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## Letters to the Editor

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## LETTERS TO THE EDITOR

To the Editor:

The recent article "Quality Control in OR Literature" (*Interfaces*, August 1978, Vol. 8, No. 4, p. 26) struck me as a clear case of trying to solve a problem by treating its symptoms. Gazis describes the "problem" in the opening paragraph: "the sheer volume of OR literature often prevents anyone from coping with it." In fact, the problem is not that a glut of OR articles have been written, but that there is now (and has been for many years) an exponential growth in the number of OR professionals. This is attributable to the rate of growth of the world population, which, when examined since the period when scientific periodicals first appeared (in the seventeenth century), gives some astounding figures pertaining to the number of living scientists (and, subsequently, to the proliferation of published articles). If I may be brash enough to consider OR as a relatively new branch of science and mathematics, it will soon become clear that there is little to be done about the number of "bad" papers submitted for publication.

The crux of the explanation is summed up by Derek de Solla Price [1]: "... Depending on what one measures and how, the crude size of science in manpower or in publications tends to double within a period of 10 to 15 years. The 10-year period emerges from those catchall measures that do not distinguish low-grade work from high but adopt a basic, minimal definition of science; the 15-year period results when one is more selective, counting only some more stringent definition of published scientific work and those who produce it. If this stringency is increased so that only scientific work of *very* high quality is counted, then the doubling period is drawn out so that it approaches about 20 years" [1, p. 6]. Thus, if every living scientist (good and bad) publishes an average number of articles in his professional lifetime (Price assumes an average of 3½ publications), we can see that 80 to 90% of all publications will have been published in one 45-year period!

If Price is to be believed, there seems to be little hope of ever resolving the problem presented in the Gazis article. One solution that comes to mind is to transform the ORSA/TIMS organization into a watchdog society similar to the American Medical Association, where membership is limited by the strict admission standards in medical schools, as well as the formal licensing procedures necessary for obtaining a medical practice. Since this is hardly foreseeable, another alternative is to try and cope with the problem by recognizing that researchers will continue to publish, and the number of publications is destined to increase at an exponential rate. A viable way to handle this situation would be for ORSA/TIMS to inaugurate an on-line automatic bibliographic system that would include citations of *all* OR-related material, similar to MEDLINE. (Again, I call upon the medical profession as a good example.) MEDLINE allows searching on titles, authors, abstracts, keywords, and many other citation elements, with online and offline printing capabilities.

So take heed, researchers! Maybe what the profession needs most is a little doctoring in the guise of an automatic bibliographic system. It may be true that we are treating the symptoms, but the disease is incurable.

### REFERENCE

- [1] Price, Derek de Solla, *Little Science, Big Science*, Columbia University Press, New York, 1963, p. 6.

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To the Editor:

While it is necessary to reduce the trash published, as discussed in the August *Interfaces* by Dr. Gazis, the flip side (the type II error) is important also. Won't the odds against recognizing the worthwhile or original or maverick be increased by listing the number of journals that have already rejected a manuscript? Consequently, I think the second step (page 29) is impractical and on balance useless. As to the other suggestions, I believe there is little evidence for them and that subjective judgement is the foundation.

Therefore, it appears that only the first step is productive. It gets to the heart of the problem. If prestige is the reason some publish, then ranking the prestige is about as much as you can do without sacrificing the innocent to punish the guilty (type II error).

In this connection, perhaps one should adopt a practice I understand some few journals do: those anonymous referees should review manuscripts by *anonymous* author s. That is, remove the author's name and affiliation (including references and tell-tale footnotes) from the version sent to the referees. (One special referee might be used to authenticate references and footnotes.) This would remove the halo effect and focus judgement on the merits which should reduce both type I and II errors. It would also reduce the threat and consideration of the retaliation effect also discussed.

I might add that I have edited and refereed many articles and have seen too many of both type errors both actual and potential.

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To the Editor:

Subject: Graham and Jahani; People, problems, and planning.

I read with interest the article "People, problems and planning: a systems approach to problem identification," by R.J. Graham and M. Jahani (*Interfaces*, Vol. 8, No. 1, Part 1, 1977, pp. 50—55). The problem I would like to consider is this: the paper focuses on a very important question, and proposes the use of Saaty's matrix approach, but the mathematical appendix is completely wrong.

I believe that, besides its rather heuristic foundation, Saaty's method provides a significant contribution to problems identification and ranking, projects management, and resources allocation. The approach requires the determination of the *maximum* eigenvalue and its unique associated (normalized) eigenvector.

The appendix to the paper contains some improper expressions, like "a vector  $\lambda$ , that solves the equation  $|A - \lambda I| = 0$ ." But, much worse than that, the proposed eigenvector for the example—(.39, .24, .37)—is not an eigenvector at all. The correct answer is (.08, .38, .54), which completely changes the conclusion to the example. Instead of concluding "that  $S_1$  and  $S_3$  are about equally important and that both are more important than  $S_2$ " the conclusion should be that  $S_3$  is clearly more important than  $S_2$  and  $S_2$  is much more important than  $S_1$ .\*

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\*See Erratum p. 123.