18 J. Elguero
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Spanish chemistry lives a golden age. Black clouds, however, point in the horizon. So me of them, chemistry shares with other sciences, whereas some others threaten more specifically chemistry.

Academic science is dependent on the national budget (Presupuestos Generales del Estado). We have to accept that in periods of economic difficulties, the state support decreases (although 1 think that this is an error, but...). Now, when the growth of the Spanish economy is higher than that of the rest of Europe and it is universally qualified as good, we must demand the support of the state for science, scientific careers and scientific journals.

Why is chemistry especially threatened? There are at least two reasons. The first one is that "chemistry" has a bad press (e.g. Thalidomide, Seveso, Bhopal, Freons,...). The second one is that our colleagues (physicists, molecular biologists, physicians,...), at least some of them, think that chemistry is a science of the XIXth century. This second problem is the most dangerous. It affects the choice of students and results in the difficulty to publish pure chemical papers in journals such as *Nature*.

Obviously, that chemistry is a discipline of the last century is not my opinion, nor that of any chemist that I know. On the contrary, chemists feel that it is especially alive with frequent discoveries. One aspect of the problems that affect Spanish chemistry is the future of its chemical journals.

An opinion frequently heard when scientists meet is that only a few good journals should remain in Europe. When asked "what will happen with rejected papers?" they usually answer "They should not be published whatsoever".

It seems to me that there is a capital error to confuse "bad/good" with "better/worse". Bad/good are absolute categories, best/worst are only relative. There are not "good" and "bad" papers, only papers "better" than others. Trying to suppress the worst papers reminds me of the well-known children test about the last wagon. If a child is told that the most dangerous part of a train in an accident is the last wagon and asked what he (she) thinks about the solution that consists in removing the last wagon, at about the age of 5 years he (she) already recognizes the stupidity of the proposed solution.

Senior scientists, well above 5 years old, still claim that the last journals should be removed to improve the quality of the publications. They fail to recognize that the "best" papers are only recognized as such by comparison with other papers that are "worse". If you suppress the half-bottom part, the half-upper part will instantly divide in two, the top and the bottom.

Europe needs a three-dimensional network of chemistry journals (X = country, Y = specialty, Z = quality). Papers will go through this "sieve", get caught in some point ($X_iY_iZ_i$) and be published. The quality co-ordinate is usually associated with the "impact index (**ii**)". Although there is no doubt that *Angewandte Chemie* is a better journal than *Chemiker-Zeitung,* it must be clearly understood that **ii** is a statistical value (and a very loose one).

I have had the opportunity to public1y comment the scientific work of our late colleague, Professor Félix Serratosa. Amongst his most quoted papers was the one he published in *Anales de Química.* He, alone, was not able to rise the ii of the soon has-be en *Anales,* but a good work is good independently of the journal where it is published.

As some people probably know, I am what is called a prolific author, with about 800 published papers. Because many of them have had a haphazard life, a reasonable estimation is that I have read about 3.000 comments of my work, which makes me somewhat an expert on the topic of peer review.

The main conc1usion of nearly forty years discussing with referees is that **if you could compare the manuscript which we initially sent with the paper which was finally published, the conclusion would be that they were almost identical,** but for two things:

1.- Referees ask for more work to be carried out (more calculations, larger basis sets, more compounds, more difficult experiments, ...). Obviously (but also, trivially) if you agree and do what it is asked for, the paper improves.

2.- Referees reject the paper based on inadequacy with the journal; usually because the paper does not meet its high standards *(JACS* being the typical example).

Why does this happen? **Because today's papers do not contain errors.** Thus, the author cannot improve them. He can only do more work and/or submit it to another journal.

Once, a well-known Spanish chemist boasted that all his papers had been accepted without modifications in the first journal where he had sent the manuscript. This is to be compared with Roald Hoffmann (Nobel Prize 1981) anecdote about one of his papers having been rejected with the following comment: "The speculations in this paper are the sort of thing that one expects to hear at research seminars, or in social chemical gatherings over a glass of beer; certainly many of them have been made at my own seminar by bright young students. No one else, however, has had the conceit or effrontery to think them worth publishing, let alone in a communication written in the first person. This paper seems to me entirely unsuitable for publication in any reputable scientific journal, let alone JACS" (The Same and Not the Same, 1995).

Since my Spanish colleague is not the equal of Roald Hoffmann, how can one explain his astonishing record? Well, simply he underestimates the value of his papers and sends them to journals below some quality index (impact factor or other).

Imagine the following situation. You are given a ranking of, say, 20 journals and a paper to review. Letting aside the possibility to ask for more work, your role could be to send the paper to journal no. 1 or to journal no. 17. In my opinion, this is the main utility of the peer review system. To suppress journal no. 9 is useless. Science is like a cone of sand (slope 30°), to grow high it needs a larger base. To pretend to build high with a narrow base could result in a catastrophe.

Journals should fight to modify the present ranking and to reach the top positions but without destroying lesser journals. Scientists should compete for funds and prestige with scientists of other disciplines but with fairness and respect, trying to understand rather than to destroy.

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José Elguero
Instituto de Química Médica C.S.I.C.