

Scientific integrity: "Truth" *versus* method

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Although not being myself a geneticist, I was requested by the German journal *GID* to comment on the Hwang story (2), probably for having recently written that due to the dishonest compromises of most experts "medical research has exerted a disastrous influence over other branches of science" (1). Fortunately for me and alas for Science, an even more recent story of gross fraud concerning anti-inflammatory agents (3) exemplifies that the problem goes far beyond genetics (or South Korea...) and is indeed intimately related to the specificity of science as a human activity.

Whatever the philosopher K. Popper may have said, the pursuit of Truth is by no means a characteristic of sciences: a honest judge who sentences (possibly: to death) an accused, a sincere lover who wants to be sure that his/her feelings are reciprocal, are far more obsessed with "truth" than a researcher in meteorology who knows that the rigour of his observations and the sophistication of his mathematical models notwithstanding, his results are marked by a strong degree of uncertainty. And who would deny that a meteorologist behaves as a scientist far more than a judge or a lover?

The specificity of science and, more precisely, its greatness, is not a quest for Truth, but far more simply: *method*, that is this fascinating effort to go out of our innate subjectivity in order to provide others with the means of replicating our observations and results¹. In this quest for sharing with others – which also takes the form of a request for their critical feedback – the system of peer-review, in one form or another, has always been pivotal for the credibility of science: every of us knows its limitations and is able to quote a number of its historical failures, but the sheer reality is that nobody has ever been able to conceive a more reliable alternative.

It is probably not true that the history of sciences includes a number of "unrecognised" geniuses: easily accepted or not, those researchers who made a significant contribution to a science were those who eventually manage to introduce their ideas and make them recognized by their peers. This is neither an easy nor a democratic process, but this has been the normal way of being "a scientist" as long as the ultimate target has been to share with others: after all, being regularly rejected by peers in a kind of methodological failure... In its essence, scientific activity has always been an asceticism, and certainly not a means for personal recognition or social promotion... And in Western history, if Christian religion has most often been an obstacle to sciences, this was not because scientific "truth" was an alternative to God (which it is not): this was because scientific asceticism – as an appeal to go out of one's *ego* and to reach a certain order of objectivation, if not of

¹ This was probably not by chance that a philosopher with such problematic biographical involvements as Feyerabend was so hostile to the very idea of method (5).

objectivity – was a genuine alternative to that rooted in the notion of original sin. Another form of virtue...

In the meantime, however, scientific research has become a road for individual success – which was probably not the main impulse of our great ancestors such as Galileo, Pasteur, Darwin, Freud or Planck. And the pressure to gain success has been such that it has circumvented our control system: if, within the functioning of one given scientific team, co-authors prove to be unable to guarantee the reliability of their own results – as is obviously the case in recent stories (2;3) – who will seriously believe that external peers – devoting a maximum of a few hours to the review of a manuscript – will do better?

Experience, on the contrary, suggests that the situation is even worse. It is a pity to compare the volume of editorials devoted to piously denounce individual failures *versus* the attention editors spend for detecting those fallacies which have a genuine impact on society: after all, the presumed falsifications of Hwang did not trigger any change in the practical management of people, which is not the case with a number of appalling investigations “turning us all into patients” *via* the fallacies of high blood pressure, menopause disorders, high cholesterol, etc. (4) – for which a dime of dozen of papers published in the greatest journals is precisely the background of their success². If, at the scale of one European researcher, a “scientific” journal is not able to recognize as fraudulent an investigation where 250 patients were supposed to be born on the same day, who will rely on huge epidemiological investigations including thousands of patients monitored during years in the farthest parts of developing countries? Whereas counterfeit drugs from East or far-East are becoming an obsession of pharmaceutical leaders, how to interpret the concomitant tendency of the same firms to export their most crucial clinical trials to the same geographical areas – and the depressing leniency of leading medical journals to publish them nonetheless: is it not far more simple to control the chemical content of a pill than to check million of data recorded over years in thousands of unidentified patients? As I said some time ago in a meeting organised by the pharmaceutical industry: the main advantage of *individual* failures is that they give a pretext for virtuous protestations from those whose success is based upon *systemic* failures...

This is a critical moment: if the system of peer-review is not any longer able to guarantee the reliability of scientific research, this means that science has lost its way. The reason for this disaster is too clear: the power of money. In academic institutions, the current dynamics of research is more favourable to the ability of getting budgets – collecting money and spending it – than to scientific imagination or creativity; and the business of edition is fuelled by a continual production of new data of problematic interest, whereas there is no place now to interpret, correct or synthesize previous results³. A striking illustration of this state of affairs is the absurd rule of some medical journals to limit up to 15 days the delay for submitting

² There is something unreal to read an editorial deploring that “the real victims [of the Hwang story] are patients” (2) in a medical journal which published one of the poorest study supporting the safety of hepatitis B vaccine (6) besides reviews on the same topics which are a coarse challenge to the most elementary principles of evidence-based medicine (7).

³ Who remembers, now, that for becoming recognized as a genius in mathematics, E. Galois (1811-1832) had to wait decades after his premature death, just the time to Artin (1898-1962) to grasp the reach of his results and to reformulate them?

correspondences on a paper: if science is *the* target, there is no deadline to put an old result in a new perspective – or more simply to detect an inconsistency in a previous investigation...

Thus, besides precautionary principles such as the declaration of interests, why do not think about a quite simple measure the reach of which could be considerable: a systematic rejection of a work by journals when its budget would have gone beyond a certain limit? Like the systematic rejection of biomedical research which did not comply with the requirement of participants' informed consent, such a measure would mean that sciences have greatest values than a presumed "truth" whatever its cost: i.e. the conviction that too much money is the surest way to ruin the fundamental prerequisite of peer-control...

References

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