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Editorial overkill

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Mandatory revision in accordance with reviewers' comments has apparently become the norm for articles published in certain quarters. Of the regular articles published in the 9-year period 1972-1980 by a sample of highly reputable journals (*British Journal of Psychology, Econometrica,* and the *Journal of the American Statistical Association*), 68% to 99% had to be revised subsequent to submission. These high rates of coerced revision place enormous power in the hands of reviewers to enforce conformity to their views while largely escaping responsibility, or accountability, for their actions. This situation is conducive to a variety of abuses that detract from the efficiency of the peer-review system and therefore constitute editorial overkill. These abuses are extensively discussed and illustrated.

The dissemination of some great scientific theories has been tragically delayed by the uncomprehending objections of colleagues regarded not merely as peers but even as superiors at the time. Isaac Newton was so vexed by the muddled criticisms of his peers "that he time and again protested that he would abandon science or would, at least, withhold his work from publication until after his death" (More, 1934, p. 42). He was so exasperated over the criticisms by Robert Hooke (one of his senior peers in the Royal Society) of his first publication, an article on colors, that "he persistently refused to publish anything further in that subject so long as Hooke was alive" (More, 1934, p. 393). Hooke died in 1703, and Newton's masterwork on optics was published in 1704, a third of a century after the youthful article that Hooke had criticized. Gregor Mendel, on the other extreme, was monkishly humble and welcomed criticism. He sent his now famous results on the genetics of peas to Carl von Nägeli, who was recognized as one of the foremost established experts in the area and whose favorite experimental plant was hawkweed. Not only was the significance of Mendel's data lost on Nägeli, but he encouraged Mendel to shift the focus of his experimentation from peas to hawkweed, a plant ill-suited to Mendel's methodology (Iltis, 1966). "The result was that Mendel labored in a blind alley for the rest of his scientific life" (Barber, 1961, p. 600).

These historical episodes illustrate some of the dangers inherent in peer review: If overdone, or done incorrectly, it can produce disgusted withdrawal by talented scientists (Merton, 1973, pp. 317-318) or unwarranted conformity to poor suggestions. It can also produce a variety of stratagems in coping with an oppressive system that does not command respect.

Many journals now accept and publish only about 10%-20% of the articles submitted to them (Council of Editors, 1981; Fienberg & Duncan, 1979), and this is much lower, at least for the more austere scientific areas, than in the recent past (Merton, 1973, p. 471). With such a low selection ratio, one might suppose that only the most carefully reasoned and well written reports of well designed and executed research had survived the selection process and, consequently, that the survivors had been accepted essentially as submitted. However, such is not the case. The journals are seldom content merely to select the best candidates for publication. They usually direct and supervise their modification as well (Bradley, 1981), thus introducing the specter of science by committee. "To look for a solution acceptable to all the professional experts is a familiar recipe for disaster–'Design by Committee'" (Ziman, 1980, p. 50). Acceptance is made conditional upon a satisfactory revision in accordance with the reviewers' suggestions. Or the article is "tentatively" rejected with the implication that the author can redeem himself with a properly responsive revision. Or, finally, the article is accepted, but the author is urged to give due consideration to implementing the changes suggested by the reviewers (and most authors will realize that acceptance of their future articles will be jeopardized by recalcitrance on the present one). Thus, revisions that occur subsequent to initial submission of the manuscript are highly susceptible to coercion to conform to the opinions and preferences of the reviewers. (Furthermore, even after final acceptance, substantive editorial modifications continue under the guise of making the manuscript conform to the journal's "style," etc.)

A mailed questionnaire (Bradley, 1981) revealed that the rate of compulsory revision is quite high among psychologists with previous publishing experience and that this high rate of coerced revision is accompanied by a variety of abuses which seem either attributable to or exacerbated by the power that it places in the hands of reviewers. The present article has two purposes: first, to supplement this information by determining the coerced-revision rate in a variety of fields using a different and perhaps superior methodology, and, second, to analyze and illustrate the apparently related abuses.

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METHOD

The more direct methods of information gathering sometimes distort the information obtained. In order to avoid this phenomenon, in which the measuring instrument influences the measures taken, the strategy of taking "unobtrusive measures" (Webb, Campbell, Schwartz, & Sechrest, 1966) was employed. The research to be reported will be concerned only with "regular" articles, thereby excluding presidential addresses, prestigious "lectures," invited articles, notes, comments, replies, rejoinders, obituaries, book reviews, and so on.

During the period from 1972 through 1980, the *Journal of the American Statistical Association* (JASA), *Econometrica* (ECON), and the *British Journal of Psychology* (BJP) have (with rare exceptions) accompanied each published regular article with information (such as dates for "manuscript received" and, in addition, if a revision occurred subsequent to receipt, "revised manuscript received") from which it can be deduced whether or not a revision took place subsequent to submission but prior to acceptance, and from which publication lag (as defined by the time elapsed from original receipt of the manuscript to the nominal date of the journal issue in which the article appeared) can be calculated for each article. This information was obtained for each regular article (for which information was given) and was then used to calculate, for the entire period from 1972 through 1980: (1) the percentage of regular articles that had been revised subsequent to original submission and (2) the "revision lag," defined as the amount of time by which the median publication lag for revised articles exceeded that for unrevised articles.

The three journals selected were chosen for the consistency of the labels used to designate the information provided. Correspondence with their editors confirmed that the writer was making the proper interpretations of this information. And, in particular, it confirmed that a revision was virtually always a necessary condition for possible publication when a date mentioning "revision" was given. Data collected from other journals yielded results similar to those obtained from the three mentioned (e.g., the revision rates for *Memory & Cognition* and for *Psychometrika* were similar to those of BJP and JASA, respectively). However, these data were omitted because of inconsistent labels or other difficulties in interpretation.

RESULTS

Results are given in Table 1. The overwhelming majority of articles underwent revision subsequent to submission, ranging from 68% for BJP to 99% for JASA. (The rate is 82% for BJP for the period from 1974 through 1980. There is little indication, however, of a substantial temporal trend for any of the three journals, except that the rate for BJP is very low, about 30%, for the first 2 years of the period covered.) These revisions typically prolonged publication lag by about 6-8 months, accounting for from 23% to 38% of the total publication lag (of about 2 years) for revised articles.

DISCUSSION

Prevalence of Mandatory Revision

Despite the fact that it increases publication lag by roughly 6 months or more, the results strongly suggest that mandatory or coerced revision not only has become the norm but, for some journals, has become a virtually automatic requirement. A revision was required of practically all of the regular articles published in JASA, including all 15 of those whose authors were formerly or concurrently editors of that journal at the time the article was submitted. (There were five such authors.) It is hard to believe that virtually none of the contributors to JASA can write a publishable article without help and even harder to believe that serving several years as editor of the journal does nothing to remedy this situation.

These results, obtained by objective and unobtrusive measures, are consistent with those obtained in a more conventional fashion. Ninety-one percent of the polled members of the elite Psychonomic Society, all of whom have published significant postdoctoral research, reported that a revision was required on the last article they published in a refereed professional journal, and 30% admitted that they had never had an article accepted by such a journal as originally submitted (Bradley, 1981).

Misallocation of Power

The obligation to rewrite one's article to conform to reviewers' comments places enormous coercive power in the hands of anonymous referees, who, although they are, in effect, belated coauthors, will not be publicly held responsible for the contents of the article. Thus, while exercising superior power, they escape commensurate responsibility, a very dangerous combination of circumstances. Furthermore, from the very nature of the situation, it is very doubtful that the reviewers of an article that is good enough to be accepted after modification are completely on a peer level of expertise with the author. Surely the author of a research article is the world's foremost authority upon the reported research, simply because he did it. Yet the author tends to be treated as a novice and the referees as experts who must be deferred to. I have been repeatedly astonished at the respect and deference with which I am treated as a referee in areas in which I may know relatively little and the arrogance, condescension, and contempt with which I am treated as an author in areas of research in which I have a quarter century of experience.

Editorial Overkill

The supremacy of reviewer over author invites a variety of abuses. A poll of university professors (Bradley, 1981) concerning their most recent refereed, revised, and published article revealed that, although 72% felt that the revision had improved it, large percentages of them encountered grievous flaws, such as the following, in the accompanying peer-review process: subjectivity (77%) and pressure to conform to it (76%), incorrect comments (73%), competence or sophistication inferior to their

Table 1
Statistics Concerning Revision Rate and Revision Lag (in Months) for Regular Articles Published From 1972 Through 1980 in the *Journal of the American Statistical Association* (JASA), *Econometrica* (ECON), and the *British Journal of Psychology* (BJP)

	JASA	ECON	BJP
Number of Revised Articles, N_r	1241	630	310
Number of Unrevised Articles, N_u	13	70	144
Proportion Revised, $N_r/(N_r + N_u)$	.990	.900	.683
Median Publication Lag for Revised Articles, M_r	21	24	19
Median Publication Lag for Unrevised Articles, M_u	13	18.5	13
Revision Lag, $M_r - M_u$	8	5.5	6
Proportion of Publication Lag Due to Revision Lag (for Revised Articles), $(M_r - M_u)/M_r$	.381	.229	.316

own (67%), concentration upon trivia (60%), treatment by referees as an inferior (43%), and careless reading of their article by a referee (40%) or editor (33%). Furthermore, 8% succumbed to the pressure and revised their article to conform to reviewers' comments that they knew to be wrong.

These abuses may be divided into at least two distinguishable varieties of editorial overkill: (1) attempts to force the author to conform to reviewers' comments and (2) an excessive production by reviewers of unnecessary, trivial, subjective, obnoxious, or incorrect comments. The former seems clearly encouraged by the high rate of mandatory revision and the unbalanced power it places in the hands of reviewers. And at least a portion of the latter seems attributable to the total or partial escape from accountability afforded by this excessive power and privileged position. Both types of editorial overkill will be discussed below.

Referees

To be asked to referee is an honor implying recognized expertise. Declining it raises the suspicion that one is shirking his professional responsibilities or, worse, that one is embarassingly incompetent to perform the task. Yet the task itself tends to be a tedious chore. There is a strong temptation, therefore, to accept the honor as well as to slight the task, and this is the case even if one is incompetent. Many objectionable referee behaviors, therefore, can be attributed to some combination of egotism, indolence, and ignorance.

The honor is apparently interpreted by some referees as implying their superiority to the author and by others as a license to impose their views upon him and to force the inclusion of their views, and even their literary style, in the author's article. By some, the chore seems to be minimized by criticizing the article on the basis of a hasty and careless glance-reading. Others, slightly more conscientious, apparently resort to the ploy of reading the manuscript carefully enough only to detect the trivial and easily discovered literary and scientific peccadillos, but not the important scientific flaws, which are far harder to find. Their review of the article, then, amounts to little more than "cataloging the trivia" in a perfunctory effort to convince the editor that they have done their job.

Thus, I have frequently been subjected to pontifical and patronizing lectures on what I already knew by "superior" referees who assumed that I was ignorant of all information that was not explicitly presented in the article and who then demanded that I include it all in a drastically shortened revision. Sometimes the superior pose is combined with ignorance or incompetence. On several occasions, a referee has claimed that everyone already understood my message and then has inadvertently revealed that he had completely misunderstood it himself. Many referees have given me detailed instructions on the substitution of their words and ideas for my own. Numerous referees have revealed their hasty or careless reading by countless misapprehensions or false criticisms. Over and over again I have been accused of saying what I have not said or of not saying what I have said. On several occasions, a referee has claimed as my "main conclusion" a relatively unimportant finding that I had mentioned only in passing and had not identified as a conclusion at all. Incompetent referees have often ignored my purpose and frame of reference (which amount to qualifying conditions) and criticized my article from the perspective of their own irrelevant or inappropriate frame of reference, or they have demanded that I abandon my own frame of reference and adopt theirs. Finally, if challenged, such referees have generally responded with unconscionable contradictions of fact or logic or with blatant sophistry. Thus, refereeing often degenerates into nonproductive and ego-glorifying (or ego-defensive) gamesmanship.

Referees are supposed to be disinterested peers, but the process by which they are selected often works against this goal. Many of the colleagues I polled (Bradley, 1981) and I myself have been asked to serve as referee in cases in which we were unqualified and have encountered incompetence in the referees of our own articles. Referees are often selected from the list of references in the manuscript submitted for publication. This tends to insure at least a gross level of competence, but it is also conducive to bias. The referenced research has often been referred to either favorably or unfavorably, and this, of course, has predictable consequences. Therefore, authors sometimes adopt the ploy of salting their manuscript with favorable references to congenial colleagues in the hope that they will be selected as referees. (Authors who have referenced me have sometimes supplemented this gambit by telephoning me for no apparent reason other than to lavish praise and admiration upon me for having inspired them to perform the research reported in the article–which they carefully neglect to mention, but which I receive from the editor a few days later.) However, referencing "friendlies" is often impossible if one's conclusions cast doubt upon those of one's predecessors. In that event, one is likely to find the path to publication barred by outraged disciples of the prevailing view who simply cannot believe that an established zeitgeist could be wrong (Bradley, 1978). Thus, a strong bias appears to operate in favor of authors whose results are consistent with those of friendly colleagues and against authors who do not enjoy this advantage (Barber, 1961; Mahoney, 1977), especially if they are loners cursed with iconoclastic results (Bradley, 1978).

Editors

One might suppose that editors would intervene to correct the excesses of the referees, especially if appealed to by the injured author. However, this presupposes that the editor has sufficient expertise, and is willing to read the article carefully enough, to arbitrate the issue, and unfortunately, this is often not the case. The editor may do very little to earn the prestige of his office, relying almost entirely upon referees to do the tedious work and settling for the mentally untaxing task of simply transmitting information generated elsewhere. Under these circumstances, the editor is highly dependent upon the referees and therefore loath to offend them, so a strong bias exists in favor of accepting whatever the referees say and supporting it however nonsensical it may be. In many cases, the initial support appears to be completely automatic. After receiving my protest that additions insisted upon by a referee were totally irrelevant to my article, an editor admitted "to being a bit puzzled by the reviewer's insistence that they be included," although he had vigorously supported the referee before my protest. Sometimes an editor's behavior, in his effort to avoid reading the article and making his own decision, is bafflingly naive or obtuse. On several occasions, I have protested that a referee's comments were demonstrably wrong or illogical, only to have the editor ask the same referee if this was the case. The referee, of course, denied it (without presenting any supporting evidence and ignoring mine), after which the editor triumphantly communicated the denial to me as proof that the referee's position was correct. Many of my manuscripts (on nonrobustness or the prevalence of nonnormality–touchy subjects) have been rejected because the editor preferred to believe the referee's misrepresentations rather than my documented refutations. (Sometimes my refutations were accepted, but then new reasons were given for rejection.)

Copy Editors

Not only are ideas forced upon the author, but also the very words in which both these ideas and his own are expressed. Practically everyone seems to feel that he can express an author's ideas better than the author himself. Often, this is due to the fact that he has not really grasped the author's meaning, in which case complex ideas, subtleties, and delicate nuances (upon which the accuracy of the statement may depend) are often reduced by the "corrector" to simplistic (and frequently inaccurate) banalities. Editors and referees are guilty of this, but the

worst offenders are copy editors who are apparently assumed by themselves and by the editor to be able successfully to edit and improve material that they do not understand. Sometimes they obviously are not attending to meaning at all but are simply looking for certain suspect word combinations or dubious punctuation, which they then edit in isolation without ever reading the edited sentence for meaning. For example, a copy editor deleted the "has" in "The case where H_0 explicitly states what values p may have has already been covered" and one of the "ins" when I wrote of "the possible situations that he may be in in particular cases," and copy editors have often edited my quotations, apparently not having observed the quotation marks.

Yet when they do read for meaning, the "meaning" they concentrate upon tends to be highly localized and divorced from overall context, with the result that their changes tend to be either trivial or inaccurate or both. An enormous number of changes are pure trivia, representing nothing more than the substitution of the copy editor's personal preferences for what the author wrote, often to the detriment of the article. Thus, they have arbitrarily changed "a real but small effect" to "a small but real effect," inaccurately changed "hardly any" to "still fewer," unconventionally changed "chi-square" to "χ-square," and ungrammatically changed "data are" to "data is." These "busybody" changes are irritating but seldom fatal. A more serious type of change comes about because copy editors, despite their lack of expertise in the subject, often confidently assume that whatever they do not understand, or whatever seems strange to them, must be an error on the part of the author and should be changed. For example, a copy editor changed "weight" to "weigh" in "it permits one . . . to weight the various sources of information according to . . . reliability," and on many such occasions, a copy editor has drastically altered the meaning of my sentence by adding, deleting, or moving a comma. Occasionally, the inexpert copy editor, having missed the author's point, rewrites entire phrases to make the sentence conform to his misconception. Sometimes the resulting sentence is so illogical or devoid of meaning that the fallacious editing could have been aborted by the copy editor himself if only he had carefully reread the edited sentence. But more often, the substitution blunts or deflects the author's meaning in a way that is not at all apparent to the "corrector," who believes that the meaning of the sentence is unchanged. Yet, unless one writes an essay of justification for returning the sentence to its original form (which is very difficult when trying to explain a professional nuance to a nonexpert) and the editor fully understands the reasons given, one's correction of the "correction" is likely to be summarily overruled. In one 10-page article, I was overruled by the copy editor in 34 of 66 attempted restorations of meaning, although I had accepted many of the copy editor's changes. The result was an article embarrassing to me, but not to the anonymous copy editor.

Thus, the author's article is likely to contain a number of indefensible or poorly defensible sentences that he neither authored nor approved but for which he will be held responsible because the copy editor (or other reviewer) falsely believed the substituted verbiage to be equivalent in meaning. Copy editing is supposed to save us from making literary dunces of ourselves. However, I would much rather suffer the stylistic consequences of my own literary gaucheries than to suffer the scientific consequences of the copy editor's literary "improvements."

CONCLUSION

It would seem to conflict with good management theory that, of all the people involved in the final revision of an ultimately acceptable article, the one person who is probably most knowledgeable and highly motivated, and who will certainly be held most responsible for the final product, should have the least say in producing it. Peer review would make a lot more sense if reviewers would content themselves with ferreting out actual, incontestable flaws and errors rather than making unsubstantiatable allegations or attempting to force the author to substitute their words and opinions for his own. It is highly inefficient for an author to have to waste time and energy refuting false accusations by referees and restoring verbiage incorrectly changed by a referee or copy editor. It is even more inefficient for him to accept authorship of ideas that he does not hold and words that he does not agree with. Therefore, both efficiency and rationality would be served if the author, rather than the reviewers, were given the final say.

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