mental health practitioners are dissatisfied with the content of their professional journals. In Thelen and Rodriguez's (1987) sample of clinical psychologists ( $N = 379$ ), only 42% reported that research is useful in clinical practice. Similarly, 37% of Morrow-Bradley and Elliot's (1986) sample of psychotherapists ( $N = 279$ ) claimed that journal articles were useful. When these same authors asked their sample to rank order nine sources of information in terms of usefulness, reading psychotherapy research and doing psychotherapy research were rated eighth and ninth, respectively. Rank ordering of the usefulness of eight information resources by child therapists ( $N = 30$ ) parallels the above findings. For child psychotherapists, empirical articles and empirical books were rated seventh and eighth, respectively (Cohen, Sargent, & Sechrest, 1986). Morrow-



Bradley and Elliot (1986) concluded that “with virtual unanimity, psychotherapy researchers have argued that (a) psychotherapy research should yield information useful to practicing therapists, (b) such research to date has not done so, and (c) this problem should be remedied” (p. 188). Barlow (1981) contended that “at present, clinical research has little or no influence on clinical practice” (p. 147). A stronger indictment was offered by Meehl (1978):

Most so-called “theories” in the soft areas of psychology (clinical, counseling, social, personality, community, and school psychology) are scientifically unimpressive and technologically worthless. In soft psychology theories rise and decline, come and go, more as a function of baffled boredom than anything else; and the enterprise shows a disturbing absence of that *cumulative* character. (pp. 806–807)

Surveys and comments of experimental psychologists also indicate considerable vexation with what is published. Only 15% of the surveyed members of the Society of Experimental Social Psychology ( $N = 229$ ), an elite subgroup of social psychologists, believe that existing criteria for evaluating research are appropriate (Lewicki, 1982). Perhaps more distressing are the findings reported by Ward, Hall, and Schramm (1975). These researchers had judges rate the quality of 114 *published* articles with the author’s name and affiliation removed. All judges were members of the Division of Measurement and Research Methodology of the American Educational Research Association. In only 40% of the articles reviewed from education journals would the judges have accepted for publication the article as submitted or with minor revisions needed. The remaining 60% of articles were judged to require major revisions (33%) or outright rejection for publication (26%). Results were not much better for psychology journals. The judges rated 43% of these articles as publishable in the form in which they were submitted, or requiring only minor revisions, whereas the remaining 57% required major revisions (29%) or deserved outright rejection (28%).

Former editor of the *Journal of Comparative and Physiological Psychology* Harry Harlow (1962) has been vocal in his frustration over the contents of articles published in research journals. He wrote that:

most experiments are not worth doing and the data obtained are not worth publishing. . . . Faced with a mounting flood of

uninspired researches and watching publication lag continuously mount. . . . I have the rubber stamp which I planned to use on a large number of manuscripts: "Not read but rejected." (p. 896)

Discontent with journal articles is evident in other disciplines as well. A sociologist and former editor of *Social Problems* wrote to his replacement that "if you can keep your head above the inundation of trivial manuscripts, you will glory in the occasional manuscript of brilliance" (Rodman, 1970, p. 269). Another sociologist (McCartney, 1976) believes "the most critical question we ought to ask is how the present system [of journal publication]. . . . serves the knowledge needs of the discipline. On this score, sociology journals deserve their lowest marks" (p. 147). Even among physicists there are rumblings about journal article quality. Cole (1991) reports the overwhelming majority of articles published in physics journals are rarely cited. A survey of physicists in England concluded that "*all* our respondents thought that the vast majority of papers in the journals which they read were of poor quality or of little significance" (p. 140).

Some of the most blunt criticism about journal content has come from the medical community. With specific reference to biomedical journals, Broad and Wade (1982a) claimed that "too many simply worthless scientific articles are published. Such publications not only prevent good research from receiving the attention it deserves, they also indirectly protect bad research from scrutiny by cluttering up the communications system of science" (p. 50). The current editor of the *Journal of the American Medical Association*, Drummond Rennie (1986), leaves little room for ambiguity about his position:

There seems to be no study too fragmented, no hypothesis too trivial, no literature citation too biased or too egotistical, no design too warped, no methodology too bungled, no presentation of results too inaccurate, too obscure, and too contradictory, no analysis too self-serving, no argument too circular, no conclusion too trifling or too unjustified, and no grammar and syntax too offensive for a paper to end up in print. (p. 2391)

Writing in the *Journal of Behavioral and Brain Sciences*, Tyrer (1991) offers the most pithy analysis thus far stated: "A determined author can get any rubbish published" (p. 164).

These findings suggest that many psychologists, regardless of specialization (as well as many other scientists from a broad range of disciplines), believe that a high percentage of articles published in their professional journals are uninteresting, uninformative, irrelevant, and not likely to contribute to the accumulation of scientific knowledge. The reasons for this widespread discontent with much of what is published in professional journals should be considered relative to the dynamics motivating many professionals to publish.

### The Motivation for Publishing

There is a tacit presumption that behavioral scientists across the country are studying psychological and educational phenomena in laboratories and clinics in order to advance the progress of science. It is assumed that these investigators must be addressing important concerns related to psychological processes and are collecting data that will aid in answering critical questions. When authors of articles in the *Journal of Consulting and Clinical Psychology* ( $N = 333$ ) were asked why they publish, 88% reported that it was for the purpose of following up on some previously published research (Kendall & Ford, 1979). Although this may be an accurate assessment, some believe that the basic reason for publishing often rests on less laudable motives. Mahoney (1985) contended:

While the ends sought through publication can be rationalized in terms of advancing thought and knowledge of a discipline, it is likely that publication is primarily motivated by personal ends of the producer. By and large, scholars are seeking enhancement of their personal reputations for the purposes of achieving tenure, increased salaries, job offers from other employers, and support for their research endeavors. (p. 20)

Bracey (1987) echoed these sentiments: "We seem to be headed toward a situation where 'knowledge production' (as they like to call it in universities) is an exercise in solipsism. The chief beneficiary is the author, who gets promotions, tenure, prestige, and more grants to write more stuff that won't be read" (p. 44).

Psychologists are human beings. As such, they often do first what profits themselves. Those trained in experimental and other primarily research-oriented areas of psychology have limited opportunities for

jobs beyond that of university employment. Thus, an experimental psychologist is captive to the standards set by universities in general, and psychology departments in particular. For practitioner psychologists desiring employment in a university setting, the same vocational dynamics apply.

The main criterion for being hired in many departments of psychology is the number of publications a candidate has produced, and it is not unusual for a young professor to want to remain at his or her place of employment after being hired. Being allowed to retain one's university position usually involves securing tenure. Again, the main criterion is the extent to which an individual has published since being hired. The next assignment a professor seeks is the supervision of graduate—preferably doctoral—students. Often the major criterion used to assess a professor's skills in this area is the extent to which he or she has published (Mahoney, 1976).

It is in the realms of publication and academic employment that we confront some of the most powerful selection processes in contemporary science. Publication, for example, lies at the very heart of modern academic science—at levels ranging from the epistemic certification of scientific thought to the more personal labyrinths of job security, quality of life, and self-esteem. . . . Teaching excellence, creative thinking, and all manner of other valuable attributes will do little to earn security in academic science if they are not accompanied by published payment to the piper of tenure. (Mahoney, 1985, p. 30)

Thus, although it is often assumed that university faculty are hired to teach and are heavily evaluated in this skill, the major factor determining professorial longevity at many universities is the extent to which one has articles in print (Boyer, 1990). In a national survey of university professors, 40% reported that at their university the number of publications supersedes the quality of the articles in determining tenure (Boyer, 1990).

In addition to job security and advancement, it is normal to want to be recognized in one's professional field. Mahoney (1976) listed the six most common forms of recognition a scientist can achieve:

1. Being invited to speak at professional presentations or to write articles for professional publications.

2. Having one's work cited in another's published paper.
3. Having grant proposals approved for funding.
4. Being offered employment at a more prestigious institution.
5. Receiving a special award for scientific achievement.
6. Having one's name associated with a phenomenon (e.g., Spearman rank order correlation, Thurstone scale).

Essential to achievement of any of these six forms of recognition is that the individual has published. It would be very unlikely for a scientist to be asked to speak or write an article, be cited in another's work, or receive special awards without having his or her work appear in print. For the same reason, one of the criteria for receiving a grant is that the submitter can document a fertile history of research experience. Likewise, to have one's name associated with a phenomenon (eponymity) almost universally requires one to have published its discovery. Being offered employment at a higher status university is frequently a function of one's publication history. Ghiselin (1989) maintained that "of course good teaching ought to be rewarded, but in academia its official recognition is little more than a public-relations gesture" (p. 71).

"Faculty research brings state, regional, and national visibility to academic institutions. It becomes a means whereby an institution can establish a reputation for outstanding faculty and demonstrate achievement and progress to the public" (Creswell, 1985, p. 1). When a scientist publishes in a professional journal, the article almost always includes the author's name and *institutional affiliation*. As a means of reducing the emphasis that is placed on publications in university settings, Tighe (1979) proposed that professors be allowed time to conduct research, with the proviso that the product not be submitted for publication until after the author's death. He termed this policy "perish and publish."

The "publish or perish" criterion is probably most pronounced in universities housing doctoral programs. In Kendall and Ford's (1979) study asking authors of articles their primary reason for engaging in research, 35% of the sample reported that conducting experiments was part of their job requirement. Creswell (1985) concluded his review of the dynamics surrounding the motivation to publish with the observation that little can be done to encourage those not interested in publishing and little can be engendered to stop those interested in publishing. Boice and Jones (1984) provided some support for this conclusion. They reported that about 10% of the scientists in a given discipline account for

approximately 50% of the published literature. It may well be that the majority of professors attempt to meet the minimum requirements of publications necessary for job security, tenure, and advancement.

In essence, for a great number of those publishing, *quantity* may well supersede *quality*. Mahoney (1979) contended that "as long as personal advancement depends on publication, we can expect scientists to display behaviours which are more expediently aimed towards their advancement, rather than that of knowledge" (p. 365). President of the Association of American Universities Robert Rosenzweig noted that scientists may have an image problem: "Mr. Chips has been replaced by Dr. Faustus, an ambitious, hard-driving entrepreneur, whose research, teaching and personal economic interests are very hard to disentangle" (Adler, 1989, p. 5).

In an effort to curb the proliferation of articles written for the purpose of meeting some university and/or academic departmental criterion for self-promotion, Stanford University president Donald Kennedy has instituted a new way of assessing the publication product of his faculty. Stanford University now gives cash bonuses for good teaching. Additionally, those applying for promotion are limited in the number of publications they are allowed to submit for evaluation. The authoring of textbooks will be on an equal footing with articles in professional journals (Gordon, 1991).

For many scientists the "university game" is publish, publish, publish. It behooves the researcher to know how to conduct experiments (a skill presumably learned in graduate school), and to learn the criteria by which journal editors and reviewers make a differential determination of the publishability of each manuscript submitted (a craft usually learned during the pre-tenurial years). Because large numbers of individuals *must* publish to obtain a desired position, *must* publish to retain their position, and often *have to* publish to advance in the position, the quantity of publications rather than their quality becomes the greater concern.

### **"The Big Picture"**

Mahoney (1987) stated: "There are over 40,000 current scientific journals, publishing 2 new articles per minute (2,800 per day and over one million per year), and this rate has been doubling about every six years" (p. 165). There are approximately 300 journals of a psychological and/or educational nature (American Psychological Association, 1988),

and about 8,000 medical journals (Broad & Wade, 1982b). Given the sheer number of professional journals, getting a manuscript published is not difficult. However, the task of writing a quality paper that advances one's discipline and having that paper published in a "better" journal can be formidable. Many empirical and quasi-empirical investigations related to the journal publication process will be reviewed in this book. There is within the domain of metascience a process termed "scientific evaluation" which involves assessing the quality of science and suggesting alterations that might improve its operation (Shadish, 1989b). This text may, thus, be appropriately considered a form of scientific evaluation.





## 2 CAUSES OF DISSATISFACTION: HYPOTHESIS TEST AND NEGATIVE RESULTS BIAS

The need to publish in the interest of professional survival dilutes the possible contribution of the scientific community. However, the approach to data analysis employed by most researchers, as well as the nature of editorial/peer reviewer bias in the manuscript acceptance/rejection process, adds a dimension that has resulted in an unnecessary distortion of the scientific knowledge bank.

### **Statistical Significance Bias**

It is virtually axiomatic that if one hopes to publish a study based on some form of statistical analysis (e.g.,  $t$  test, ANOVA, correlation coefficient), an associated  $p$  value must accompany the data. That  $p$  value is used to determine whether to retain or reject a null hypothesis. For over 30 years editors have been publishing criticisms about the use of significance testing. Yet such testing persists as the preeminent form of data analysis. However, when we attempted to locate, in professional journals, articles supporting its use, only a few papers were found.

Those who argue for the use of statistical significance testing as the primary means of hypothesis testing do so for a variety of reasons. Thus, the debate on this topic involves a number of issues, such as the use of a  $p$  value as a decision-making tool, problems associated with the interpretation of  $p$  values, etc. Our position is that the use of any predetermined  $p$  value for the purpose of making a decision regarding whether to retain or reject a (null or experimental) hypothesis is inappropriate.