

On Publishing, Perishing and Promotion in Fisheries Science

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"**V**ALUABLE SURVEYS OF FISH DISAPPEAR WITHOUT TRACE. A high powered team that studied the [Sri Lankan] fisheries sector's two-year track record 1983-1984 was amazed to learn that the results of valuable surveys of fish resources carried out by Norwegian and Russian teams around 1977 have disappeared without a trace. ... Now we have to start all over again," a knowledgeable source said.

That was the headline and beginning of an article in the newspaper, *The Island*, 17 September 1985. I can guess what happened. The results were probably collated into a report of which one copy was made for each government (i.e., three copies only) via the scientific team. These scientists kept their copies, later fading out of the fisheries scene, perhaps unwittingly taking \$200,000 (cost of redoing the survey) worth of data with them.

On the other side of the world, there are not many western fisheries scientists who do not subscribe to the salary-tested adage "publish or perish," since most institutions marry the number of a scientist's publications to his/her rate of promotion. The scientific publication is certainly the moment of truth for a researcher's work. In the manuscript are revealed all the workings and logic of his/her work, laid bare to an extent never expected of a businessman or politician, and for far less monetary satisfaction.

The Promotion System

The reason for the "publish or perish" syndrome is nearly always tenure and promotion. Publication is the usual evidence of research; without it, the research may have no value. Exceptions are contract research, military research and oil/mineral data which are private or secret. However, given the governmental nature of most fisheries research organizations, it is often the case that simple duration of attendance ("seniority") prevails, such that a dogged individual who may do no more than fill in the time book can eventually become the director.

In more rational but bureaucratic organizations, some form of output is

expected and promotion is based on a scale for different publication types: for example, 10 points for a refereed journal article; two points for a technical report, etc. Solo authors nearly always get more points than if they share the credit. Senior authorship, particularly in an "et al." (more than two authors) paper is most desirable. The journal article is the peak, while conference papers and especially technical reports are always "poor cousins."

Such a system is very open to abuse, and scientists, being also human, can get up to some interesting "tricks," such as dividing their results into "least publishable units," meaning the shortest possible papers, and publishing the same material with maybe a different introduction in different formats, e.g., a journal and conference proceedings. A few papers can be found almost unchanged in two or more conference proceedings or journals.

Some senior scientists in, for example, China, India, Pakistan and parts of South-east Asia, are known to have their manuscripts written by junior staff members whose names do not appear in the article. In another, advanced oriental country, authors tend to recycle papers even within the same journal when multi-authored: the senior authorship rotates; but only about three-quarters of the data are to be found in any one of the papers, so they are not "identical".

I know one author who was told by the supposedly anonymous referee of his manuscript that his paper was publishable with some additional details - which the referee would provide only if he was made a coauthor of the paper! The (original) author said this virtual blackmail was not uncommon in his country. I dare not tell you where.

There is one journal, I was reliably informed, that will publish any paper - for a fee of Rs150.

In at least one academic institution, it is explicitly stated that the supervisor will coauthor anything written by his/her students. In another research institution, the supervisor puts his name first on all papers, although in a recent published paper his name was placed second (of four

authors) because he knew nothing at all about the subject.

Outright fraud seems to be rare in scientific publication. The chances of discovery are too great. The only known recent aquatic case was a paper on shrimp in the Arabian Gulf in *Marine Biology* (vol. 32, 1975) which the editor-in-chief discovered was "in essence, identical with a previously published paper," an FAO Fisheries Report, in fact. (See *Mar. Biol.* 35:294, 1976 for details).

Because of all these "loopholes," the publish-or-perish promotion system is not very efficient but persists partly because those who have passed through it have a vested interest in maintaining it. On the other hand, it makes even less sense to uncouple the written evidence of performance from promotion or other recognition.

A Dichotomy in Expectation

The contrast between the loss of the Sri Lankan fisheries survey reports and the unseemly behavior that sometimes surrounds journal article publication is vivid. It reflects differences in the roles of academic and applied research institutions which, in turn, reflects differences in their expected output.

There are fundamental differences between the university and the fisheries research institution. University research is (or should be) unfettered and basic. In the research institution, research is fettered - it must be linked to the institution's objectives; moreover, it is to some extent applied, depending on proximity to the industry. Few fisheries scientists are distant enough from the industry to have time to produce the advances that the rest of us subsequently apply to the fishery or fishpond. So, while primary literature is the beacon for university-based researchers, scientists in fisheries institutions find that their job depends on production of reports, sometimes internal periodical reports, but generally an "external" report that may be produced in just enough copies to satisfy the needs of the organization and/or donors (c.f., the Sri Lanka case). For the research institu-

tion worker, that may be all he/she has time to do before embarking on the next project; more probably he/she will be still struggling to finish the report while already working on the next project.

The technical report has enormous importance in fisheries science. This form of scientific communication has been perpetuated in the FAO Report and Technical Paper series, which are often underrated simply because of their dreary appearance. (Recently some have appeared in hardback book form and acquired a more respectable life of their own.)

Fisheries authors should take heart in the success of several contributions by J.A. Gulland, Rodney Jones and others who shaped fishery science, which were never properly published; and there are many highly cited "Rapp. P.-V. Reún. Int. Cons. Explor. Mer" papers.

The conference paper also has great importance for the fisheries researcher. It may represent the only opportunity for external announcement and appraisal of a project's results. Unfortunately, all the sweat of preparation and stress of presentation are often for nought when the conference organizers decide not to produce the proceedings or have them published directly from a mixture of typewritten and computer-typed originals provided by the authors, completely unedited.

To highlight the value of conference proceedings volumes, ICLARM's 1980 conference proceedings, *The Biology and Culture of Tilapias*, which was carefully edited and produced, had been cited 285 times (that we know) in the literature up to February 1988.

To make a first approximation of the relative importance of the various kinds of literature in tropical aquatic science, I counted the references in the only recent "tropical" book I could lay hands on easily. This was the *Ecology of Tropical Oceans* by A.R. Longhurst and D. Pauly (Academic Press 1987). It has received much praise from reviewers. There were 754 references, of which slightly less than half (48.5%) were primary literature. Reports (19.1%), proceeding papers (11.3%) and monographs (18.4%, which include mostly disguised reports and proceedings) made up almost all the remainder. In other words the "grey" literature was about as important as the primary.

So What?

Unfortunately, the heads of some fisheries organizations still expect university-type output from staff and prospective staff. When perusing an applicant's bio-data they expect (1) primary literature (refereed journal articles); (2) lots of it; and (3) in prestigious journals.

The easy-to-say solution is that such people should be made aware of the fact that some fisheries reports and proceedings are extremely well-cited while about half the fisheries primary literature never gets cited at all, regardless of the journal.

A more practical solution is to make sure that when setting up a project there is both time (to write) and money (to publish and distribute the report) allocated for this aspect. The report should be published in at least 500, not 5, copies. A mailing list can be easily built up from various directories of institutions and scientists. Make sure ASFA and other contents/abstracts journals are included.

The report should also be edited, i.e., checked by a person other than the authors. Fisheries scientists as authors don't have time for the niceties of style and organization that they expect in others' reports. Even small fisheries institutions can gain tremendously by having an internal editorial process.

The all-important (to the individual, especially) conference paper deserves elevation also. Organizers should take the trouble to edit, not just compile or reject. Inexpensive publishers can be found. Allocate funds for publishing; don't expect one of those well-known publishing names to go on their knees to take up the proceedings. As long as money is allocated to distribute copies widely, you may be better off without them.

Finally, because projects and conferences are not everyday events, a big output should not be expected from a fisheries researcher. Quality, not quantity, should be the key element of a supervisor's appraisal. Then we might see an end to many of those repetitious papers and other unwelcome "tricks of the trade."

A Different View

A colleague who heads a fisheries research institution in Southeast Asia read

my views as presented here and pointed out that the importance of quality is a good reason for taking the peer review - primary literature path "since many supervisors in Asia lack the competence for quality appraisal".

He said that "reports of investigations are best published in refereed journals to be part of the scientific literature. This is intended primarily for scientists (technology developers) and future scientists. Institutional publications (in-house publications) are better limited to materials for information dissemination. These are intended primarily for technicians (technology users) and future technicians. Examples are reviews, training manuals, and extension materials. They are also written by scientists from their and related papers in the scientific literature, and simplified by information specialists. Through the above process, technology is developed and refined as the in-house publications are repeatedly used and revised."

"Training future fisheries and aquaculture scientists by the above procedure might (a) eliminate losses of study reports and abuse in publication; (b) minimize pollution of scientific literature; (c) simplify evaluation procedures for promotion; and (d) hopefully give the user a technology that works."

My colleague's arguments have shown up my own biases. ICLARM produces a number of institutional publications, some of which have been quite successful. However, the refereed journal article is the most desirable outlet for original research results. No doubt about it. Even ICLARM staff feel that we may have neglected the primary literature by focusing on our own publications series.

However, the rejection rate of papers is high - 40% is not uncommon amongst journals - and much is due to lack of space rather than technical problems in the manuscripts. Where will those papers go? My concern in this article is that as long as authors continue to present original research results in reports and conference papers, these media should be produced more professionally and their contributions better recognized.

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