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Science in the 21st century: social, political, and economic issues

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This report presents a nonidealized vision of 21st century science. It handles some social, political, and economic problems that affect the heart of scientific endeavour and are carrying important consequences for scientists and the rest of society.

The problems analyzed are the current tendency to limit the size of scholarly communications, the funding of research, the rates and page charges of journals, the wars for the intellectual property of the data and results of research, and the replacement of impartial reviewing by anonymous censorship. The scope includes an economic analysis of *PLoS'* finances, the wars *APS versus Wikipedia* and *ACS versus NIH*, and a list of thirty four Nobel Laureates whose awarded work was rejected by peer review.

Several suggestions from HARRY MORROW BROWN, LEE SMOLIN, LINDA COOPER, and the present author for solving the problems are included in the report. The work finishes with a brief section on the reasons to be optimists about the future of science.

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Introduction

This report handles some social, political, and economic problems that differentiate 21st century science from the science done during past centuries.

Those problems affect the heart of scientific endeavour and are carrying important consequences for scientists and the rest of society. Several suggestions for solving them are included in the report –see also *Launch of new canonical science journal*[1] for several suggestions already in use–.

The problems analyzed are the current tendency to limit the size of scholarly communications, the funding of research, the rates and page charges of journals, the wars for the intellectual property of the data and results of research, and the replacement of impartial reviewing by anonymous censorship.

«Salami science», a new cancer for scholarly communication

Nowadays, the *Journal Citation Reports* covers more than 7500 journals in approximately 200 disciplines,[2] and still its coverage is incomplete! This wide array of highly specialized journals is carrying important consequences for scientists and librarians, as stated by Marye Anne Fox:[3]

> *First, short communications published in highly specialized journals will often be invisible to those outside that specialty. These may be precisely those people who would benefit most by thinking more broadly of the place their own work occupies within the spectrum of the study of nature. Second, the highly specialized journals are expensive and fail to attract a sufficiently numerous readership for most local libraries to justify subscription. When budget cuts come, wise librarians select these journals for termination, with the consequent effect being that some faculty do not have access to the range of journals in which they themselves publish. These faculty may, in fact, have never seen their own published work apart from reprints they have purchased. Third, the inaccessibility of these specialized journals is a particular problem in developing nations, where library budgets are even more constricted than in a typical university library in an industrialized nation. Finally, the rapid proliferation of short articles published in multiple venues makes it virtually impossible for even the most dedicated scientist to keep up. This failure to embrace up-to- the-minute mastery of one's own field by a thorough reading of the relevant literature exerts a highly negative effect, I believe, on the standards and work ethic of our current student apprentices.*

Marye Anne Fox –member of the Editorial Advisory Board for *Chemical Reviews*– points that the rapid proliferation in the number of short articles and communications appears to be driven both by perceived standards of quantitative accountability for career advancement at universities and by what can only be described as greed by some of commercial publishers. She also adds:[3]

> *Sadly, however, the prevailing tendency toward "thin-sliced science", the so-called "salami science", seems to retain its stronghold over publishing customs. So long as Communications to the Editor or the strictly length-defined short articles of the general scientific journals such as Science or Nature are perceived as representing the highest standards for quality within the scientific community, it seems likely that similar proliferation will continue.*

And the tendency continues nowadays. At the time of writing this report *Nature Publishing Group* is engaging on launching the new journal *Nature Chemistry* with a more restrictive size format to bound articles to generally three–eight printed journal pages in length. Its first issue is scheduled for publication in mid-March 2009, with next size restrictions for its *Articles* format,

> *The main text (not including abstract, Methods, references and figure legends) is limited to 3,000 words. The maximum title length is 15 words. The abstract — which should be 100–150 words long and contain no references —* [...] *The Methods section in the main text is limited to 800 words* [...] *References are limited to 50* [...] *Depending on the word count, Articles may have up to 6 display items (figures and/or tables). In addition, a limited number of uncaptioned molecular structure graphics and numbered mathematical equations may be included if necessary. To enable typesetting of papers, the number of display items should be commensurate with the word length — those with word counts less than 2,000 should have no more than 4 figures/tables.*

A maximum of 3,000 words is also the limit imposed to its *Perspective* format. The report you are reading now exceeds that figure in more than the double! This means that any *Perspective* of about the same size as this one, independently of its quality and interest, would not be approved for publication in that journal just because the report is considered too long.

But is this new «salami science» representing the highest standards? Response is negative, as reported by Linda Cooper in a recent issue of *Nature*. Current article size limitations are deteriorating the communication of science:[4]

> *To ensure clear communication, most journals encourage authors to write for a broad audience. But most published papers still compress too much information into uncomfortably short articles, leading to convoluted sentences, specialized terminology and a proliferation of abbreviations. Errors in grammatical style result in impenetrable and ambiguous texts that seriously undermine the scientific literature. This need not be the case.*

And points to a paradoxical situation with the swell in e-publications:[4]

> *Electronic publishing could offer authors limitless space to explain their ideas and discuss their new findings. Surprisingly, though, online manuscripts are often bound by the same space constraints as print manuscripts.*

As pointed before, the restrictions on size do not obey to scientific motivations but appear to be driven both by perceived standards of quantitative accountability for career advancement at universities and by what can only be described as greed by some of commercial publishers:

- For instance, if two similar authors apply for the same academic vacancy and first author's curriculum includes ten papers of about twenty pages long each but second's includes twenty papers of the half-size, then the winner will be the latter author –this is so because only the number of publications counts for career and not the total number of pages of research generated–.

- Also when a publisher artificially limits article formats to a total of ten pages long, for instance, the larger papers will be splinted into smaller pieces and the publisher will earn double, triple or more money from readers and authors. Standard rates for authors and readers will be discussed in the next section.

A cure to the metastasis of «*salami science*» relies on the intensive usage of recent electronic publishing tools. Scholarly works may be distributed in PDF, XML, or newest formats –such as CanonML, which is still under active development–.

The PDF format has become the *de facto* standard for printable documents on the web. The cost of producing the PDF version of a forty pages long work is virtually indistinguishable from that corresponding to a ten pages work. This contrasts with the almost linear increase associated to more traditional, non-electronic methods, of production.

Further advantages of electronic formats include (i) the saving of physical space –a single Compact Disk may store thousands of reports like you are reading now–, (ii) the instantaneous access to published contents –eliminating the amount of time to arrive in the departmental library or readers office, especially if they come from across the Atlantic–, (iii) the inexpensive usage of color –e.g. in figures–, and (iv) the browse zooming, user annotation, hypertext linking, and full-text search capabilities do not available in print.

Funding and other economic issues

In the past golden age, scientists were driven by their own curiosity and intuition to study topics. This was the age which gave us most, if not all, scientific revolutions; together with the names of who are considered main contributors. An alphabetically ordered representative sample is reproduced next,

- Electromagnetism and optics: Charles Coulomb, Christian Huygens, Georg Simon Ohm, Hans Christian Ørsted, Heinrich Lenz, Heinrich Rudolf Hertz, James Clerk Maxwell, Michael Faraday, and Thomas Young.
- Evolutionary biology: Alfred Russel Wallace, Charles Robert Darwin, Ronald Aylmer Fisher, and Sergei Sergeevich Chetverikov.
- Mechanics: Galileo Galilei, Isaac Newton, Joseph Louis Lagrange, and William Rowan Hamilton.
- Quantum theory: Erwin Schrödinger, George Uhlenbeck, John von Neumann, Louis de Broglie, Max Born, Max Planck, Niels Bohr, Pascual Jordan, Paul Dirac, Samuel Goudsmit, Werner Heisenberg, and Wolfgang Pauli.
- Relativity: Albert Einstein, David Hilbert, Hendrik Antoon Lorentz, and Jules Henri Poincaré.
- Stoichiometry: Antoine-Laurent de Lavoisier.
- Structure of matter: Amedeo Avogadro, Dmitri Ivanovich Mendeleev, Ernest Rutherford, Gilbert Newton Lewis, Heike Kamerlingh Onnes, Henry Moseley, John Dalton, Joseph John Thomson, Linus Pauling, and Lothar Meyer.
- Thermodynamics and statistical mechanics: Constantin Carathéodory, Henri-Louis Le Chatelier, Hermann von Helmholtz, James Joule, Joseph Fourier, Josiah Willard Gibbs, Julius Robert von Mayer, Lars Onsager, Lev Davidovich Landau, Lord Kelvin, Ludwig Boltzmann, Peter Debye, Robert Boyle, Robert Brown, Rudolf Clausius, Sadi Carnot, and Walther Nernst.

Evidently, the sample is incomplete and lacks some important achievements and authors. My aim was not to give a complete summary on history of science, but only a representative sample for comparison with the lack of results provided by the current *career-driven* science.

Also its classification scheme is open to debate. For instance, ALBERT EINSTEIN would be better associated to quantum theory, for which EINSTEIN received the Nobel Prize;[5] however, I decided to associate him to relativity theory according to popular folklore. In any case, this is a minor issue that does not dismiss the importance and rigor of the rest of this section.

Nowadays, the driving force of scientific research is very different. Scientists obtain grants from governmental science agencies and related funding organizations. Research topics are not freely chosen by scientists but strategically planned by funding bodies. The curiosity and intuition of experts are substituted by politic and economic agendas.

The winner of the Nobel Prize for Physics PHILIP WARREN ANDERSON opines this has changed the way young researchers see science in the 21st century:

> *In the early part of the postwar period career was science-driven, motivated mostly by absorption with the great enterprise of discovery, and by genuine curiosity as to how nature operates. By the last decade of the century far too many, especially of the young people, were seeing science as a competitive interpersonal game, in which the winner was not the one who was objectively right as the nature of scientific reality, but the one who was successful at getting grants, publishing in Physical Review Letters, and being noticed in the news pages of Nature, Science, or Physics Today...*

And what is the position for genuine creative and independent scientists in this hostile environment? LEE SMOLIN dissects the current situation in his article *Why No 'New Einstein'?* published in *Physics Today*. He notices current disadvantages for those scientists:[6]

> *Those who follow large well-supported research programs have lots of powerful senior scientists to promote their careers. Those who invent their own research programs usually lack such support and hence are often undervalued and underappreciated.*
>
> *People with the uncanny ability to ask new questions or recognize unexamined assumptions, or who are able to take ideas from one field and apply them to another, are often at a disadvantage when the goal is to hire the best person in a given well-established area.*
>
> *In the present system, scientists feel lots of pressure to follow established research programs led by powerful senior scientists. Those who choose to follow their own programs understand that their career prospects will be harmed. That there are still those with the courage to go their own way is underappreciated.*
>
> *It is easy to write many papers when you continue to apply well-understood techniques. People who develop their own ideas have to work harder for each result, because they are simultaneously developing new ideas and the techniques to explore them. Hence they often publish fewer papers, and their papers are cited less frequently than those that contribute to something hundreds of people are doing.*

And traces social and political causes for the actual absence of revolutionary advances in theoretical physics. This lack of revolutions of importance comparable to those in the golden era of science has been picturesquely titled as «*the absence of a new EINSTEIN*». It may be interesting to compare the academic environment on the early part of last century –when ALBERT EINSTEIN published his revolutions– with the current for a hypothetical «*new EINSTEIN*».

ALBERT EINSTEIN submitted his *Annus Mirabilis* papers of 1905 lacking any official affiliation to academia. As Nobel Winner for physics BRIAN DAVID JOSEPHSON once remarked, EINSTEIN would have been today impeded to disseminating his papers in electronic archives for physicists like *ArXiv*, when moderators had discovered that EINSTEIN lacked academic affiliation.

Either if we believe or not JOSEPHSON's remarks, the truth is that the own *ArXiv* endorsement policy introduces **bureaucratic** favoritism when states: «*Users with recognized academic affiliations may be exempt from the endorsement process*».

It is evident that the *Annus Mirabilis* papers were accepted for publication, but not everyone knows they were published without a formal peer review process. That is something virtually impossible today for a «*new EINSTEIN*»!

Moreover, it is likely that the *Annus Mirabilis* papers would be rejected today by reviewers. EINSTEIN lived in an academic environment much more open to new ideas. In journals in those days, the burden of proof was generally on the opponents rather than the proponents of new ideas –see below the section *When peer review reinforces orthodoxy rather than quality* for a fascinating list of more than thirty Nobel awarded papers rejected for publication–.

But these are not the only differences between the academic environment on the early part of last century and the current. About 1905 scientific research was science-driven; this means that EINSTEIN freely chose the research topics with independence of what everyone else did. Without doubt, an essential ingredient of the recipe for scientist's success is a favourable academic environment. But, as reported above by LEE SMOLIN, academic scientists have lost it.

FRANK JENNINGS TIPLER has expressed similar complaints about funding bureaucracy and the hostility directed towards certain innovative research programs:

> *Laymen rarely appreciate how centralized scientific research has become in the last fifty years. Funding for my own area of physics, general relativity, is located in one and only one division of one and only one bureau of the federal government, the National Science Foundation. If the referees for a grant proposal submitted to this division of that bureau happen not to like your work, your grant proposal will not be funded—period.*

Besides a brave description of some problems of current science LEE SMOLIN provided a number of *recommendations* would help to solve them.[6] SMOLIN considers that (i) young scientists would be hired and promoted with independence of their contribution to seniors research programs and that (ii) equal promotion of alternative points of view about unsolved questions may be guarantied, and (iii) calls for a reward system favouring scientists working in more difficult open questions and for (iv) the creation of specific fellowships for creative thinkers. So far as I know those recommendations have not been adopted in any part of the world, and even if they were tomorrow they would not solve other economic issues discussed next.

One of those issues is the high price of top academic journals. A high price impedes the effective access to information as well for scientists, students, and people living in wealthy countries. The worldwide economic crisis, with epicentre in *USA*, has deteriorated the situation.

The subscription to few specialized journals makes sense for research units specialised in a single narrow topic. Still University libraries are cancelling many subscriptions to academic journals, with the paradoxically situation that authors do not have access[3] to specialised journals in which themselves publish! Scientists and groups working in multidisciplinary topics –such as complexity or unified theories– would subscribe to a wider array of generic and specialised journals, which is economically impossible.

The situation is not better when purchasing single works in electronic format. When searching academic literature, one usually finds eight pages long research articles being priced so high as $25 –additional taxes excluded– in mainstream journals published by societies like the *American Physical Society*.

This is not a problem exclusive to physicists because you may find about the same prizing per article for *American Chemical Society* journals like *Molecular Pharmaceutics*, *Crystal Growth & Design*, *Inorganic Chemistry*, *Chemical Review*, *Analytical Chemistry*, *ACS Chemical biology*, etc. The prizing is standard and independent of the article size. As a consequence, chemists, physicists, engineers, and material scientists interested in recent work[7] published in the journal *Nano Letters* would purchase it by $25, which is over $12 per published page!

$25 is also the price for online articles in the *International Journal of Modern Physics D* published by *World Scientific*. Five pages long articles in *New Astronomy*, published by *Elsevier B.V.*, are priced something higher: $31.50. And $32 is the prize for articles in *General Relativity and Gravitation*: a journal published by *Springer Netherlands*.

Among the more expensive instances, we can find *Nature research highlights*[8] of less than one page long being priced so high as $32. Few authors, research groups, and institutions can support these rates. Scientific excellence is being substituted by economic discrimination.

The open access initiatives –see, for instance, the *Public Library of Science* (*PLoS*)[9]– try to solve the economic difficulties for accessing information when provide us free access to journal contents. However, its open access model requires an article **fee** –ranging from the $1300 of *PLoS ONE* to the $2850 of *PLoS Biology* and *PLoS Medicine*– to authors and research sponsors for **each** article they publish.

Moreover, *PLoS* demands additional financial support via individual and institutional memberships. The rates for individual memberships range from the $25 corresponding to *Student* level to above $1000 for *Innovators*. Evidently, the figures are higher for institutions.

PLoS is also funding its open access model from sponsorship. According to analysis done by John Ewing –Executive Director of the *American Mathematical Society*– *PLoS* is actually accepting sponsorships of $25,000–$100,000 from pharmaceutical companies.

Nowadays, *PLoS* offers a complete or partial fee waiver for authors who do not have funds to cover publication fees. But it looks more like a marketing strategy for spreading their model of academic publication than an reliable policy for the long-term. According to analysis of *PLoS* finances performed in July of 2008, done by third-parties, the model has been kept afloat financially by some $17.3 million in philanthropic grants, since its launch in 2002.

Other publishers have adopted different variants of open access models. The *American Physical Society* provides open access to *Physical Review A–E* and *PRL* owing to per article charges of $975 and $1300 respectively. However, the scientific society applies an article charge of $700 plus a length dependent additional charge of $80 per 125 lines for *Physical Review Special Topics — Physics Education Research*; this journal is published only electronically.

The actual role that economic factors play on the selection of what will be published is evident when the society of physicists advises to authors with «*Accepted manuscripts will not be forwarded to production until the publication charges are paid in full*». Requests for reduction of charges may be considered but «*The aid will come from limited funds donated by the sponsoring organizations, American Association of Physics Teachers and the APS Forum on Education*». That is, if your work is fine but you lack the needed money it will be not published — period.

Another set of difficulties for authors becomes from additional journal charges. Authors publishing in the mainstream *Physical Review Letters* are expected to pay a publication charge of $750 –reduced to $275 for a *Comment* or *Reply*–. For papers which were submitted for the editorial review process in electronic formats preferred by the *American Physical Society*, charges are lower –nowadays $605 for a *Letter* and $215 for a *Comment* or *Reply*–.

Both color-figure and reprint charges were not included in above and would be paid separately. Again the costs vary with journals and editorial decisions. For *The Journal of Chemical Physics* –published by the *American Institute of Physics*–, authors and research sponsors may pay about $650 for the first color figure and $325 for each additional color figure may be considered non-essential by editors. The charge is $800 for a single color figure and $425 for each additional color figure in the print version of the article for journals published by the *American Physical Society*.

Intellectual property and other wars for the rights

In the current model of scientific publication, authors may fill form agreements of copyright transfer to the publisher, which becomes copyright holder of the published work. Copyright assures authors that authorized copies will not mangle or misattribute their work. But most copyright holders want to restrict access of the data and research results to paying customers.

A recent war for the control of the rights of scholarly data started when the *American Physical Society* withdrew its offer to publish two studies in *Physical Review Letters* because the authors had asked for a rights agreement compatible with both the online *Wikipedia* and more specialized encyclopedias as *Quantiki*, *Qwiki*, and *SklogWiki*.[10]

The *American Physical Society* asks scientists to transfer their copyright to the society before they can publish in any of their journals. Once scientists do this, they are no longer the owner of the work! The current policy for *Physical Review Letters* prevents authors from posting their **own** figures on *Wikipedia*, for example. The authors of the rescinded papers and thirty eight other physicists are calling for the *American Physical Society* to change its policy.

Another war for the rights, that received an extensive covering in media,[11,12] put face to face to the *American Chemical Society* and the *National Institutes of Health* for the control of *PubChem*, a new molecular database.

Chemists Want NIH to Curtail Database was the title chosen by *Science*, but the news received a more accurate treatment in *Nature*:[12]

> *Many chemists might not know it, but the organization that represents them in the United States is fighting to limit their free access to chemical information. The American Chemical Society says that a new publicly funded database of molecules threatens its own fee-based Chemical Abstract Service (CAS), and it is lobbying politicians to restrict the free version. But it is having trouble convincing members that this is in their interests.*

PubChem database connects chemical information with biomedical research and clinical information. The war started when the *American Chemical Society* took several actions against *PubChem*, calling for the *National Institutes of Health* to eliminate or restrict its project, in «*order to avoid unnecessary duplication and competition with private chemical databases*».[13]

A fundamental principle behinds *PubChem* is that medical research information developed with public funds must be made freely and publicly available for the good of advancing medical research to cure diseases. This seems reasonable, does not? However, the *American Chemical Society* holds a different position and officially expressed its concerns to that the *National Institutes of Health* «*will be providing, for "free," a taxpayer-subsidized substitute for the CAS* [Chemical Abstracts Service] *Registry*».[13]

The conflict continued with public accusations of bad faith becoming from both sides, and with leading chemists as Richard J. Roberts –Nobel Laureate in Physiology or Medicine– resigning his membership in the society after more than twenty years as a member. He also wrote:

> *My only interpretation of the recent actions by the ACS* [American Chemical Society] *Board and management is that it is no longer trying to be a scientific society striving towards the goals of its Congressional charter, which is to represent the best interests of the scientists who form its membership. Rather it seems to be a commercial enterprise whose principle objective is to accumulate money.*

In my opinion, the wars will terminate when **creators** of the works hold the rights. The publisher is, in the end, a bare medium for the distribution of the work generated by others. Of course, publishers would hold the rights for issues where they spent money and time, including (i) logos, (ii) special publication formats like scientific markup language they developed, and (iii) additional services of formatting, indexing, metadata, and others, they had generated; but publishers would not hold the rights of the text, data, and figures generated by others.

When peer review reinforces orthodoxy rather than quality

According to recent systematic investigation of the peer review system, its assumptions about fairness and objectivity are rarely tested.[14] The scientific method is about the testing and verification of hypotheses; therefore, it is surprising that main hypotheses of academic communication were not tested more often.

Tom Jefferson –who led the investigation– states that peer review needs to be more open and accountable. Not only did peer review pander to egos and give researchers licence to knife each other in the back with impunity, he said, but it was also «*completely useless at detecting research fraud*».

Notorious cases of research fraud in the top journals *Science*, *Physical Review*, and *Nature* were detected in latter times –a popular case was the late Schön scandal, considered by some the biggest fraud in physics in the last 50 years–.

Fortunately fraudulent scientists represent a **minority** and are sanctioned when the fraud is detected –e.g. Jan Hendrik Schön has seen his doctoral degree revoked, cannot apply for *Deutsche Forschungsgemeinschaft* [*German Research Foundation*] funds, and received other sanctions for an eight-year period–. Moreover, the fraudulent published papers were withdrawn.

However, the current peer review system lacks mechanisms to control bias and abuse by anonymous reviewers. Tom Abate reports unfair situations when writes:

> *Particularly troublesome for the younger investigator is the Oz-behind-the-curtain effect: Reviewers often work anonymously, giving them greater opportunity to act arbitrarily. The reviewer usually has no comparable curtain to stand behind.*

Most investigators who worked in revolutionary theories have denunciated both difficulties to publish their work and abuse from reviewers. As Nobel Laureate Jack Steinberger has rightfully observed, «*new ideas are not completely easy to accept, sometimes even by the brightest and most open of people*».

Among the more notorious instances of resistance to scientific discovery, Juan Miguel Campanario has collected many cases in which Nobel Prize winners were involved. I reproduce some in which Nobel class papers were formally rejected by reviewers:

- The 1948 Nobel Prize in Chemistry was awarded to Arne Wilhelm Kaurin Tiselius «*for his research on electrophoresis and adsorption analysis, especially for his discoveries concerning the complex nature of the serum proteins*». His paper was first rejected in a biochemical publication.

- The 1949 Nobel Prize in Physics was awarded to Hideki Yukawa «*for his prediction of the existence of mesons on the basis of theoretical work on nuclear forces*». His paper was rejected by *Nature*. *Physical Review* also rejected a similar manuscript in 1937.

- Hans Adolf Krebs received a letter from *Nature* declining in a polite way to publish the first report on the citric acid cycle, the discovery for which Krebs would eventually share the 1953 Nobel Prize in Physiology or Medicine. *Nature* argued that they had sufficient letters to fill the correspondence columns for seven or eight weeks.

- In the year 1958 Pavel Alekseyevich Cherenkov, Ilya Mikhailovich Frank and Igor Yevgenyevich Tamm shared the Nobel Prize in Physics «*for the discovery and interpretation of the Cherenkov effect*». However, their original manuscript entitled *Visible radiation produced by electrons moving in a medium with velocities exceeding that of the light* was turned down by *Nature*, «*whose editors did not take the work seriously*».

- A half of the 1959 Nobel Prize in Physiology or Medicine was awarded to SEVERO OCHOA «*for his discovery of the mechanisms in the biological synthesis of ribonucleic acid and deoxiribonucleic acid*». OCHOA had to defeat very adverse criticism by a reviewer.

- The other half of the 1959 Nobel Prize in Physiology or Medicine was awarded to ARTHUR KORNBERG. The referees that reviewed in 1957 two manuscripts submitted –by BESSMAN and colleagues and by LEHMAN and colleagues, both being senior author ARTHUR KORNBERG– to the *Journal of Biological Chemistry*, rejected the manuscripts.

- The manuscript based on highly significant findings concerning antibody response made by FRANK MACFARLANE BURNET was rejected by the British scientific journal to which it was originally submitted. This work stated the ensuing implications of the discovery. Not swayed by negative response, BURNET pursued the topic, collected more data, and published his observations in an unrefereed monograph entitled *The production of antibodies*. The discovery reported in the second edition of the monograph was awarded with a share of the 1960 Nobel Prize in Physiology or Medicine.

- A share of the 1963 Nobel Prize in Physics was awarded to EUGENE PAUL WIGNER «*for his contributions to the theory of the atomic nucleus and the elementary particles, particularly, through the discovery and application of fundamental symmetry principles*». One of his highly cited papers on symmetries dealing on the unitary representations of the inhomogeneous LORENTZ group was nevertheless rejected when first submitted for publication. As WIGNER pointedly remarked upon this unjustified rejection «*not all articles originally rejected by a journal prove to be valueless*».

- A manuscript authored by MURRAY GELL-MANN and dealing with «*strangeness*» in elementary particle physics was similarly rejected by reviewers of the *Physical Review* in 1953. Moreover, the editors objected to the use of the main concept that GELL-MANN coined: «*curious particles*». He had to change to «*new unstable particles*» after «*strange particles*» was also rejected. The reviewers also objected his explanation of differences between neutral boson and neutral anti-boson. It took many efforts for GELL-MANN to convince reviewers that he was right. The work reported in this article was awarded with the Nobel Prize in Physics in 1969. Nowadays GELL-MANN is considered one of the most important physicists of 20th century still alive.

- The original publication in which BARUCH SAMUEL BLUMBERG related Australia antigen to the etiologic agent of "viral" hepatitis did not elicit wide acceptance. The reviewers rejected a more extensive paper by BLUMBERG and co-workers that spoke about the same topic, on the grounds that the authors were proposing another «*candidate virus*» and there were already many of these around. This was the discovery for which BLUMBERG shared in 1976 the Nobel Prize in Physiology or Medicine.

- WILLIAM NUNN LIPSCOMB received the 1976 Nobel Prize in Chemistry for his studies on the structure of boranes. In an interview, LIPSCOMB recalled how the *Journal of the American Chemical Society* rejected the first manuscript in which he used the concept of pseudorotacion to explain the structure of a boron hydride. Another manuscript in which he showed that *p*-dithiin was V-shaped was also rejected by the *Journal of Organic Chemistry*.

- The development by Herbert Charles Brown of the techniques that permitted the usage of boron-containing compounds as crucial reagents in organic synthesis was awarded with a share of the 1979 Nobel Prize in Chemistry to him. One of the referees who reviewed Brown's key paper stated that «*there are nothing new about the reaction...*» and «*moreover, the reactions produce organoboranes for which there are no known-applications. Consequently rejection is recommended*».

- The 1983 Nobel Prize in Chemistry was awarded to Henry Taube for «*his work on the mechanisms of electron transfer reactions, especially in metal complexes*». The original article was submitted for publication in *Chemical Reviews*, it was reviewed and rejected.

- A share of the 1986 Nobel Prize in Chemistry was awarded to John Charles Polanyi. According to the Nobel Press Release «*The method which he has developed can be considered as a first step towards the present more sophisticated, but also more complicated, laser-based methods for the study of chemical reaction dynamics*». His paper was rejected by *Physical Review Letters*.

- One half of the 1986 Nobel Prize in Physics was awarded to Gerd Binnig and Heinrich Rohrer for developing the scanning tunnelling microscope. In their Nobel Lecture, they have spoken about being often told that they were addressing something that would «*not have worked in principle*». Actually, their first attempt to publish a letter describing the scanning tunnel microscope failed.

- In 1986 Stanley Cohen shared the Nobel Prize in Physiology or Medicine for his works on growth factors. However, one of the first articles on this topic was rejected by the first journal to which it was submitted. There was a reviewer who insisted that the laboratory mice used in the experiment were nothing but ill.

- Again *Nature* rejected a Nobel class article written by Hartmut Michel, who shared the 1988 Nobel Prize in Chemistry.

- The discovery for which Tomas Robert Cech received one half of the 1989 Nobel Prize in Chemistry was in conflict with some well-established ideas in Biology. Nonetheless, in his Nobel Lecture, Cech vividly described how contemporary enzymologists feeled outraged with the use of words «*catalysis*» and «*enzyme-like*» to describe the function of RNA he had recently discovered.

- Twice the *Journal of Chemical Physics* rejected in 1965 the key paper that led to the 1991 Nobel Prize in Chemistry awarded to Richard Robert Ernst.

- Michael Smith received one half of the 1993 Nobel Prize in Chemistry «*for his fundamental contributions to the establishment of oligonucleiotide-based, site-directed mutageneis and its development of protein studies*». His paper was rejected when first submitted for publication. Smith interpreted the rejection as a cause of «*a subjective judgment by the editor of a journal to which many more manuscripts are submitted than could be published*».

- The other half of the 1993 Nobel Prize in Chemistry was awarded to Kary Banks Mullis for his discovery concerning the polymerase chain reaction, which turned out to become the most widespread method for analysing DNA. Both *Nature* and *Science* rejected one of the first reports.

- *Analytical Biochemistry* rejected «*due to insufficient advancement*» a paper coauthored by Martin Rodbell describing a highly sensitive adenylate cyclase assay. This was the work that earned one half of the 1994 Nobel Prize in Physiology or Medicine to him.
- The 1996 Nobel Prize in Physics was awarded to David Morris Lee, Douglas Dean Osheroff, and Robert Coleman Richardson for the discovery of superfluid Helium. Their key paper was rejected by the reviewers of the journal *Physical Review Letters*. One reviewer argued that the system «*cannot do what the authors are suggesting it does*».
- The *Journal of Biological Chemistry* also declined to publish the Nobel Prize winning work of Paul Delos Boyer. Skepticism remained even after Boyer first published his theories in 1971. His work, describing the molecular motor that creates cellular energy and the biochemical pump that transport such energy across membranes in cells, was awarded with one quarter of the 1997 Nobel Prize in Chemistry.
- One of the two reviewers of *Nature* who read Robert Francis Furchgott's article describing the «*endothelium-dependent relaxation*» expressed doubt about the validity of the experimental procedures and conclusions. The findings reported in this work turned out to be discovery that earned its author a share of one third of the 1998 Nobel Prize in Physiology or Medicine.
- Leading academic journals refused to publish Louis J. Ignarro's discovery that NO is crucial to the life process, the discovery that was awarded with other third of the 1998 Nobel Prize in Physiology or Medicine.
- The 2000 Nobel Prize in Physics was awarded to Herbert Kroemer «*for developing semiconductor heterostructures used in high-speed and opto-electronics*». His paper was rejected by the journal *Applied Physics Letters*.

Unlike some readers would believe, the above list is not exhaustive. Other authors whose highly significant work was rejected include Rosalyn Yalow, who described how her Nobel-prize-winning paper was received by the journals:

> *In 1955 we submitted the paper to Science.... The paper was held there for eight months before it was reviewed. It was finally rejected. We submitted it to the Journal of Clinical Investigations, which also rejected it.*

And also include Günter Blobel, who in a news conference given just after he was awarded the Nobel Prize in Medicine, vividly stated that the main problem one encounters in research is «*when your grants and papers are rejected because some stupid reviewer rejected them for dogmatic adherence to old ideas*».

Naturally, reviewers rejections extend to other great achievements of last century science. It comes to the memory here Mitchell Jay Feigenbaum, who described the reception that his revolutionary papers on chaos received,

> *Both papers were rejected, the first after a half-year delay. By then, in 1977, over a thousand copies of the first preprint had been shipped. This has been my full experience. Papers on established subjects are immediately accepted. Every novel paper of mine, without exception, has been rejected by the refereeing process. The reader can easily gather that I regard this entire process as a false guardian and wastefully dishonest.*

Junior researchers challenging established views are not the only authors who suffer peer review abuse. The whole process may be also unfair for senior investigators with lot of credentials, including Nobel Laureates!

John Bardeen –twice a winner of a Nobel Prize in Physics– had difficulties publishing a theory in low-temperature solid state physics –the area of one of his Prizes– that went against the established view.

And during a talk given on 7 December 1991, in Japan, the Nobel Laureate Julian Seymour Schwinger described the current abuse by anonymous reviewers in the next terms:

> *The pressure for conformity is enormous. I have experienced it in editors' rejection of submitted papers, based on venomous criticism of anonymous referees. The replacement of impartial reviewing by censorship will be the death of science.*

I find difficult to accept that editors of many eminent journals still permit this kind of unfair practices by reviewers. We may agree with Harry Morrow Brown that peer review should not be anonymous.[15]

This agreement does not necessarily imply that *any* anonymous review may be automatically discharged, neither implies that referees may be obligated to sign their reviews; there is many referees acting in good faith who do not sign their reviews –some of them prefer remain anonymous by modesty–.

I propose that authors of works under review may have the choice to reject any biased review was not signed by the reviewer. As a rule of thumb, a reviewer who is unable to sign its own review may be considered like one author who could not support its work.

Reasons to be optimists

As showed in this report, the idyllic conception of science may be substituted by a more realistic conception of a science with some problems. However, there exist **not** reasons to be pessimist. Scientists continue providing us captivating responses to open questions about Nature –including ourselves–. Moreover, the main problems have been identified and several solutions proposed. Some of the proposals are already on use.[1]

At the end of last century, it was often asked what the future of science may be. For some, such as Stephen William Hawking in his *Brief History of Time*, we are close to the end, the moment when we will be able to read the mind of God. In contrast, the Nobel laureate Ilya Prigogine believes that we are actually at the beginning of a new scientific era, the birth of a science that is no longer limited to idealized and simplified situations but reflects the complexity of the real world.[16] I could not agree more with him.[17]

Through this report, I have tried to eliminate «*idealized and simplified situations*» also from the metascience, presenting to my readers a more realistic conception of the scientific endeavour.

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