
OPINION

PROBLEMS OF SCIENTIFIC PUBLICATIONS

We publish below an article that appeared in The Guardian Weekly (August 6, 1989), which is the English translation of an article in Le Monde. It is a report (by Jean-Paul Dufour and Franck Nouchi) of an interview with Hubert Curien, the distinguished scientist, crystallographer and administrator who was in charge of many science departments, including the Department of Space, in France, and is now Minister of Research and Technology. He raises many problems that French science faces—the vast power exercised by a few 'powerful' journals that dominate the scientific publication market; the problem of refereeing papers; the problem of scientists and the news media; the involvement of Government in establishing journals; and the process of assessing scientists and scientific laboratories. The article brings out the fact that India is not the only country that is greatly concerned with its scientific journals and the publication of scientific papers. (In the reproduction here, we have added italics for emphasis.)

—Editor

'If God tried to enter CNRS (National Scientific Research Council), he'd flunk' quipped Hubert Curien, himself a scientist and now Minister of Research and Technology. 'He performed an interesting experiment, but nobody has ever succeeded in replicating it. He has explained his work in a voluminous publication, but it was not even in English, and he has published nothing since.'

The witticism has the merit of clearly raising the question of evaluating and publishing scientific research. This question was brought up this year by two sensational controversies—one a claim by a Frenchman, Dr Jacques Benveniste, that water retains the molecular memory of substances even in infinitesimal dilutions, and the second, an assertion by two scientists, an American and a Briton, that they had succeeded in achieving cold nuclear fusion. These controversies illustrate the dominant part that some scientific journals play in disseminating the work of research scientists by confirming their discoveries or exposing their errors.

The researcher who believes he has made an interesting discovery in the secrecy of his laboratory must communicate it to fellow scientists the world over who will then try to reproduce the experiment. If the results are positive, they in turn will publish their conclusions and only then does the original researcher's 'proposition' become a discovery that may at some future date rate a Nobel award. There are two major snags in the procedure: first, the conservative character of one section of the scientific

community, and secondly, *the vast power exercised by a few big journals which dominate the scientific publications market.*

It is possible as of now to draw a few conclusions from the Benveniste case. In an interview he gave *Le Monde*, Hubert Curien raises a number of questions concerning the part played by big international scientific journals and makes an appeal to Europeans to establish their own high quality scientific publications so as to counterbalance the hegemony of the English-speaking world in this area.

Q. What are the lessons you draw from the Benveniste case?

A. Basically, three. First, it seems to me necessary to think about a scientific journals policy. It is extremely important. Secondly, we need to consider more thoroughly the question of evaluating research. And lastly, *it would seem to be desirable to set up a system for regularizing relations between scientists and the media.*

Q. In a book entitled *Les Explorateurs de la santé (Health Explorers)*, to be published in a few weeks, Philippe Lazar, of INSERM (National Health and Medical Research Institute), points to the discretionary powers that big scientific journals have in accepting or rejecting the conclusions of scientific laboratory research. He wonders whether this does not entail the danger of genuinely original work being brushed aside.

A. It's a tricky question. Like many of my fellow scientists, I have been a member of refereeing committees of several major scientific journals. So I have some practical experience in this sort of thing. Very often, three kinds of articles are encountered: bad articles, which can be dismissed without difficulty; good articles, which can easily be passed over too quickly; and a fairly large number of articles whose utility no one is quite sure of, articles about which someone can one day say, 'Yes, I've read the article, it's not even false.' In general there is no objection to publishing such articles for their authors have put in honest work. There then remain those surprise articles, like Benveniste's contribution, which don't fit into any category. Obviously they need to be approached differently.

In this connection I recall the article that Otto Hahn and Friedrich Strassmann published in 1939 in which fission was mentioned for the first time. The writers, both excellent physicists working in Berlin, ended their article with the following remark, which I paraphrase: 'We feel that what we publish here conforms with nothing that is acceptable to our fellow physicists. It is possible we are mistaken. If that is the case, dear colleagues, do not treat us as idiots. Bear with us.'

Q. Are you suggesting that Dr Benveniste should have followed the same course?

A. What I'm saying is that when you publish surprising things, you must keep a very open mind and remind yourself that you may well be mistaken, and appeal to fellow scientists to confirm or invalidate your own findings.

But to get back to scientific journals, the danger is that if we don't look out, the number of such publications which have any authority in the scientific market will diminish, leaving only American journals in the field.

Like everybody else, I'm of course distressed to note that fewer and fewer scientific articles are published in the French language. It's a problem. Another is whether the French and, more generally, the Europeans, will retain control of any major international journal. In my view, this is an even more important matter. Having control of an international journal is a matter of the greatest importance because, *whatever one may say, an American publication will not put the stress on the same things or have the same thrust and mix of articles as a European counterpart. A major Europe-based international journal for each of the principal sectors of science appears to me to be a fundamental objective.*

Q. In this connection, do you think that Britain is part of Europe?

A. There are big British international journals, like *Nature*, for example. Let's stay with continental Europe. Your remark is quite appropriate. British scientists often feel closer to their American than to their European colleagues. It's not clannishness, but a matter of disposition and mindsets.

Q. Is it up to the government to facilitate setting up journals of this sort and decide in what language they should be published?

A. The government should of course facilitate the establishment of such journals. It pays researchers, buys the equipment and provides operating credits. It's therefore quite natural that it should help establish publications.

The question of language is a difficult one. It must be realized that the scientific output of the French-speaking amounts to roughly 8 per cent today. Which is good. But among the other 92 per cent of science producers, there isn't a single researcher who doesn't know English. You have to face the facts.

Some French researchers exhibit a ridiculous coyness in believing that if they are not read or heeded it's because they speak French. *It is probable that the reason they are not read is because they're not 100 per cent convincing.*

What should be banned is a European journal, subsidized by the French government, ruling that it will not accept articles written in French. But such a journal deciding, on the contrary, that it will publish nothing except in French is beyond me.

Q. Are you in a way appealing to research scientists to be more public-spirited?

A. Our researchers must be able to continue publishing as they please in major English-language scientific journals. That's clear. They must be allowed the greatest freedom to publish where they like. Yet publishing in an English-language journal must not be made a sovereign remedy. If Europeans show that they have sufficient strength to publish journals of the same high quality, then it becomes just as interesting to have articles appearing in such publications. *The difficulty with an international journal is not so much launching it as keeping it going.*

Q. Are you going to take concrete measures to this end?

A. We do support scientific publications. It's important to make a distinction among the various kinds of publications. In the first place, there are

journals publishing raw data where scientists should very quickly be able to publish their findings in a form more or less usable by their colleagues but possibly unusable by readers not specializing in the area.

Next, there are journals which carry scientific backgrounders and syntheses for a very well-informed but not necessarily specialist readership.

Lastly, there are journals for the public at large. There are good quality magazines of this sort in France. On the other hand, we don't have enough French-language journals specializing in syntheses. We have launched a highly appreciated journal, *Medecine-Sciences*, in cooperation with the Canadians. Consequently, we would be very receptive and make the necessary effort if we are approached with proposals in areas other than medicine and biology.

Journals publishing primary findings ought to be multilingual. In any case most of the papers will be in English. We have no illusions on that score.

Q. Let's go back to the report, which you describe as surprising. Should special publication procedures be provided for them?

A. That's up to the publishers of the journals. Let me just say that John Maddox, editor of *Nature*, showed a rather insular Machiavellianism in his handling of the Benveniste case.

Q. How do you judge the evaluation procedures currently prevailing in France?

A. There are at least two kinds of evaluation: evaluation of individuals and collective evaluation. Is a given laboratory good on the whole? Has a given research body a good output? Is a given research funding mechanism properly adapted? Should our country become more involved in this or that particular research area?

If we go back to individuals, it's clear the best way to judge a work of research is on actual evidence, that is to say basically on what has been published, but keeping in mind that, given the way scientists live today, published work is not everything. There is also—and this is very important—the way researchers can present their work at congresses or

at conferences to which they have been invited. The latter system is very widespread in the United States, and Europeans have several such projects in the pipeline, one of them currently being examined by the Fondation Européenne de la Science. I'll be happy to see it produce results.

I should like to sound a word of caution about utilizing published work as a yardstick, in particular against the fad (happily already on the way out), of quantifying the evaluation of researchers on the basis of the volume of published material. In this method, the number of articles and the number of references to such articles are totted up and coefficients allotted more or less all round. True, not everything is bad in this system, but misusing it is. It in fact opens up possibilities of making considerable mistakes. One must beware of the effect of fashions and the formation of clans ('I'll quote you, and you quote me'). *The practice of making evaluations on the basis of the mass of published material requires to be treated with a good dose of humour. The unfortunate thing is that the most well-known enthusiasts of the method frequently appear to have no sense of humour at all.*

I know of no really rational system of quantitative evaluation. One has to depend on a kind of consensus in the scientific community. But even here we need to examine things closely. Who should sit on committees to evaluate researchers? Full-time research workers? By definition, they have very limited time in view of the fact they are themselves busy working on their own projects. Assessors whose active research careers are behind them? They will judge with the greatest honesty, but may use observation and (older) scientific models such as they had constructed as frames of references.

We're often told that people sitting on evaluation committees are too old. Well, that's true, but often because the youngest who have been approached have refused to take part. Scientists can't be asked to spend too much of their time judging others. There isn't a perfect evaluation system. Which is why it's absolutely necessary for confidence to reign in the research community.