Fisheries Scientists Must Write

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It is possible that writing on reprints (see Newsletter April 1982, p. 18) and on citation analysis (see Newsletter April 1984, p. 6) before writing on writing itself was a case of putting the cart before the horses. But again, writing on writing is not easy, and besides, this seems to be a topic about which preconceived and ill-founded notions are most widespread. I can't recall, for example, how many times otherwise brillant and extrovert colleagues have "clammed up", muttering things against the "publish or perish" principle when asked why they do not write more, or at least something.

Now why, indeed, should scientists write? One could answer this as the historian would, by pointing out that modern science emerged near the end of the 16th century in Europe as a *public* effort, when the early scientists began to *communicate* about their assumptions, methods and results and to increasingly seek and depend on their colleagues' opinions.[1]

Or one could argue that one should write in order to be able to join those scientists already playing the game[2], or simply because it's nice to see one's name in print (except, obviously in libel suits, etc.).

More prosaically, the main reason why one should write up one's work is because it is not completed—even doesn't exist as far as one's colleagues are concerned—before it is written up and *published* (i.e., available to colleagues working outside your institution).

Writing up one's work *as part* of research leads to the conclusion that whoever works a lot should also write a lot. The converse of this is that whoever has written a lot should also have worked a lot (which forms the logical basis for promoting prolific scientists). Implied also is that whoever has not published much has not worked much either (at least as far as *scientific* work is concerned). But how about Albert Einstein?

Albert Einstein comes in here because colleagues who don't write tend to point out the fact that he supposedly got his Nobel Prize on the basis of a single short paper (which is *not* true), and hence that it is quality that counts, not quantity.

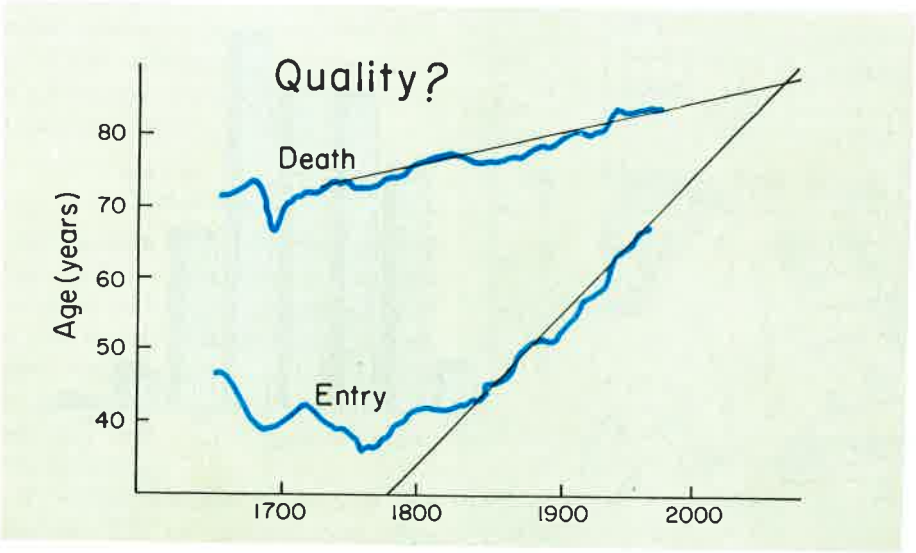
This argument doesn't hold for a number of important reasons:

1) It is bad for one's mental health to compare oneself with Albert Einstein (in fact, I am always amazed by the nerve of colleagues who put posters of him on the walls of their office—what are they trying to say?).
2) Albert Einstein was very prolific and authored or co-authored hundreds of articles and books over his career.
3) Given that we are born neither omniscient nor omnipotent, how else but through *practice* can we learn to improve our abilities to communicate results, ideas and even complex theories?

Scientists are paid to do research, shoemakers to make shoes; shoemakers who don't make shoes, or refuse to let people see them may justly be suspected to be doing nothing worthwhile. Now imagine a shoemaker who would say "How about Alberto Unarocca, he never made more than one pair of magnificent boots" (Sounds silly?—just imagine how *we* must sound to a shoemaker!).

One particularly sad aspect of the Einstein-did-not-write-much-either excuse is that it may in fact hold for physicists, but certainly not for fishery scientists. The results obtained by the former are explicitly meant to be independent of time and space (e.g., the value of a physical constant is the same, be it measured now in China or five years hence in Liechtenstein). In fisheries, as in all other biological sciences, such independence of time and space does not occur because biological systems evolve along a unique historical path. Thus, events such as the decline and downfall of a fishery and the stock that supported it, happen only once, and, if left undocumented will forever be lost, with all the lessons they might have contained, and the insights they could have generated.

Turning this essay from a diatribe against writer's cramp into a guide on how fisheries scientists should write is difficult, given the range of topics that would have to be covered for such guide



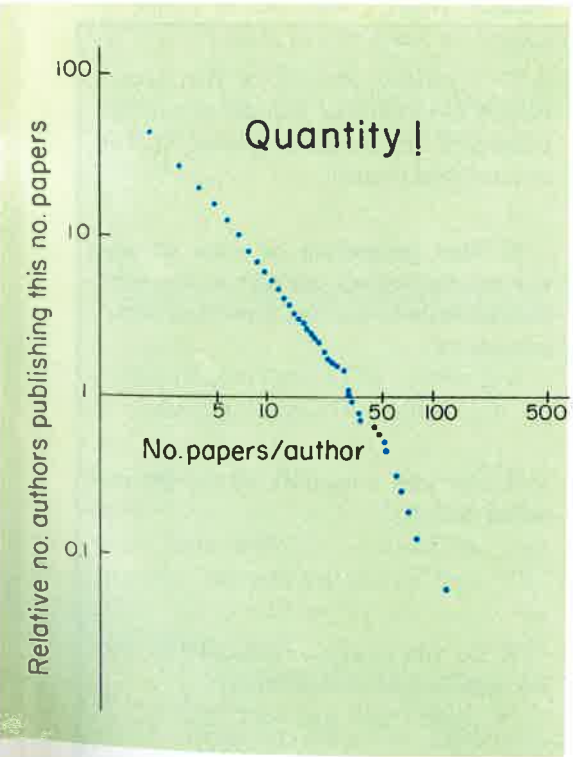
This graph shows the average age at which members enter and leave (by dying) the Académie des Sciences. The astute reader will note that in the second half of the 21st century, the members of the Académie will pass away the day they attain membership. This figure and that opposite are adapted from "The Force of Knowledge" by J. Ziman. 1976. Cambridge University Press, Cambridge.

to be effective. But here are some of the major points.

On Planning the Write-up As Part of the Research

Planning a research project without due regard to the ensuing publication is a waste of time—and neither should a few perfunctory weeks be tagged on to the end on a research project to "prepare report". Rather, a research project should be planned *backwards,* starting with the expected final product (a report, one or several papers, or even a book). Opportunities should be planned for intermediate results, preliminary reports, or data compilations (e.g., length-frequency data, station lists and catch data) which will allow timely identification of gaps or inconsistencies in one's sampling program, and provide a standard data set upon which to base further, more detailed analyses.

Such preliminary publication and data compilations, besides helping to prevent information losses frequent in research projects (such as fishery surveys), also offer opportunities for junior members of a team to write and author something, and hence learn this significant aspect of their trade.



Number of authors publishing at least *n* papers as a function of *n*.

On Authorship and Plagiarism

Frequent writing provides a simple solution to the perennial problem of who should be the first author of a given paper—since alphabetical ordering will always be acceptable to Dr. Aas, but never to Dr. Zzwerkna. If you are three researchers writing a number of papers and reports, simply alternate (Abdul, Pongase and Schulze, Schulze, Abdul and Pongase, then Pongase, Abdul and Schulze).

Plagiarism must be mentioned here, in connection with authorship because there are lots of colleagues (colleagues?) who do not know that science is a public and collective venture *because* the individual contributions of scientists are explicitly acknowledged in the form of quotes and citations. Not identifying an idea, a paragraph, or any other part of somebody else's work is intellectual *theft.* In the European Middle Ages it was not so and what we now call plagiarism was then a sort of compliment. Anyone now resorting to this middle age practice, exposes himself or herself to another middle age practice, that of being branded forever (then by hot iron, now by being excluded from the scientific community).

On the Mechanics of Writing

The trick here is: imitate (but don't copy, see above)! Take a publication of the type you are aiming at and follow the outline of its content. This is perfectly legitimate, especially if you make this explicit. Thus, J.L. Munro and co-workers, working on Caribbean fishes, followed "to the greatest extent possible [for] the arrangement of [their] chapters . . . version No. 2 of the outline given in the FAO Publication: Preparation of synopses on the biology of species of living aquatic organisms". This they did not because they could not have devised another system, but because this outline was already well-proven. The basic structure of scientific papers should be followed:

Title
Abstract
Introduction
Material/Methods
Results
Discussion
Acknowledgements
References

of which each element is essential, and which neophyte scientific authors are well advised to follow slavishly, lest they get stuck, and see their papers rejected. Books exist about the mechanics of writing. One of the most readable was written by R.A. Day[3].

On The Rewards of Writing

Scientists are paid to do science because it can be shown by economists, sociologists and others, that the application of science to production systems (agriculture, manufacturing, fisheries) generates more than the cost of training, equipping and maintaining scientists. While true in general, this is not necessarily true all the time in every country, for every discipline. I know of many fishery "research" institutions which must cost more to maintain than will ever be recouped through their work, however indirectly. Look at the low productivity of some institutions in Morgan and Hopkins' article (p. 3). How do you rate? (see article p. 10)

Note that scientists are usually not rewarded with cash for the research they do, but with prestige (lots of prestige can be turned into cash via job promotion though). Thus, at least as far as the short term is concerned, scientists do not write for money ("consultants" do this, and their reports often show it!). This needs to be said in view of the tendency, creeping into some countries, to offer cash to the authors of papers published in prestigious journals—which might work in the short term, but at what price?

Saying these things explicitly may sound crude to some, but this contribution is meant to provoke—and to make more colleagues write. •

[1]See e.g., W.E. Knowles Middleton. 1971. The experimenters: a study of the Academia del Cimento. The Johns Hopkins Press, Baltimore.

[2]See Chapter 1 in C.J. Sindermann. 1982. Winning the games scientists play. Plenum Press, New York.

[3]Robert A. Day. 1983. How to write and publish a scientific paper. ISI Press, Philadelphia.