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Peer review versus editorial review and their role in innovative science

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Abstract Peer review is a widely accepted instrument for raising the quality of science. Peer review limits the enormous unstructured influx of information and the sheer amount of dubious data, which in its absence would plunge science into chaos. In particular, peer review offers the benefit of eliminating papers that suffer from poor craftsmanship or methodological shortcomings, especially in the experimental sciences. However, we believe that peer review is not always appropriate for the evaluation of controversial hypothetical science. We argue that the process of peer

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review can be prone to bias towards ideas that affirm the prior convictions of reviewers and against innovation and radical new ideas. Innovative hypotheses are thus highly vulnerable to being "filtered out" or made to accord with conventional wisdom by the peer review process. Consequently, having introduced peer review, the Elsevier journal *Medical Hypotheses* may be unable to continue its tradition as a radical journal allowing discussion of improbable or unconventional ideas. Hence we conclude by asking the publisher to consider re-introducing the system of editorial review to *Medical Hypotheses*.

Keywords Peer review · Academic freedom · Editorial policy · Periodicals as topic · Innovation · Scientific hypotheses · David F. Horrobin

Introduction

Taking different opinions seriously is one of the characteristic features of a cultivated society. Tolerating opinions of minorities is challenging to the majority. The trouble for the majority is that there remains a small chance that the minority's perspective may be true and the majority's may be wrong, although the chances are much greater that the minority is wrong. How do we deal with different opinions in science?

Historically, the majority has tended to suppress divergent views. Before Galileo Galilei published his book *Dialogue Concerning the Two Chief World Systems* in 1632 [1], his admirer, Pope Urban VIII, had encouraged him to discuss Copernicus's idea of heliocentrism as a hypothesis only. Galilei, however, could not resist taking a firm stand on this question. His views were interpreted as an attack on Aristotelian geocentrism and a defense of the Copernican theory. Under *public pressure*, his former supporter Pope Urban VIII ordered Galilei to Rome to stand trial on suspicion of heresy one year later. According to the sentence of the Inquisition, Galilei had to "abjure, curse, and detest" his views on the motionless sun in the center of the universe and a moving earth in order to avert his execution on the stake [2]. And yet it moves.

At least Galilei's life was saved in the trial. Others before and after him were not so lucky. The scientist and philosopher Giordano Bruno (1548–1600), for example, found himself on the stake for defending Copernican heliocentrism. Up to the twentieth century, scientists feared for their lives for supporting controversial ideas. In 1936, the physicist and philosopher Moritz Schlick was shot on the way to a lecture on the stairways of the University of Vienna by a former student who, probably among other reasons, opposed his philosophical view on atheistic positivism. Today's methods of silencing a colleague's tongue, however, are somewhat more subtle.

Hypothesis

Our hypothesis is that peer review is the modern mechanism by which challenging minorities' views are suppressed. While valuable for much scientific discourse, peer

review is often incompatible with innovative hypothetical science. According to this view, innovative ideas tend to be filtered or conventionalized by the peer review process. Based on a comparison of the systems of peer review and editorial review, we will investigate the consequences that the introduction of peer review had on the Elsevier journal *Medical Hypotheses*, which traditionally refrained from applying peer review and applied editorial review instead. We believe it is likely that this journal cannot continue its tradition as a forum for radical ideas because the newly introduced peer review as a "quality control system" can be biased against novel, innovative, and unconventional theories.

We define innovative science as science that goes far beyond the current state of understanding. Innovative papers and proposals in this sense often appear provocative because they cut the ground out from under the feet of established explanations. Innovative studies hence tend to be controversial and are likely to provoke opposition from some in the scientific community. In terms of its reception by the community, innovative science is often identified with shoddy or even fraudulent works, or at least ridiculous and non-scientific work, because, like bad science, it appears to defy what is accepted as "true."

Discussion

The system of peer review

In the process of peer review, two, three, or more experts from the field are asked to evaluate the soundness of a scientific paper. Its main intention is to ensure or even raise the quality of the submission in question. Nobody doubts the benefits that peer review offers. The process of refereeing limits the enormous unstructured influx of information and the sheer amount of dubious data, which, in its absence, the scientific community could never deal with. Further, peer review offers the benefit of eliminating papers that suffer from poor craftsmanship or methodological shortcomings, especially in the experimental sciences. Without peer review, therefore, scientific publishing would be chaotic and unmanageable. Objectivity in the process of peer review, however, is probably overestimated. In the long term, the objective evaluation of a scientific idea or result can only stem from what has been termed "peer usage" [3]. In this process, scientific information is proliferated by being incorporated (via citation) into subsequent works. "Bad science" (science that "does not work") will not be repeated and hence is neglected or refuted, whereas "evolution" will favor "good science" only. Peer usage, however, is a slow process (although modern interactive publishing is enabling peer comments on a faster timescale); hence peer review remains the instrument of choice for ensuring that the quality of state-of-the-art applied science is "enhanced" from the get go.

Although doubts about whether peer review partly fails to reach its original goal—i.e., improving the quality of scientific publications by rigorous quality control—have been published [4–6], most of us will agree that it does succeed in many cases. Most of us have, at some point, learned lessons from a referee's report and made significant improvements to a manuscript. Even famous physicists Albert

Einstein and Nathan Rosen unexpectedly had their paper on gravitational waves returned for major revisions by the journal *The Physical Review* based on a referee's comment. Einstein withdrew the paper and submitted it to the *Journal of the Franklin Institute* [7], where it appeared after Einstein radically altered the conclusions in the proofs after realizing that his initial assumptions had been wrong [8]. Here, peer review prevented the publication of a wrong hypothesis.

However, there are examples of peer review's failure at quality control as well. One is the sensationalistic proclamation of microorganisms growing at over 250 °C in 1983 [9]. Subsequent work [10] showed that the data reported as bacteria from the vicinity of hot vents were problematic with respect to its reproducibility. When attempting to repeat the experiments, identical results could be obtained not only when using the same equipment, sampling, and analytical procedures but also when conducting the experiments without the original samples. The conclusion was that the extraordinary forms of life were in fact artifacts. The point is reiterated by the inability of any group to isolate real microorganisms from sources identical or similar to the one examined by the group that made the original claim.

Recent studies by world-class pharmaceutical companies that have failed to repeat high-profile peer reviewed research suggest that an astonishing number of such studies cannot be reproduced [11, 12]. Other analyses on specific high-profile findings, from the role of sirtuins in longevity [13] to whether bacteria can have arsenic in their DNA [14] (against which concern was expressed publicly [15–24]), suggest that peer review is far from a perfect system for eliminating even obvious methodological flaws when there is strong pressure to publish.

For most science, the problems of peer review are outweighed by the benefits. However, controversial and hypothetical studies face specific problems in the course of peer review, which we believe makes peer review problematic or even unsuited to their evaluation.

It is widely observed that reviewers are often "biased toward papers that affirm their prior convictions" ([25] as cited in [26]) and "biased against innovation" [25]—"unless it is their innovation. Innovation from others may be a threat because it diminishes the importance of the scientist's own work" [27]. Even if an unconventional, innovative paper is finally accepted after thorough peer review and one or more revisions are made by the authors, they often feel forced to alter the originally submitted version against their own will just to fulfill the referees' conditions for acceptance. In evidencing this point, a recent study has found that the outcomes of peer review in terms of scientific quality are highly sensitive to the effects of even *small* numbers of biased reviewers [28, 29]. In a kind of anticipatory obedience and self-censorship, some papers are even written in advance in a way that increases their likelihood of being "approved" by the typical reviewer; thus, peer review directly encourages conservatism and conformity [30, 31]. In all honesty, who amongst us can say that they have never used a certain phrase or a specific style simply to avert a reviewer's criticisms? A brilliant parody on this bizarre specter was published in—guess where—*Medical Hypotheses* [32].

A prominent historic example of how the peers' opinion and *public pressure* influenced the appearance of a scientific work is Charles Darwin's *On the Origin of Species* [33]. It has been suggested that while the first edition was crisp,

straightforward, and revolutionary, the following editions were weakened by Darwin making concessions to his peers, the church, and the *public* [34]. Luckily, the scientific community still has access to the original text at each stage of its revision, so the development of the work has remained transparent. Today, a regular scientific paper, in contrast, only appears in print as the final, accepted version that the reviewers agree with; the uninfluenced—and perhaps sometimes "better"—original manuscript disappears forever. In Darwin's case, the objections were at least in part religiously motivated, which one may argue is no longer relevant to today's science. The mechanisms for the self-censorship then and now, however, appear comparable at least.

Among the most popular arguments of referees for rejecting a paper are the following: the scientific work may be irrelevant to the respective scientific community or the readership of the journal; the topic is too specific or too general for the scope of the journal; not novel; not novel enough; written in bad and simple English (please consult a native speaker!); too long and wordy; too short, incomplete and imprecise; of little or no scientific significance and value; or finally—and this is the one that all scientists dread—simply wrong. Many of these are reasonable causes for complaint, and providing the referee is specific about length, grammar, subject, obvious experimental flaws, etc., their comments can help to improve the paper. Novelty and correctness, however, are judgment calls, and referees are observed to err quite often on the side of caution, claiming that a result is implausible if they cannot easily accept it or not novel if they thought that someone might have thought of it before. For many papers these are straightforward and valid judgments to make. But if the paper is postulating a new hypothesis that says, in essence, "for much of your professional career, you were wrong," how likely is a referee to take a calm and entirely objective view? The hypothesis may stand in stark contrast to what he or she has learned or experienced before. Does this necessarily mean that the study, the conclusions, or the paper itself is not correct? It could, but this need not be the case.

In extreme cases, authors who fear having their papers rejected in the course of peer review for these reasons tend to be dishonest with their results in one way or another (which, of course, is no excuse for scientific fraud). It may be the case that only results that support a certain hypothesis are included in the paper because the authors may fear the referees' judgment if "disconfirming" data are presented and discussed. As a counterculture to those who beautify their results, the editors of *Nature Cell Biology* recently implored, "Let's celebrate real data—wrinkles, warts and all. We want to publish gritty documentary movies, not digitally beautified yarns!" [35]. However, the root of the problem is not the authors' intent to do dishonest science, but a well-founded fear of intellectual suppression by peer reviewers, who will only approve what is unequivocally correct and who will not recommend for publication anything that is ambiguous, controversial, or tentative. In conclusion, the concept of peer review, according to our hypothesis, can introduce bias into innovative science even via the authors themselves who are pressured to suppress their own innovation.

It must be noted that peer review is also a relatively recent imposition on science. From the beginning to the mid-twentieth century (a "golden age" that saw the

invention of relativity, quantum mechanics, modern genetics, and biochemistry, most of modern chemistry and physics and the basis of pharmacology, among other things), peer review was not significantly involved in the publication of science. Journals utilized editorial review as standard. Therefore it would appear that peer review is neither necessary nor sufficient for the production of significantly innovative science [29], and may even be antagonistic towards it.

Extension of use of peer review

Peer review is also being applied to other aspects of science, such as evaluation for grants, awards, and positions. Here, review is often organized as a dual stage process. First, the application is sent out to anonymous peer reviewers. At this stage, peer reviewers are often asked to rate different applications or proposals with a numerical score, in order to make them fully comparable. Second, committees (usually with undisclosed membership) decide on the worthiness of the proposal based on the referee reports received. The principal idea behind this is that committee decisions are regarded as more transparent and objective than decisions made by any one individual who may *appear to be* (regardless of whether they *actually are*) intrinsically unreliable and corrupt [36]. Current literature however seems to indicate that precisely the opposite may well be the case: committee decisions based on peer review are prone to bias because members of the committee can influence their colleagues. In the case of the National Institutes of Health (NIH), 25 % of the reviews of research proposals have been reported to be potentially biased [37, 38]. It is believed that committees also judge science in a highly conservative and process-orientated way, adding another layer of selection against highly innovative science [39]. This means that the granting process that funds science can also be biased away from the genuinely innovative. This problem was addressed two decades ago by the founding editor of *Medical Hypotheses*, David F. Horrobin, "Peer review in the grant-giving process is so restrictive that most innovative scientists know they would never receive funding if they actually said what they were going to do. Scientists therefore have to tell lies in their grant applications" (see [27, p. 1440] and references therein).

The peer review process for both papers and grants is sometimes defended on purely statistical grounds by the assumption that averaging the opinions of several referees will yield a more objective outcome than any individual's decision. Surely five opinions are more likely to be right than one? But this is invalid. The (limited) number of referees cannot provide the degree of "statistical security" that the process claims to offer. The assumption that any unjustified opinion will be cancelled out by the other members of the circle is a misconception: a 2008 study on the peer review process of NIH grant applications estimated that it would take the staggering number of 38 416 reviewers to ensure a good, unbiased assessment of any grant proposal put before the NIH at that time [40]. The reason for this bizarre number is the large number of applications to the NIH coupled with the assumption that a proposal just above the funding line, as suggested by the rating, should be significantly better than a proposal that has been rated just below the funding line. In any case, the study showed that the number of peer reviewers usually applied in

science is too low to guarantee a fair evaluation and ranking of closely scored proposals.

The system of editorial review

The core problem of peer review is that publication should be contingent upon a referee answering "yes" to the question of whether or not a study or hypothesis appears plausible. Plausibility means that the reader is convinced that there may be something in the idea. But peer review is inherently conditioned to look for possible problems: it seeks not plausibility but certainty. A new idea is bound to be one with gaps, holes, and weak links in the argument. These are automatic cause for rejection under conventional peer review. Therefore peer review appears to be an improper instrument for quality control of revolutionary ideas.

As a "radical" journal, *Medical Hypotheses* was founded around an intrinsically different approach. Probably unique within the vast number of well-established scientific journals available today, the plausibility of the presented work was not necessarily the ultimate criterion for acceptance. The editor, David Horrobin, justified the foundation of the Journal in its very first editorial with the sentence, "I shall willingly and proudly plead guilty to the charge that I shall publish some ideas which seem improbable and perhaps even faintly ridiculous" [41, p. 2]. Horrobin was aware of the obvious consequences of this policy, since "many and probably most of the hypotheses published in this journal will turn out in some way wrong." The reason for this remarkable and unconventional approach to the production of scientific literature was the possible gain to science that could accrue from giving a forum to these ideas. If a hypothesis that is generally believed to be plausible is proven right, little progress has been made. However, if a revolutionary, implausible hypothesis turns out to be correct, the scientific leap is giant by comparison [41].

Consequently, Horrobin (together with a small editorial board) did not regard the opinions of "peers" as binding rulings on whether a paper was suitable for publication. They reviewed the paper themselves and assumed the mantle of responsibility for the acceptance or rejection of articles submitted to *Medical Hypotheses*. This is the process of "editorial review."[1] Naturally, therefore, editorial review "relies on hard-to-quantify and non-transparent individual judgments" [42]. However, so does peer review. It only appears objective because it is an anonymous process. The anonymity of the referees is no defense against subjective and prejudiced evaluations due to conflicts of interest. In contrast, bias in editorial review can be used to favor and further courageous innovation, revolutionary concepts, and originality. The inability of peer review to deal with conflicts of interest is therefore one of its major disadvantages [3]. This problem shows why, in any case, the outcome of a review process must be open for objective evaluation [42]. Indeed, editorial review is also not objective, but it is at least more honest in

[1] Of course the system of peer review also includes some editorial review. The final responsibility for acceptance or rejection lies with the editor in peer reviewed journals as well. This is why decision letters come from editors, not the peer reviewers.

that the editor stands publicly by his or her decisions [43], stating clearly that they have or have not found an idea interesting.

After Horrobin's death, editor Bruce G. Charlton assumed responsibility as editor-in-chief of *Medical Hypotheses* for the Journal's publication policy. The editorship of Charlton saw a continuation of the Journal's basic philosophy, as potentially revolutionary hypotheses were accepted for publication, "even if the paper itself is almost certainly not correct," as long as the hypothesis in question had the potential to be a "valuable contribution to the scientific literature" [44].

According to Charlton's view, wrong ideas and hypotheses can also have a stimulating effect on the supporters of the opposite, correct ideas. In order to evaluate the basic soundness of this approach, one can look back into history at the controversy between the atomists and anti-atomists at the beginning of the 20th century. The famous physicist Ernst "I do not believe in atoms" Mach—well-known for his work on supersonic phenomena and eponym of the still used Mach number—challenged his opponent Ludwig Boltzmann in an often provocative way ("Have you ever seen an atom?"). In the face of such potent opposition, Boltzmann and his supporters were forced to expand their theory, to discuss it in more detail, and to phrase their ideas on the model of the atom with the utmost precision in order to avert Mach's attacks. Indirectly, therefore, Mach paved the way for the now generally accepted model of the atom. Hence, even wrong ideas and hypotheses have the potential to be enormously valuable. The above is just one of many examples where a controversy and open discussion turned out to be a powerful and fruitful motor of science.

For many decades, until 2010, editorial review was *Medical Hypotheses*'s recipe for success. Originally, we did not want to discuss the scientific or pseudo-scientific circumstances that led to changes in its liberal publication policy and the sacking of editor-in-chief Charlton. However, a reviewer of this paper requested a respective discussion, which we will try to provide in the following. Charlton decided to accept two very controversial papers [44–46], which a committee appointed by the publisher Elsevier found to be examples of AIDS denialism and thus potentially damaging to global public health. Both papers (though most concern was expressed against the paper published by P. Duesberg et al.) have been retracted from the Journal's homepage and are no longer available. Therefore, it is difficult to comment on them in their original form. Since this is a very specific scientific topic, most of the authors of the present article do not have a professionally founded opinion on this matter, and many of those who do, view the ideas of AIDS denialism with concern. The controversial article may have been correct or may have been wrong: both cases will be discussed so as not to have to pass judgment.

What if Duesberg's hypothesis was correct? Then our current view of HIV and AIDS would be wrong and in need of a radical update. In this case, Elsevier's committee decision was wrong and an important finding would not have been published as urgently as it should have. The introduction of peer review would have been a mistake, because editorial review (by Bruce Charlton) had made the correct decision (but it was overruled).

What if Duesberg's paper was wrong? Then Elsevier's committee decision to retract the paper was correct. In this scenario, editorial review had failed to provide

the necessary quality control and a potentially dangerous study—until its retraction by the publisher—saw the light of day. However, it is common knowledge that in scientific publishing (almost) every paper that is written ultimately will be published somewhere, as long as the author persists in submitting it to another journal and is willing to make concessions with respect to the impact or reputation of the journal. In fact, a reworked version of Duesberg's paper was submitted elsewhere and finally accepted after peer review [47, 48]. Even if Duesberg's view is wrong and dangerous, the system of peer review was unable to prevent (only delay) its publication. Ultimately, the system of peer review did not perform any better than Charlton's editorial review did.

Whether or not Duesberg was correct, in 2010, a 34 year old tradition was abandoned: peer review was introduced to *Medical Hypotheses* and a new editor installed. Elsevier and the new editor-in-chief, Mehar S. Manku, state that the reviewers will be asked to take into consideration the radical spirit of the Journal. In particular, as of approximately July 2011, the "Aims and Scope" of the Journal included the following statement, "Medical Hypotheses takes a deliberately different approach to peer review. Most contemporary practice tends to discriminate against radical ideas that conflict with current theory and practice…. Furthermore, traditional peer review can oblige authors to distort their true views to satisfy referees, and so diminish authorial responsibility and accountability" [49]. In principle, this statement and its related insight deserve appreciation. However, it remains entirely unclear how their aim will be put into practice and how their review will differ from reviews at other journals. In the absence of any clear differentiation between *Medical Hypotheses*'s peer review and that of any other journal, we feel that the introduction of peer review into the publication process of *Medical Hypotheses* has the potential to become a major mistake. Dr. Charlton reported having received more than 150 letters of support by March 2010 (mostly from *Medical Hypotheses* authors) [45], indicating widespread concern within the scientific community about the policy changes.

By far the most disturbing aspect of the *Medical Hypotheses* case is the publisher's ultimate motivation for changing the journal policy. According to Adrian Mulligan (Associate Director of Research & Academic Relations, Elsevier), "*public pressure*" was the main reason for the "replacement" of editor Charlton and the introduction of peer review [50]. This exemplifies precisely the worst type of bias. Should not a scientific publisher be more devoted to science than to public opinion? Would Galilei have gotten his *Dialogue* published with Elsevier? Probably not.

Comparison of peer review and editorial review

Any method of quality control can fail: filtering out the good as well as letting through the bad. The same is true for both peer review and editorial review. *Medical Hypotheses*'s abrupt change from an editorial review policy to that of peer review in 2010 provides a unique opportunity to compare the two systems of quality control. Not all parameters may be fully comparable because, after all, a scientific paper is an individual contribution of its author(s) and not necessarily dependent on exterior

Table 1 Comparison of selected publication parameters of *Medical Hypotheses* in 2009 and 2011

	2009	2011	Changes from 2009 to 2011 (%)
Number of publications[a]	429	512[c]	+19
Average time to acceptance	16.7 days	119.7 days	+617
Acceptance within 14 days after submission	353 papers (82.3 %)	10 papers (2.0 %)	−97
Acceptance later than 100 days after submission	10 papers (2.3 %)	271 papers (52.9 %)	+2600
Number of correspondences[b]	150	47	−69
Average number of correspondences per article[a]	0.35	0.092	−74
Impact factor	1.393	N.A.	

N.A. not available

[a] Without editorials, correspondences, errata

[b] Please note that correspondences published in *Medical Hypotheses* traditionally did not always target a publication in this journal

[c] Please note that of 533 papers published in 2011, only 512 reported an explicit date of submission and acceptance. Hence we will only discuss these papers as they appear to be regular submissions

circumstances. However, we assume that the journal's community of readers and authors as well as the scientific challenges they dealt with were roughly comparable in the years immediately before and after the change.

In Table 1, we compare the *Medical Hypotheses* publications that appeared in 2009, the last year submissions to *Medical Hypotheses* were handled exclusively in editorial review, and in 2011, the first year of exclusive use of peer review. The numbers are based on a careful review of the published dates of submission and acceptance for each article published during the two respective years.

One of the most obvious differences between papers published in 2009 and 2011 is the radical change in the duration of editorial handling, as also illustrated in Fig. 1. One could argue that good science takes time, and the time between submission and acceptance is not an important measure of quality. In any case, "normal peer review" seems to take much longer than necessary even for a very thorough evaluation of a paper—as shown by many important papers published in leading journals (e.g., on the sequencing of the human genome) that were under review for weeks rather than months.

A much more important measure than the time to acceptance, however, is whether the hypotheses stimulate further ideas and research in the community. Ultimately, only the impact factor (which is not yet published for 2011) or similar analysis will tell us this, but in the interim one can examine how much correspondence has been generated in *Medical Hypotheses*'s own correspondence section (see Table 1). It is obvious that the average number of correspondences published per regular article in *Medical Hypotheses* decreased dramatically by 2011 to less than one-third of the 2009 value. This may have various reasons or combinations of reasons. It may be just coincidence that fewer authors felt the need to write a correspondence to the journal. It may be that the articles published are less controversial and hence do not call for

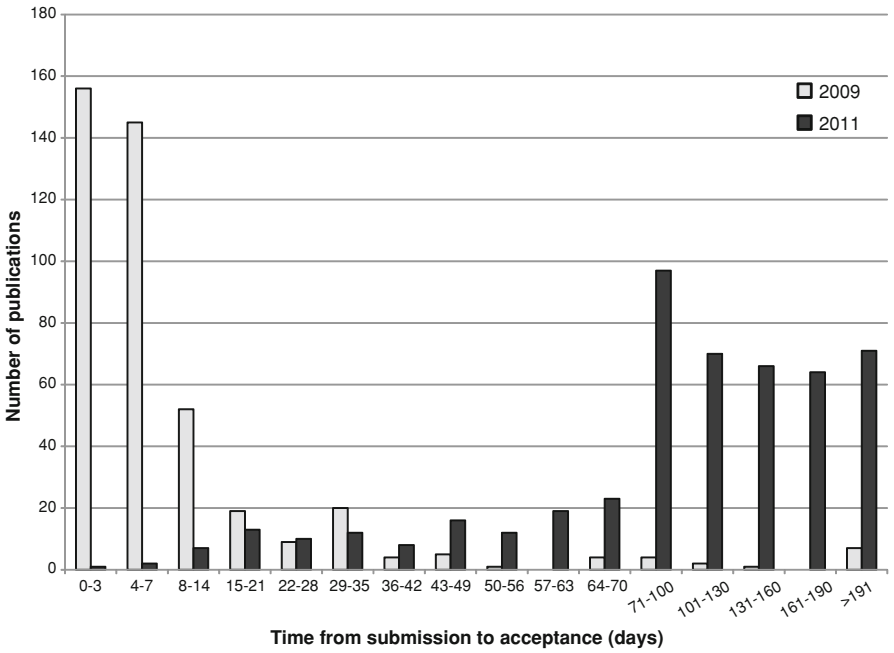

Fig. 1 Comparison of the time from submission to acceptance of *Medical Hypotheses* papers published in 2009 (*light grey*) and 2011 (*dark grey*)

immediate comment in the form of a letter to the editor. It may be that *Medical Hypotheses* suddenly did not appear as an attractive forum for a public correspondence. It may be that a comparable number of correspondences were submitted to the Journal but that the process of peer review filtered out many of them in a radical way (we do not know, however, if (all) correspondences submitted to *Medical Hypotheses* are currently forwarded to peer reviewers). It may be that the reception of articles by the community suffered for some reason (hence not provoking an echo). The latter scenario would indicate a decrease in peer usage. This may be the case if the articles published, for instance, have been conventionalized by peer review. The overall impression is that the journal has forfeited to some degree its original feature that encouraged other scientists to think. Our prediction for the impact factor of *Medical Hypotheses* is that it will slowly decline as the papers in it are seen to be less interesting and less useful, and cease to be a springboard for new research.

Conclusions

Peer review, though a widely accepted instrument for ensuring the quality of science, is not properly applicable to the evaluation of innovative, hypothetical science because as the controversial character of a paper or proposal increases, the likelihood for its rejection also increases. The referee's opinion is unlikely to be positive when judging speculative ideas, and thus, there exists a bias against the

most innovative ideas; those manuscripts which have the highest potential to become enormously valuable scientific contributions if they turn out to be true will never, or hardly ever see the light of day. Peer review is not able to deal properly with problematic conflicts of interest because it introduces bias against authors and institutions with little reputation or against the "other" party in a battle involving "big science" controversies. Further, there seem to be papers and topics for which peer review per se is hardly applicable. For instance, now that 198 authors have thoroughly discussed and expressed their views, we wonder how the opinions of two, three, or four referees could improve a manuscript such as this one.

Peer review applied to *Medical Hypotheses* will lead to the peculiar situation in which *not enough* wrong ideas are published in the Journal. The undesirability of this situation seems paradoxical, but this is the only way that the "high risk–high gain" philosophy of the Journal, which has been successful for many decades, can be maintained. In the words of the founder, David F. Horrobin, "…when hypotheses are proposed it is impossible to predict which will turn out to be revolutionary and which ridiculous. The only safe approach is to let them all see the light and to let all be discussed, experimented upon, vindicated or destroyed" [41, p. 2]. The introduction of peer review will have four consequences for *Medical Hypotheses*. One, papers will be published with significant delay. Two, the conventional "quality" of the average published paper will undoubtedly increase, at least as measured by its implicit acceptance of contemporary conventional scientific wisdom. Three, fewer hypotheses will appear that will ultimately be proved wrong (as shown by the rapidly decreasing number of correspondences—obviously, the published articles are no longer as controversial as they used to be). Four, the distinctiveness of *Medical Hypotheses* will be irrevocably destroyed. The result of this is that the peer reviewed *Medical Hypotheses* is just a conventional journal, lacking its original spirit: indeed a "zombie *Medical Hypotheses*" (to quote Bruce Charlton [45]). Many years ago, David F. Horrobin wrote, "And if we are to deliver results, quality control can be only one side of the editorial equation. The other must be the nourishment and encouragement of high innovation. There I think we have a problem. In all that is said and written about peer review, quality control appears overwhelmingly important and the encouragement of innovation receives little attention. That is a recipe for failure" [27, p. 1439]. Further, Horrobin said he was "aware of situations in which peer review has delayed, emasculated, or totally prevented the publication and investigation of potentially important findings" [27, p. 1439]. Hence, we do not share the new editor-in-chief Manku's opinion [51] that the founding editor, Horrobin, would have been "proud" of the current development of his Journal. We believe the opposite is true. We are convinced that the previous system of review at *Medical Hypotheses* provided a valuable scientific service and respectfully call for its restoration. After all, we are the peers.

Note added in proof

After this article was accepted for publication, the 2011 impact factor of *Medical Hypotheses* was published. It has decreased to 1.150, which is a drop to 83 % of its 2010 value (1.389) and its 2009 value (1.393).

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