

WHAT'S WRONG WITH THESE PUBLICATIONS? OR, SOME PATHOLOGIES OF SCIENTIFIC AUTHORSHIP

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A 1992 National Academy of Sciences report concerned with misconduct in scientific research¹ and a book on fraud, plagiarism and other misconduct in scientific publishing² have drawn needed attention to these seamy sides of the scientific endeavor. Need more be said? I believe so.

We all progress in science by standing on the shoulders of our teachers and other scientific progenitors. But this progression need not, and should not, lead to our stepping on the faces of our scientific ancestors or contemporaries as we continue the enterprise of science and try to communicate something we think is new and valuable.

Here I classify and discuss some pathologies of scientific authorship, pathologies that inevitably arise from some aspects of human nature. Science is universal in scope, but only people do science. Therefore one must expect that their productions may sometimes reflect the frailties of human nature. In particular, the "publish or perish" syndrome that has developed over the past 50 years has led to great pressures to publish, especially for young academics near the beginning of their careers in science. Such pressure in turn can sometimes lead them to accept ethically improper shortcuts.

Thus although it should not be too surprising that scientists occasionally cut corners and make unjustifiable claims, these and other kinds of misconduct reflect badly upon the perpetrators if they are recognized and, more importantly, can result in appreciable costs to society even if they

aren't. As Edmund Burke cautioned in *Reflections on the Revolution in France*, those who make themselves the most conspicuous are not necessarily the most important. This is a lesson worth heeding in science as well as many other areas.

Such serious scientific fraud as fabrication or alteration of data and plagiarism have been much in the news in recent years. I will discuss three other less obvious types of problems that often appear in scientific publications. These pathologies do not usually involve overt fraud, but they nevertheless cause damage to individuals, science and society as a whole. Even though the costs arising from any single publication that exemplifies one or more of these three problems are likely to be small, the problems occur so widely across all areas of science that their total hidden costs may even exceed all those arising from overt fraud.

The three types, in order of increasing implicit costs, are (a) duplicate or multiple publication of the same or nearly the same work, (b) "serial" publication of many papers on the same subject, which contributes little or nothing beyond what was contained in the first paper in the series or divides a complete paper into several smaller parts, and (c) publication of work implicitly or explicitly claimed to be new without adequate reference to the existing relevant literature in the field.

The ethics guidelines of the American Physical Society are relevant to this discussion (see PHYSICS TODAY, January 1992, page 62). In part, the guidelines say: "It should be recognized that honest error is an integral part of the scientific enterprise. It is not unethical to be wrong, provided that errors are promptly acknowledged and corrected when they

are detected. Professional integrity in the formulation, conduct and reporting of physics activities reflects not only on the reputation of individual physicists and their organizations, but also on the image and credibility of the physics profession as perceived by scientific colleagues, government and the public."

Multiple publication is worst when two or more virtually identical papers are published in different, readily available journals covering the same field. Journal space is needlessly preempted and costs are increased without the likelihood of much increased audience. Although it is difficult for editors and reviewers to recognize multiple publication when it is nearly contemporaneous, this artifice to increase the number of publications is explicitly prohibited by most journals and so is fairly rare—but it still happens. I believe that multiple publication should not be considered particularly reprehensible, however, when the first paper appears in a low-circulation conference proceedings or when several versions of a paper are published in different journals having small readership overlap.

Although many authors would probably prefer to write a complete paper on their subject and findings, they can increase their publication count by dividing the work into an ongoing series of short parts or LPUs—least publishable units. Furthermore, they can publish the first part sooner rather than having to wait to complete the unity of the work. Such early but incomplete publication also helps one claim priority for new ideas and results.

Serial publication of this kind is thus advantageous for the author but often occurs at the expense of readers and society. First, the reader must

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initially judge the value of the work only on its first stages and must wait, sometimes for years, for its completion. Since such a series of parts must have connecting and background material in each part, its totality will nearly always be appreciably longer than that of a single paper covering the same material. Sometimes this practice can be carried to absurd lengths. I know of an example where at least 11 papers, published by the same authors over a seven-year period, were devoted to the analysis and discussion of the same solid-state data set. One or two would certainly have been sufficient. Unfortunately, because of the advantages of serial publication mentioned above, it is not likely to diminish significantly in the future.

Of the three pathologies, the most damaging and costly one is inadequate or missing attribution, as well as the unjustifiable claims arising from such inadequacy. Sometimes a paper that claims new results actually redoes, by the same or not significantly different methods, work already published by others. If citation of the earlier work is included, this is perfectly acceptable. If the overlap is great, however, a responsible referee would likely recommend rejection of the manuscript. If the lack of attribution is based on ignorance of the earlier work by the author and the referees, no serious scientific misconduct is present, provided the author moves quickly to acknowledge omissions and withdraw unjustified claims of novelty.

In one case of missing attribution, a distinguished physicist did not learn of the earlier work until after his paper was published. "I like to decide first what I think and then find out what other people think," he said. "How do you know what you think if you consult other people?" Nevertheless, he added that he should have tracked down and cited the earlier work. Many of us can sympathize with the desire to create knowledge independently of the work of others. But once completed, the obligation remains to fit it within the existing framework of the field.

Even in the case of honest errors of omission, there is a possibly large cost borne by both the authors of the earlier work and society as a whole. First, published corrections do not reach all readers of the new paper either near the time of the corrections or into the indefinite future. There is thus a good chance that the new work will become the standard, and it alone will be referenced in the future—causing obvious erosion of

the intellectual property of the authors of the earlier work.

The cost to society can likewise be considerable. Suppose the new work is the product of a research group supported by government grant or contract, the usual case in academic science today. The referees of the original proposal clearly were not aware of the earlier work, which in extreme cases may have already achieved all the goals proposed for support. Thus the support is approved, including summer salary for the principal investigator and salaries for at least several postdoctoral fellows and graduate students. After two or three years of such support, the paper describing the work appears, and someone points out that it has all, or nearly all, been done before.

In this extreme but nevertheless realistic example, nearly all the support costs were wasted; given adequate initial information, they could have been devoted instead to truly new investigations. Since research funds are extremely limited, I believe that this scenario depicts scientific misconduct, primarily by the principal investigator. By not carrying out an adequate initial literature search—for example, by means of an inexpensive search through one or more of the many currently available computer-based scientific databases—the principal investigator has wasted a great many thousands of dollars of public money and has failed in discharging his obligations to his research group and students.

In the worse case of misconduct, that of outright fraud by plagiarism, the authors of the paper know about the earlier work, but they probably consider it sufficiently deeply buried in the literature that they can plagiarize it with impunity and, anyway, they need another publication or two! Marcel LaFollette, in a book highly relevant to the present subject,² characterizes scientific plagiarism as possibly worse than outright fabrication of data and as "malignant."

Unfortunately, one cannot usually distinguish this criminal approach to authorship from the honest but lazy one described above. The situation is most likely to involve fraud if one or more of the following characteristics apply. First, the authors may refuse to acknowledge the priority of the earlier work and may even refuse to state whether or not they knew of it before writing their paper. Second, if one finds that the same authors have published several other papers on different subjects with inadequate or entirely missing attribu-

tion to earlier relevant work, one can probably reach correct conclusions about their general mode of operation: superlatively creative or just criminal.

Finally, very tardy acknowledgement of citation errors and incorrect claims may suggest fraud, but it probably is only characteristic of the type of person who doesn't do his or her homework before publishing. I am aware of a case where the authors of a paper based on work supported by the Federal government claimed 12 "firsts" in a single paper, although many of them were actually "seconds"! These authors did not publish corrections for well over two years after being made aware of earlier relevant work. By such delay, they knowingly or unknowingly prolonged the period during which credit for their work could be used to aid in personal advancement.

The practice of claiming "firsts" and novelty is a dangerous one and should be totally eschewed. For one thing, it is unnecessary: Acceptance for publication is itself evidence of likely novelty. Goethe said, "No one can take from us the joy of the first becoming aware of something, the so-called discovery. But if we also demand the honor, it can be utterly spoiled for us, for we are usually not the first." Let the marketplace make the judgment and thus avoid possibly embarrassing self-aggrandizing claims.

Following my own precepts, I wish to state that it is likely that everything said here has been said before in other ways, and I certainly do not claim priority for any of it. There is thus no need for anyone to cite this work. But if it helps reduce some of the pathologies discussed above, it will be worthwhile.

In conclusion, the following words of Francis Bacon in the preface to his *Maxims of the Law* should be a lifelong guide to all professionals: "I hold every man a debtor to his profession; from the which, as men of course do seek to receive countenance and profit, so ought they of duty to endeavor themselves by way of amends to be a help and ornament thereunto."

References

1. Panel on Scientific Responsibility and the Conduct of Research, *Responsible Science—Ensuring the Integrity of the Research Process*, vol. 1, Natl. Acad. of Sci., Washington, D. C. (1992).
2. M. C. LaFollette, *Stealing into Print: Fraud, Plagiarism and Misconduct in Scientific Publishing*, U. Calif. P., Berkeley, Calif. (1992). ■