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How to Reject any Scientific Manuscript

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Abstract – After a short overview of arguments pro and contra peer reviews, examples of gross misjudgement are compiled, followed by an attempt to identify some frequent, recurrent patterns of unjustified rejection of scientific manuscripts. A few specific questions are studied in more detail: the claim for still more precise and comprehensive definitions, the right way of handling "parallel theories", and the frequent misuse of the term "pseudoscience". Finally, practical rules to improve refereeing, and "basic rights of authors" are proposed, together with a word of encouragement for future authors.

Keywords: Peer reviews – cases of misjudgement – patterns of unjustified rejection – misuse of the term "pseudoscience" – proposals to improve refereeing

1. The Controversy about Peer Reviews

The controversy about peer reviews has a long tradition. Here it can be presupposed that the reader is familiar with the basic facts and frequent arguments. The present text will focus only on the refereeing of scientific papers submitted for publication in a journal, or for presentation at a congress, workshop, etc.; the assessment of research proposals is beyond our scope.

After a brief summary of arguments pro and contra peer reviewing, examples of unjustified or unfair rejection of papers are compiled, where it will be tried, as far as possible, to identify recurrent patterns. It is the main purpose of this study to encourage authors who may be victims of unfair treatment, and to supply diagnostic tools to those editors and publishers who want to guarantee the quality of refereeing within their domain of responsibility.

To avoid any misunderstanding: thousands of referees worldwide are doing a fine job, year after year, and on a voluntary (unpaid) basis. The following unfriendly examples are taken from reliable printed sources and from the immediate experience of scientists personally known to the author (marginally, a few items are reported from the author's own experience).

The arguments pro are simple: peer reviews have to
- exclude papers of poor quality and mere nonsense,
- safeguard the usual scientific standards,
- return manuscripts to the author(s) for correction of minor flaws, and
- prevent an overloading of information channels and a flood useless material.

The list of arguments contra is by far longer; the principal objections to peer review are that

- Refereeing is slow and delays publication.
- The claimed validity of the procedure is not guaranteed.
- Acceptance or rejection is biased; it is influenced by the referees' personal interests, preferences, and worldview.
- Innovative concepts are at risk of being suppressed; texts which stick to the current opinion have a superior chance of being accepted.
- The chance to publish is placed under the control of small minorities ("elites").
- A reviewer can obtain an unfair advantage by becoming informed about novel concepts, research strategies, or empirical findings earlier than other researchers.

The well-known term "Matthew effect" (Robert K. Merton) characterizes the recurrent pattern that those authors who already have published a handsome number of articles will have a greater chance to get a new paper accepted than an unknown author would have, even if the quality is comparable ("they who have will still be given more"). A similar bias is evoked by the authors' affiliation. (Also, prizes are more likely to be awarded to senior scientists, even if the work has been done by younger researchers.)

Two extreme positions have been articulated. Based upon his empirical research, Armstrong (1982) formulated what he called "the author's formula", a set of rules that authors should use to increase the likelihood and speed of acceptance of their manuscripts. "Authors should (1) not pick an important problem, (2) not challenge existing beliefs, (3) not obtain surprising results, (4) not use simple methods, (5) not provide full disclosure, and (6) not write clearly." Taschner (2007) even opposes "the illusion that papers written by researchers are really read by those colleagues who keep the power of important decisions. In my view, the situation – at least in some disciplines – is much more miserable: often no longer anything is read, but, in the best case, good friends among the gatekeepers are asked by phone or email whether the author really is suitable."

2. Examples of Gross Misjudgement

"It is a well known fact that many ideas and theories are not attributed to the first inventors, because originally they were rejected, then forgotten, and later on they were to be discovered anew by other people. In these cases, it is not the pioneers who are quoted and rewarded for their accomplishment, but later inventors and imitators." (Fischer, 2007: 5) The Indian astrophysicist Chandrasekhar with his theory of stellar development is an example. After having encountered heavy resistance from established scientists, the young researcher did not publish his theory and turned to other branches of physics. More than twenty years later the theory was developed again by others (Wali, 1996: chap. 6). Chandrasekhar was finally awarded the physics Nobel prize in 1983 for other achievements.

Turning now from natural science to humanities, we must inevitably regard what became known as "Sokal's hoax". In 1996, Alan Sokal, professor of physics at New York University, concocted a deliberately nonsensical and parodic text entitled "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity". This was sent to a journal dedicated to cultural studies and run by a renowned university - and printed even without any doubtful question from the editor's side. Sokal himself ascribed his quick success to the fact that his text had been in perfect conformity with the editor's ideological preconception, and he revealed his conscious hoax in a subsequent text published by another journal (Sokal, 1996; for an overview, including further developments, see also Sokal & Bricmont, 1998).

In 1921 the chemist William C. Bray discovered a chemical reaction with periodic oscillations, but this simply was not believed. In 1951 an article, written by P. B. Belousov, on a similar observation was rejected; the editor declared that the reported results were simply impossible. Finally, the discovery was accepted about the year 1970 – now it is generally known as the Belousov-Zhabotinski reaction – but only after a suitable theory had been developed (see Bauer, 1992: 23, with further examples).

The medicine Nobel prize 2005 was awarded to Robin Warren and Barry Marshall for their (re-)discovery of the bacterium which finally was named *helicobacter pylori*; in contrast to earlier chance observers, Warren and Marshal had elucidated its medical significance. But initially they encountered a rather hesitating and reluctant acceptance, and when they wanted to publish their findings, their earliest articles were rejected as incredible; even accepted papers were significantly delayed (Marshall, 2006: 245). Nowadays infection with *H. pylori* is held responsible for the majority of all stomach ulcers.

For the realm of economics, Gans and Shepherd (1994) compiled material on "classic articles by leading economists" which originally had been rejected.

In an experimental study, Peters and Ceci (1982) selected 12 already-published research articles by investigators from prestigious American psychology departments. After slightly altering the 12 articles and changing the authors' names and their affiliations, Peters and Ceci re-submitted the manuscripts to the journals which had originally refereed and published them 18 to 32 months earlier. Only three of the re-submissions were detected, and eight of the nine other were rejected. The two experimenters argue that the reviewers' bias against less prestigious institutions accounted for the eight rejections, because the authors' affiliations had been changed from renowned universities to fictitious obscure places. (For further material on "reconstructive studies" see Fröhlich (2003); further examples and analyses can be found in Fischer (2004, 2007), Hook (2002), and Horobin (1990)).

Now it's time to present the *record-holder* (with the greatest number of unjustified rejections of one paper, to the author's best knowledge). The biologist Lynn Margulis developed a fundamentally new "theory of the origin of eukaryotic cells" (cells which divide by classical mitosis). As she remembers in conversations with a biographer, nevertheless "the paper was rejected by about fifteen scientific journals" (Brockman, 1995: 135). Currently, her contribution is recognized as a landmark and a key to the understanding of the genesis of organelles. Finally, the article was accepted and printed, meanwhile under the name Lynn Sagan (Sagan, 1967) – sometimes marrying can run faster than publishing.

3. Elementary Tricks to Justify Rejection

To try to make some sense of this, the "simpler" tricks will be listed here, and the more sophisticated ones, which require some discussion, will follow in the next section. The following list is certainly incomplete.

1. How sloppy reading can be helpful: Henry H. Bauer (2002: 270) reports: "One of my early papers in electrochemistry was turned down because I suggested that a certain parameter had a certain value and the referee refused to believe that possible; he overlooked that I had cited the value from the published work of a highly respected researcher, work that never before had been contradicted." In another case, a referee remarked: "Sometimes 'Delta' is spelt in

capital letters, sometimes not." It had been explained in the onset that "Delta" and "delta" stood for Δ and δ - in a typescript before the era of modern word processing.

2. Blank ignorance or bold claim: An author was undeservedly named the creator of a "new mathematics". The true story is that he only had proposed to apply *graph grammars* for a specific purpose. Indeed, graph grammars have existed since 1969; with about five mouse clicks the referee would have found this fact, together with a special bibliography (Drewes, 2007) comprehending about 1000 entries (already in 2005), and displaying also the various fields of application.

3. Something is missing in the beginning: It is criticized that some old stuff, e.g., a particular episode from the history of the field, has not been addressed.

4. An article can always be criticized for some extensions, conclusions, or applications missing in the end. Reviewing a paper on the teaching methodology (didactics) of a specific discipline, a referee criticized that no new curriculum for that subject had been expounded.

5. It is objected that references are incomplete. This can apply both to things that are well known as well as to very obscure references. One may guess that one of the missing titles is related to the referee, and another one to his best friend. And, evidently, the principle of "uncited classics" is not generally known – in quoting the famous $E = mc^2$ it is not required to quote Einstein's original paper. As an illustration, the mentioning of the Matthew effect (Section 1) is left undocumented here.

6. Taboos in unexpected places: There are not only taboo words, but also *taboo authors* (as critically reported by Clauser (2002: 71-74)), whom one should better not quote "in order to get papers accepted"; names are not disclosed here in order not to reinforce the ugly chorus. Once an argument for rejection was the fact that a book quoted stemmed from an unknown writer and was published in a little town – in the referee's view it could not be excluded that the book was distributed by a printer's shop naming itself "publisher".

7. *Ultima ratio regum:* This Latin phrase – "the kings' ultimate argument" – was engraved on cannon some centuries ago. Indeed, if a referee has only few feeble arguments for rejection, or wants to hide a fundamental opposition against the contents – then the ultimate weapon, the big cannon comes in: "This paper has flaws with respect to English style and grammar." Sometimes this may even happen when the text had been edited by a native speaker. And why not return it for corrections?

4. Advanced Tricks and Boundary Cases

4.1. The Struggle about Definitions

No matter whether only established terms are used or some new terms are adequately introduced in a manuscript, there will always be a chance for criticism: one or another *definition* is claimed to be either missing, not clear enough, deviating from the usual habit, or "a minimal definition which is not sufficiently elaborate". So some remarks on definitions in scientific texts may be appropriate.

It is generally accepted (and meanwhile rather trite) that in the very onset the notions must be clear and that there must be a consensus about the meaning of terms. But, in our present context, this issue can be seen from a different perspective; there are at least some specific arguments against an undifferentiated use of such objections:

- In any definition, the *definiens* again contains terms for which a definition could be requested, and so on *ad infinitum;* every system of notions includes *fundamental notions* which can not be reduced to underlying simpler notions.
- In many cases, it is possible to find a divergent definition and to highlight this as the generally compulsory one.
- An observation can be regularly made in discussions after a lecture presented in a conference or workshop: someone demands a definition just to attack; if no concrete arguments against the statements made in the lecture are available, then at least a fog grenade can be thrown to spread an atmosphere of scepticism in the audience.
- Authors who consciously use a divergent terminology will jeopardize the acceptance of their proposals or findings.

4.2. Fads, Nostalgia, and Wrong Perspective

Even in mathematics, where nobody would suppose, a strange fad may emerge, time and again, for a short-lived existence, and this can be used by referees, for example "this paper does not sufficiently consider catastrophe theory (or: fractal theory, chaos theory, fuzzy logic, etc.)"; nevertheless, about a year later, the next buzzword can be "in".

Just the highly innovative proposals are at risk of being viewed under an obsolete - and hence wrong - perspective. Repeatedly we find a tendency to stick to traditional concepts, simply based on emotion, such that we can speak about "nostalgia in science" (this holds equally for authors and reviewers). In applications of mathematics, the recurrent pattern of "nostalgia mathematics" can be illustrated by the inclination to avoid complex number (e.g., by a separate handling of real and imaginary parts – more sophisticated examples and the inconvenience of this way of proceeding are outside the present scope of this paper). At any rate, one should be cautious of comments stating that "the same could be done by simpler mathematics". Further samples of a wrong perspective and its outcome may be

- the demand for details of a special microphysical effect in an article on macroscopic phenomena (when there is not any known connection between both), and
- the insistence on special episodes from the history of science in a case where the real topic is the correct interpretation of recent empirical findings.

4.3. Parallel Theories and the Argument of the Simpler Description

An example of the fruitful coexistence of two equivalent theories is given by the Heisenberg representation and the Schrödinger representation in the early years of quantum theory. Fortunately both formulations were created nearly simultaneously, such that neither of them could take over the role of a dominating theory. In our modern view they are unitarily equivalent, i.e., there is a one-to-one translation from one to the other.

In the present context, we have to scrutinize the frequently used argument that something could be easily done without the proposed new concepts, and that the empirical material could as well be handled in the traditional style. Leaving aside the rare case of two equivalent and simultaneous theories, we propose the notion of a *parallel theory* to denote a new scientific framework which describes just the same empirically verified facts as are contained in already-existing theories. The notion of a "parallel theory" (rather than "alternative theory" or something like that) is deliberately chosen in order to avoid any bias in favour of one of the candidates.

A candidate for a new parallel theory must possess the following qualifying features:
- *General requirement:* The usual properties of a scientific theory must be guaranteed, e.g., internal consistency.
- *Continuity of terms:* As far as possible, established terms should be used, and furthermore, this use should maintain their traditional meaning.
- *Empirical correctness:* The theory must be compatible with the empirical data.
- *Explanatory power:* There must be at least one class of empirical facts which can be explained only through the new theory, or in which the new theory replaces a totally unsatisfying and preliminary older explanation or interpretation with a clearer one.

Now we are in the position to formulate a criterion under which a proposed parallel theory will be legitimate and meaningful. The decisive criterion is its *explanatory surplus,* which may be restricted to a specific class of empirical phenomena. Under this condition it is counterproductive to argue that the older theory suffices. It must be kept in mind that the new theory may turn out as significant for a broader class of phenomena which are not being discussed at the moment or which will be detected in the future; those who reject a novel theory may be rejecting a pathway to new discoveries.

Authors carry the burden of proof for the alleged explanatory surplus of their proposals.

Closely related herewith is the argument that a "simpler description" would be possible. Rather often, such objections sail under the flag of Ockham's razor, a methodological principle due to the mediaeval philosopher William of Ockham. But in reality Ockham's principle is very frequently quoted in a sense incompatible with the original text and intention: Ockham mainly opposed an unjustified creation of new terms in philosophy. In modern times, two principal versions are circulating:
- The *principle of parsimony* comes close to the original version by demanding cautious discretion before introducing new terms.
- The *principle of simplicity* aims at explanations, reasons, theories, etc., which should be as simple as possible.

It is clear that modern science requires a functional, sufficiently differentiated system of terms. But, on the other hand, the referees should stop the authors from an unbridled creation of new terms, which may be motivated, e.g., by mere ignorance of the usual standard or by vanity ("show" vocabulary).

The principle of simplicity requires a more detailed analysis (Bunge, 1963; Gernert, 2007). Very briefly, some principal aspects can be summarized as follows:
- A precise measure of simplicity cannot be generally defined; rather, it would depend on the underlying means of description and on the context of the analysis.
- Simplicity cannot be the exclusive guiding principle for research, but it stands in competition with other partial goals, like exactness; if simplicity were the only criterion, then thousands of nonlinear formulas in science would be in danger of being replaced by "simpler" linearized versions.

- There is no objective and formalized procedure for deciding which of two competing explanations for the same empirical facts is to be regarded as the "simpler".
- A subjective bias can never be excluded: what is compatible with somebody's own pre-existing worldview, will be considered simple, clear, logical, and evident, whereas what is contradicting that worldview will quickly be rejected as an unnecessarily complex explanation or a senseless new hypothesis.

4.4. Pseudoscience – An Intricate Notion and its Ambivalent Use

"The demarcation between science and pseudoscience is not merely a problem of armchair philosophy: it is of vital social and political relevance." (Lakatos, 1998: 20) Indeed, this problem of demarcation is one of the two hard problems within the philosophy of science, paralleled only by the problem of formulating a generally valid theory by induction from empirical data. So here no global overview can be given (nor is it planned to join the debate on fundamental questions); rather, the problem will be viewed mainly under the aspects of refereeing. Also some lesser-known facets will be addressed.

The terms "pseudoscience" and "pseudoscientific" are invariably defamatory and derogatory (Hansson, 1996: 169). So Hanson makes no attempt to formulate explicit definitions of these notions. Also three comprehensive encyclopedias, otherwise very reliable, circumvent this issue by simply omitting these keywords. "The supposed experts on this, the philosophers of science, in their honest moments will admit that no satisfactory definition of pseudoscience exists." (Bauer, 2001: ix)

As Hansson (1996: 173) explains, it is not careless experiments that are thus denigrated, but deviant doctrines. Reported results which are too contradictory with regard to established knowledge are rejected, even if they were obtained methodically. (Bauer, 1992: 58, 136)

A demarcation is not possible by majority opinion, and even Popper's falsifiability criterion is not helpful here (Lakatos, 1998). There is no general agreement about scientific method. A great many phenomena or interpretations, controversially debated in earlier times, were accepted later on (meteorites, continental drift, ball lightning, reverse transcriptase; for further examples see Bauer (1992: 23-24)); some frontier sciences have been eventually converted into reliable textbook science.

Turning now to the referees' practical activities, first of all we must state that there are masses of "intellectual trash" and "junk science" (Bauer, 2001) – an argument for the necessity of refereeing. Everybody who works in related institutions knows the trouble with unsolicited visitors and written material. There are characteristic patterns which justify a quick rejection, e.g.:

- proposals for "squaring the circle" (which show that the true problem has not been understood),
- attempts to prove Fermat's Last Theorem (generally a first step into a proof technique, the futility in the general case of which was demonstrated some decades ago),
- a gross denial of the heliocentric system, or of Earth's shape as a spheroid, etc.

In such cases a rejection must be permitted without a need of further justification. A practical tool to simplify and to speed up working is a list published by John Baez (2007) – even if it looks funny at a first sight (like a quiz where scores are given), it really sums up a lot of practical experience.

In this context, two brief warning remarks may be adequate. Some amount of animosity, and the readiness to rashly use pejorative terms, are revealed by the fact that even in mathematics, where nobody would suppose, two (then) newly developing branches of mathematics were placed into the neighbourhood of pseudoscience by mathematicians: catastrophe theory and chaos theory. (Sussmann, 1978; Steffen, 1994) The second warning concerns a "non-criterion". It is not necessarily a symptom of wrong science when a group of scientists builds up an organizational and communication structure of its own. Clauser (2002) gives an impressive account on the situation within the quantum physics community (about the early 1980s), characterized by him as "forbidden thinking". A minority of physicists advocating an experimental inquiry of quantum theory felt urged to publish an underground newspaper (under the inconspicuous title "Epistemological Letters"), the circulation of which was limited to the members of a "quantum-subculture". Eventually, the banned experimental tests – the pioneering work was the experiment by Alain Aspect and his team – led to the modern state of the art with the undisputed role of quantum communication and quantum cryptography.

To sum up: apart from the clear extreme cases, a rejection should be supported by concrete reasons and should not be based upon or orbiting around one single, frequently misused word.

5. Some Practical Conclusions

This tentative summary compiles some practical advice, both for editors and for authors. First of all: *refereeing is necessary,* in spite of all critical reservations; the positive reasons given in Section 1 cannot be questioned. Specifically, each reviewer should take care that no "private terminology" will be printed where the established one would suffice (new terms are admissible only for real innovations) and that parallel theories will be accepted only if they are justified by a demonstrable explanatory surplus (Section 4.3).

There is no guarantee for the detection of fakes and frauds. No referee can repeat experiments, nor check lengthy mathematical derivations; nevertheless, nonsense like the claimed non-existence of gravitation (Sokal's hoax, Section 2) should be intercepted. Some of the weak spots expounded here can be partially alleviated. A first, well-known advice is to keep the authors (and their affiliations) in anonymity; also in some cases, the reviewer can identify the author through favourite topics, phrases, quotations, and the like.

The following rules should be observed in order to safeguard "basic rights of authors":

1. If a rejection is based upon the poor quality of a paper, or its nonconformity with the scope of the journal, then the paper should be returned rather quickly (within a few weeks).
2. The same holds if there are only some errors which can be easily corrected (e.g., wrong years for historic events), or if the argument refers to English grammar and style, with a chance to submit a revised version.
3. *Pacta sunt servanda:* If a manuscript has been returned with detailed hints for corrections, and if the revised version meets these requirements, then the editor should have no chance to take refuge behind formal regulations ("We never accept a third revised version" – it really happened).
4. In cases of a referee's manifest error – beyond any question of subjectivity, worldview, and interpretation (see the examples in Section 3, numbers 1 and 2) – the author should have the right to address the editor or publisher, with the expectation that he will receive an answer in due time.

This paper may end with an encouragement to future authors. The present author knows a small number of concrete cases in which a really valuable and innovative paper collects dust in a cellar or attic because its author, after rejections, shrinks back, through sheer frustration, from submitting it elsewhere. The material compiled here may entail sufficient motivation to submit a rejected manuscript (after corrections) once more at another place.

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