Three statements for Research Integrity

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One of the objectives of the workshop

Present case studies of how research integrity guidelines and codes of conduct are pratically implemented.

Statements from David Goodstein

A couple of decades ago, when progress in science was constrained by the limits of imagination and creativity of the participants (an age of purely intellectual competition), peer review was well suited to identify valid science. However, in the present times, science is in an age of competition for scarce resources; peer review, notably by experts, creates a conflict of interest. "*It would take impossibly high ethical standards for referees to fail to use their privileged anonymity to their own advantage, (…) Thus the whole system of peer review is in peril*".

D. Goodstein, 1996, "Conduct and misconduct in science", in "*The Flight from Science and Reason*" P. R. Gross, *et al.* (eds.), New York Academy of Sciences, New York, 1996, pp. 31-38.

Statements from the theoretical chemist Michael Dewar

«*What lessons can be drawn from my career? Undoubtedly the most important is the need to publicize one's work. In the past, this was not necessary. Indeed, attempts to do so were liable to backfire. With the present prevalence of operators, the situation has changed. Now, unless one blows one's own trumpet fortissimo, people tend to assume that one has no reason for blowing it at all*».

"A Semiempirical Life", Profiles Pathways, and Dreams, J. L. Seeman series ed., American Chemical Society, Washington, 1992, p. 180.

Statements from the theoretical chemist Michael Dewar-2

«*A second lesson is the disastrous effect of the growing urge for conformity in chemistry. This trend has been made worse by the tendency to subdivide chemistry into smaller and smaller minidisciplines. The easiest way for a young chemist to succeed nowadays is to produce minor advances in one of these areas, with, of course, full acknowledgement of the contributions of the accepted authorities in it. Any attempt at real innovation is extremely dangerous. I was lucky enough to have already made my reputation before the situation became intolerable. I would not like to be starting on a career in chemistry today*».

A statement from the philosopher Jean Petitot

«*I myself think that science, in order to regain its sovereignty, should seek independence and become a power again – a power similar to medical power. Science needs to confront the problem of responsibility*».

"End of rationality?", in "*Which Values for our Time*", Daniel S. Hamilton, ed., Fundação Calouste Gulbenkian, Lisboa, 2007, pp. 24-35.

Topics to be adressed

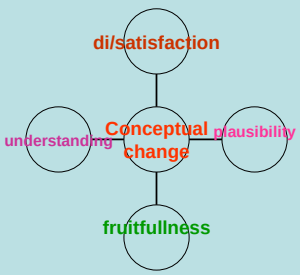
- Weakness of the peer-review system.
- Cognitive change, the visibility of scientific heterodoxy and the difficulties with innovation.
- Scientific responsability; cultural marks in science and in misconduct in research.

Scientific heterodoxy: a case study

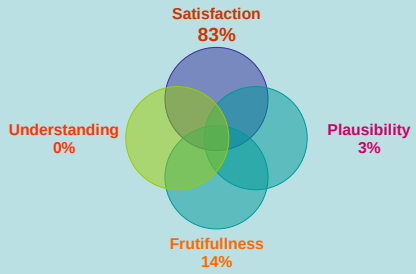
An analysis of the peer review system in chemistry concerning an unidimensional model of chemical reactivity, entitled the intersecting-state model (ISM) considered heterodox matter by several reviewers, was carried out and presented in three books in Portuguese. Over 50 articles were submitted for publication during 12 years (I985-96) and almost 100 referees' reports were collected.

S. J. Formosinho, "Nos Bastidores da Ciência. Resistência dos Cientistas à Inovação Científica", Gradiva, Lisboa, 1988;
"O *Imprimatur* da Ciência. Das Razões dos Homens e da Natureza na Controvérsia Científica", Coimbra Editora, Coimbra, 1994;
"Nos Bastidores da Ciência. Vinte Anos Depois", Imprensa da Universidade de Coimbra, Coimbra, 2007.

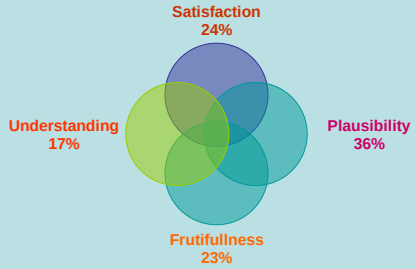
Conditions for cognitive assimilation



articles acceptance



articles rejection



Choice between theories

The choice between theories *à la Kuhn* is **not a dispassionate judgment** and each "paradigm" **perceive differently the "world"**. Although this is reasonably true, the different views are not completely incommensurable since translations among them can be produced. Such task is facilitated if the theories possess **common exemplary solutions**. Therefore improvements can always emerge from the controversy with the referees.

Choice between theories-2

Both the criteria of plausibility and of scientific value tend to enforce conformity, while the value attached to originality encourages dissent. This internal tension is essential in guiding and motivating scientific work. But my view leading with controversial issues for more than 30 years is that the process is too erratic and time consuming, because it tends to prevail from controversy in chemistry the growing urge for conformity, notably if the referees find satisfaction with existing conceptions.

Referee's noncognitive comments

- *Throughout their manuscripts the authors have exhibited a shocking ignorance of the «distance parameters» which are actually appropriate to the construction of the potential energy surfaces for the complex molecules in their examples*
- *If I received a paper like this from an undergraduate, I would be very distressed by its naivete. From a professional it is inexcusable. I have never before been asked by an ACS journal to take something like this seriously*

Explanatory coherence

The coherence of a report can be assessed in terms of the fraction of noncognitive comments. Furthermore, when there is a lack of explanatory coherence between two comments, one of them is assumed as noncognitive.

i) With the support for X theory coming both from a large body of experimental data evidence and quantum-mechanical simulations…

ii) X theory seems too succeesful in explaining the rate of … leaving little room for nonadiabaticity. Is this a fail of the theory or rather just the example of our incomplete understanding how to calculate the nonadiabaticy factors?

Explanatory coherence-2

Based on concepts of mixing entropy, an overall report can be considered sound if the fraction of noncognitive comments is <(1/*e*)=37%.

Non-cognitive comments in referee's reports

articles →	A	A modified version	B	C	D
referee α	29%	55%	17%	57%	9%
referee β	39%	50%	67%	50%	46%
referee χ			50%		

Lack of cognitive coherence

This can be taken as an indication of a more "common human conduct" by scientists under intense competition for scarce resources (academic and research posts, research funds, pages in prestigious journals, financial interests, etc.), not well suited for peer review systems aimed to identify valid science.

Visibility of scientific heterodoxy

The new kind of rules could be established in terms of three questions: i) is the subject a scientific matter? ; ii) does the work contains logical inconsistencies?; in affirmative case those must be stated; iii) if the authors were proved to be correct, does that have [high, medium, low] consequences in the scientific field?

Refereeing on line?

The possibility of refereeing on line for open access journals (*system-2*) could also be contemplated, where comments could be periodically added by the referees and from the authors in reply, and after a certain period of time (ca. two to years) the paper could be accepted on those grounds. This will provide some quality control to the web. *Philica* is an online academic journal that is along those lines by accepting publications on any subject. *Philica* takes a completely revolutionary approach to the publishing and reviewing of academic research. What one needs a mechanism of transition or recognition, after a certain period of time, of the publications of *system-2* to those of *system-1*.

Cultural component in plagiarism

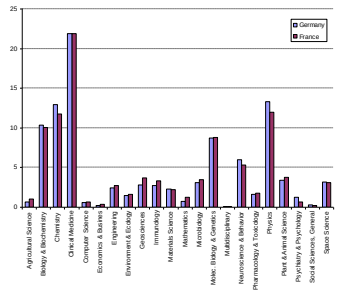
Nature 451, 397-399 (24 January 2008)

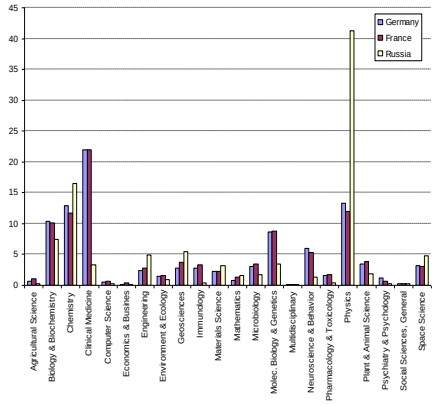
Errami & Garner found in Medline database that the duplication rate extracted from Déjà vu for a country is roughly proportional to the number of manuscripts that the country contributes to Medline. Top eight contributors: United States, Japan, Germany, China, United Kingdom, Italy, France and Canada (ca. 75% all Medline records). China and Japan, have estimated duplication rates that are roughly twice that expected for the number of publications they contribute to Medline. Perhaps the complexity of translation between different scripts, differences in ethics training and cultural norms contribute to elevated duplication rates in these two countries.

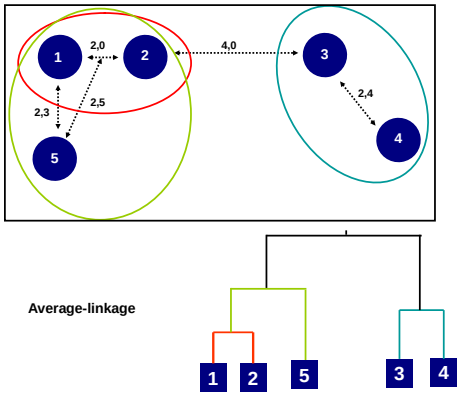
Header Quote of the Polanyi Society

«Tacit assent and intellectual passions, the sharing of an idiom and of a culture heritage, affiliation to like-minded community: such are the impulses which shape our vision of the nature of the things on which we rely for our mastery of the things. No intelligence, however critical or original, can operate outside such a fiduciary framework».
There is a trusting submission that constitutes the "fiduciary" relationship the young have with those teachers, masters, or other authorities.

Michael Polanyi, "Personal Kowledge", Routledge & Kegan Paul, 1958; paperback ed. 1973, p. 266.

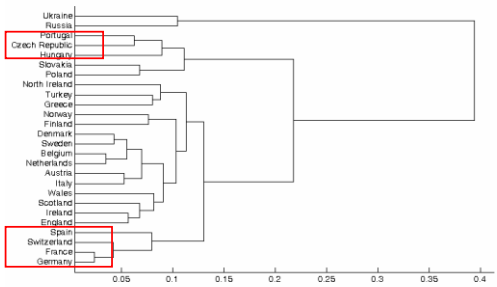






Dendrogram average-linkage

ESI November 2007



Scientific tradition

Scientific tradition has an articulated part and an unarticulated part. The articulated part is composed of the established theories. The unarticulated part refers to things such as the skill of carrying out research, the sensitivity to important problems, the insight or hunches in scientific discovery, and the ethos of the scientific community.

Tacit knowledge

Science is beyond borders, but is not free of tacit knowing which focuses upon the art and craft of research and the task of cultivating insights leading to discovery and also to emotional and moral values. Thus, explicit tradition is global; it is tacit tradition that is local. An art and craft which cannot be specified in detail can be passed on only by example from master to apprentice.

Ethic control and "get to the truth of matters": the master responsability

Thus an ethical control must be exercised through conviviality between the master and the apprentices – not micromanagement that kills creativity –, and with the same aim one should avoid researchers working alone, because that breaks (or weakens) the possibility of the transmission of the unarticulated components of the scientific tradition of the research group and the ethos of research integrity.

Fraud in science

The panorama of frauds in science is emerging in hot areas and the number of retraction of papers is slowly increasing. Although the panorama is not alarming, because it is precisely in the hottest research areas that the reproducibility of results is most strongly tested. Nevertheless, cases such as the one of Jan H. Schön of the *Bell Labs*, dismissed in September 2002 were damaging for the reliability of scientific research. Around ninety papers were published some in prestigious journals such as *Science*, *Nature*, *Physical Review*, *Applied Physics Letters*.

L. Cassuto, "Big trouble in the world of «Big Physics»", in http://www.salon.com/tech/feature/2002/09/16/physics/print.html ; access 16th September 2004.
Geoff Brumfiel, "Bell Labs bottoms up", *Nature*, vol. 454, 21 August 2008, p. 927.

Fraud in science-2

Around 100 laboratory groups working on Schön's results USA and around the world were involved in trying to reproduce such spectacular results on organic molecules that were claimed to be converted into transistors at a molecular level, the high number of Ph.D. thesis of young research which was destroyed. Postdoctoral fellows worry about their prospects for future employment. The world has spent tens of millions of dollars trying to reproduce Schön's key results that were down into the pipe. This is an enormous responsibility facing society and the scientific communities. Concerns rise about the careers of younger physicists that may have been jeopardized, and by the unreliability the whole system now shows.

Fraud in science-2

In fact, Schön's ideas were actually true. It is just that he did not carried out the experiments and in such a way delayed the development of molecular transistors by ca. 5 to 10 years. Many fear that Bell Labs will not recover. Because Schön's results are now suspect, scientists worry that funding for a highly promising area will now dry up. Not only he has delayed the development of this important technology but may have contributed to his decline in Bell's Laboratory. Dangers of working alone and the lack of an ethical control by the master.

Customs and norms

The modern codification of civil law developed out of the ***customs*** of the Middle Ages, expressions of law that developed in particular communities and slowly collected and later written down by local jurists. Customary law is a recognized, but inferior, source of law within jurisdictions of the civil law tradition. Bearing that in mind, what I intend to mention is also that in scientific research the norms of conduct should emerge from a background of **correct customs,** that cannot be entirely neglecteed. In fact, should be strongly pursued through the master-disciples conviviality.

A norm for difficult experiments

Errors can always be committed in good faith in research, but in difficult experiments psychological prejudices can lead to strongly theory-dependence observations and thus contaminate the experimental results. In such circumstances, it appears to be a good practice that such experiments **should also be conducted by researchers that have no specific expectations about the results**.

The level of financial incentives

Scientific tradition has an unarticulated part that has to do with the *ethos* of the scientific community, the pursuit for scientific truth, i.e., a grasp of rationality in nature, or an attachment to veracity. Perhaps the most important driving force of the scientific communities is the search for **scientific prestige**, which when not guided by ethical principles can lead to various forms of untruth in science, including scientific fraud, plagiarism and dissemination of lies.

Science and technology play a very relevant role in the economic development of nations and, as a consequence, several **financial incentives for the production of scientific work of excellence** could trigger publication of scientific papers which are intentionally misleading and distorted by self-interest, notably in areas especially visible or influential.

The level of financial incentives-2

This appears to be the case with some of the policies in course in China; the running slogan was: «*Big grants, no review; small grants, big review*». The *Knowledge Innovation Program* (KIP) launched in 1998 aims to raise China to a level of excellence in research in a short period of time. Salaries three to four times higher than current levels and also payment for each article published for researchers included in such program create strong psychological pressures that can facilitate fraud in research or at least impair research integrity.

New Focus, *Science*, *312*, 9th June, 1465 (2006).
Editorial, "In China, Publish or Perish is becoming the new reality", *Science*, *291*, 23 February, 1477-1479 (2001).

The level of financial incentives-3

The Spanish scheme of promotion of excellence in science (based on a *Tabla de Valoration de Publicaciones*) reflected in salaries on a moderate level can be considered a good practice because appears to be less open to such triggering of scientific untruths and shows some efficacy. The outcome of such a policy was evaluated through *Essential Science Indicators* by comparison with Portuguese scientific productivity that lack any sort of research incentives associated with university salaries. The comparison of the two countries of similar cultures, in terms of the areas of *ranking* in the *Essential Science Indicators* for universities and hospitals, reveals that one is dealing with a Michaelis-Menten type of catalytic effect with a maximum acceleration of 15 times between Spain and Portugal, much higher than the ratio of the populations (ca. a factor of 4).

S. J. Formosinho, "A mentira em ciência", *Revista Portuguesa de Psicanálise*, 28, 223-269 (2008).

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