Peer review in context*

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Scientific publishing is in a transition between the old paper-bound, static forms and the new electronic media with its interactive, dynamic possibilities. This takes place in the context of imploding library budgets and exploding magazine costs. The scientists as authors, reviewers and editors of scientific journals are exposed to an increased pressure by the their administrations and the public towards quantification, objectification and certification of scientific achievements. The "publication roulette" resulting from low-quality editorial procedures often amounts to malign censorship, which not only is experienced as a frustration by the authors, but is also delaying and hampering the progress of science. It also leads to a waste of funds under the cover of pseudo-objectivity and pseudo-legitimacy of financial decisions. Different solutions are outlined and discussed. As concerns scientific publishing, an e-print service should be established, which, in continuation of existing e-servers such as *arxiv.org*, is operated either directly by the United Nations Educational, Scientific and Cultural Organization, or by an international consortium. In order to become generally accepted by the scientists, certification criteria must be provided, which would make it possible to successfully pursue a scientific career besides the traditional peer reviewed print publications.

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I. PEER REVIEW

To a noninvolved observer, peer review can be explained as a kind procedural pattern or ritual, in which a decision over the publication of scientific reports (and/or over the funding of some research project) is reached. At the beginning, an unsolicited article is submitted by the author about some research results. The article is sent from the editor to unpaid reviewers, called *peers*. These reviewers provide reports and recommendations which are sent back to the editor. The editor anonymizes the reports and sends them to the authors. The article is revised by the author and re-submitted. This procedure can repeat itself. Finally, the editor decides whether or not the article is worth publishing or is rejected. Rejections rates vary strongly, depending on the field covered, from 10 % to 95 %. And despite the critical evaluation of the situation, most protagonists attempt to do a decent job under the given circumstances.

A. Why peer review?

Peer review has at least three main goals: (i) quality certification of scientific publications, (ii) career planning of the new scientific generation by comprehensible, "objective," quantitative criteria, as well as (iii) the evaluation of research projects requesting funding .

The importance of peer review for scientific careers is enormous: a publication which does not appear in a journal whose contributions are subjected to peer review, is mostly considered "worth nothing" in terms of career planning; and without peer review there is no certified progress in science; at least this is what is emphasized over and over again. Therefore, it is mandatory for the novices as well as for the established researchers requesting positions, status, influence and resources, to expose themselves to this verdict. And although most authors express their frustration with this kind of censorship behind closed doors, public criticism is considered inappropriate; except if one is willing to bear the consequences, such as being denoted a "whiner."

Peer review is seen primarily as assistance to the author for improving articles. It avoids the publication of uninteresting, plagiaristic, faulty, erroneous and fake results. Each reader should form an own judgment whether or not these advantages, should they be achieved, counterbalance the disadvantages of the scientific censorship. These issues deserve public concern. After all, not to a small part tax money and the pursuit of scientific progress is at stake.

B. Peer review in the historical perspective

The history of peer review needs still to be written; amazingly few details have been documented. After 1650, the first magazines of scientific societies developed, whose members understood themselves as "peers." Examples are the *Institutes de France* or the *Royal Society of London,* publishing magazines like the *Journal des Savants* or the *Philosophical Transactions,* which already used the review process among the "peers" for editorial purposes. In which form this happened does not seem to have been sufficiently examined yet.

The fact that already in the early stages the system needed improvements and adjustments is documented by a quotation of Babbage around 1830 [1]:

> *. . . it would be a material improvement on the present mode, if each paper were referred to a separate Committee, who should have sufficient time given them to examine it carefully, who should be empowered to communicate on any doubtful parts with the author; and who should report, not only their opinion, but the grounds on which that opinion is formed, for the ultimate decision of the Council.*

No reference is given to the necessity of and the reasons for anonymity; as well as to the costs of this procedure.

Often one hears the astonishment in physical circles about the willingness of one of the most outstanding physics journals of its time, i.e., the *Annalen der Physik* and its editor Röntgen, to publish the groundbreaking ideas of a hitherto unknown official in the Swiss patent office called *Einstein.* Would this still be conceivable today? Einstein's attitude towards peer review, as he experienced it in the USA, can probably be best characterized by an anecdote mentioned in Pais' biography of Einstein [2, pp. 494-495]. In 1937, Einstein had submitted an article to the *Physical Reviews* and got back a lengthy review. His immediate reaction appears unexpected to contemporary scientists:

> *Einstein was enraged and wrote to the editor that he objected to his paper being shown to colleagues prior to publication. The editor courteously replied that refereeing was a procedure generally applied to all papers submitted to his journal, adding that he regretted Einstein may not have been aware of this custom. Einstein sent the paper to the Journal of the Franklin Institute and, apart from one brief note of rebuttal, never published in the Physical Review again.*

A further anecdote is about the joke which the later Nobel laureate Bethe made by ridiculing Eddington's inclinations for numerology in deriving the fine structure constant from the absolute zero point temperature; this article got published an in *Die Naturwissenschaften* in the year 1931 [3].

The so-called "Sokal affair" [4], in which the New York physicist Sokal ridiculed the publication efforts in the Social Sciences, is already a legend. (Marketing-wise, i.e., in terms of self-promotion, this conscious fraud of Sokal is a great achievement; it moved up the then widely unknown Sokal into the center of the scientific and even general attention; Sokal was discussed, considered and invited everywhere.) The arrogance of this gentleman became clear, when physics afterwards suffered from affairs of her own; beginning with the frauds of Schön and his co-authors [5], to the asseverations of the Bogdanov brothers to have manufactured their thesis and articles, which later were certified and published in venerable peer reviewed journals, in earnestness and good scientific conduct [6]. To well camouflaged scientific fraud and charlatans probably the same applies as to perfect crimes: they remain mostly hidden. At the moment, there are no estimates of the estimated number of unknown cases of such occurrences.

It would be probably a worthwhile task to examine the angloamerican influence on the scientific publication regime in the time after the World War II. Here one may express the assumption that in Central Europe everything changed dramatically; and that the American peer review model became generally accepted; to a degree, which makes it almost impossible to reconstruct the traditions and practices of the time before the World Wars.

Nowadays completely different signals are sent from the USA, once again showing the strength and innovative potential of this great nation: motivated by the necessity of rapid dissemination of research results in high-energy physics, an area rapidly developing in the nineties, an electronic system of "preprint" or "reprint servers" developed "bottom-up," which today has become a *de facto* standard, and the main distribution channel of scientific literature: we shall deal with *arxiv.org* later on in greater detail.

II. PEER REVIEW "FROM THE INSIDE"

Scientists experience peer review in three different functions: (i) as authors, (ii) as reviewers, and (iii) as editors. In what follows, these functions will be dealt with briefly. The reader is also referred to the publications of Fröhlich [7, 8], which offer a wealth of thoughts, details, investiga-

tions and much background information on this topic (see also Ref. [9] and the following articles of the magazine *Cortex*).

A. Authorship and "Publication roulette"

As already described, an author, who wants to publish the results of its scientific work, writes an article and sends it, usually electronically, to the editor of a scientific journal. After a more or less long waiting period, the author receives a reaction, which depends on the recommendation of the reviews. To a certain extent, the author experiences a "supply sided market situation:" there are always sufficiently many unsolicited articles between which a journal editor seems to be able to select.

The referee reports are not always drawn up in a generous, respectful style of benevolent criticism. Indeed, often sarcastic, hurting and not very sober, unobjective, even humiliating remarks of the "peers" are passed on one-to-one from the editor to the author. Unfortunately, this habit, caused by unqualified, weak editors, which do not want to be bothered with the quality of the review, but are just interested in evaluations, no matter what, contributes much to a degeneration of manners in the scientific community. In the appendix, some anonymized anecdotes give a sample of what could await an author of scientific articles trying to publish them.

In a large-scale study [10] over 600 authors were asked about their experiences with peer review. The results were devastating: the authors emphasized their frustration over peculiar reports, which criticized unimportant details without dealing with the main results; they emphasized the incompetence of the peers, who treated the authors arrogantly. Many authors suspected also that from their experience many reports had not been written in order to improve the quality of the articles but to impress the editors.

In the long run, sooner or later, almost every article succeeds to get published by some peer reviewed journal. One rumors about cascade-type publication tactics, which begin with the submission to the most prestigious journals and, in the case of refusal, continues with less respected and less known journals until final publication.

The time delay caused by the peer review procedure, also for manuscripts which get immediately accepted, amounts from three months to several years in extreme cases; on the average, the delay is about half until three quarters of a year; at least in physics. (Other fields might exhibit still longer latencies.) These not inconsiderable delays, particularly in fast moving, active

research areas, contributed to the development of preprint servers, which by now have taken over almost an exclusive communication role in scientific publishing. In these fields, the additional, "post-"publication in peer reviewed journals almost exclusively for career planning is used.

From a szientometrical perspective, and for many fellow scientists and administrations, the market value of the author is derived from the market value of the journals in which this author publishes (as well as from the outside funding attracted). This procedure, which is often related to indicators such as for instance the *Science Citation Index (r)* [*SCI (r)*], which is copyright protected, owned and operated by the private firm *Thomson/ISI*, and derivatives such as the *impact factor*, results in problematic consequences and may even lead to grotesque developments. In general, for all kinds of quantitative indicators, concerns are not completely unfounded that they may lead to an inefficient form of scientific practice by adopting marketing strategies to cope with the quantitative measures rather than to concentrate on the quality of work: Quantity instead of quality!

Nevertheless, and despite of all that, often serious suggestions, comments and criticisms are conveyed by peer review, making the manuscript better and preventing mistakes. And some reports contain so valuable suggestions that they would even justify co-authorship of the anonymous peer. The question remains whether the advantages outweigh the disadvantages.

B. Reviewer: no time, no money

In a large-scale study [11], 150 research projects of physics, chemistry and economic science were re-examined by the *National Science Foundation.* The results were devastating. This study showed how strongly the acceptance or refusal of a research project depends on the choice of the particular reviewer evaluating that proposal:

> *An experiment in which 150 proposals submitted to the National Science Foundation were evaluated independently by a new set of reviewers indicates that getting a research grant depends to a significant extend on chance."* They proceed by stating that, *"the degree of disagreement within the population of eligible reviewers is such that whether or not a proposal is funded depends in a large proportion of cases upon which reviewers happen to be selected for it.*

Well into this picture fits a study, in which articles were re-submitted after one and a half to three years to the same journals in which they were already published [12]. Another issue is the

tendency of some reviewers to delay or even impede the publication of certain competition articles for egotistic self-interests.

Still another problem is the bias, with which scientists judge their area in comparison to others. The Swiss *Wissenschaftsrat*, an advisor committee of the government, attempted to gather the opinions of well established scientists about prospective future research fields. After some years these recommendations were compared with the actual developments. Many recommendations were misleading. Heinrich Urprung, for many years the president of the *ETH Zurich* and Swiss secretary of state for science and research, has expressed the findings as follows [13] (cf. also [14]):

> *At the beginning of the seventies our science advisors undertook a monumental effort to anticipate promising scientific research areas of the future. The strategy of this search consisted of asking hundreds of professors about their opinion. The result was an impressive document with reference to urgently necessary upgrades of those areas, which were already established at our universities. In this sense, the expenditure was worthwhile itself. Meanwhile, at the time, nearly nobody referred to the necessity for additional research in semiconductor technology, and nearly nobody stressed the necessity for increased efforts in the area of the energy research. Either the appropriate experts had not been reviewed, or the importance of the development of their own areas escaped them. Stated more generally, the lack of such planning results from the fact that, by definition, gaps do not have proponents. In addition to that, professors, as many other mammals and most socially organized organisms, are characterized by a pronounced territorial thinking.*

In general, speculations that peer review discriminates against innovative, not well established ideas, and favors the advancement of extensions of contemporary knowledge, do not appear completely unfounded. The latter would actually be nothing despicable, but the first is problematic.

With all the respect, distrust and contempt, brought forth for and against peer review, two further important factors should not be ignored, which are essential to an understanding of the situation: time and money. Because on the one hand the pressure on single scientists from committees and the administration gets bigger and bigger to submit as many articles as possible. On the other hand, as reviewers they are expected to prepare their review assessments anonymous, by unpaid and unnoticed. So, there is a simple rule here: a single article earns more official bonuses than

numerous referee reports. The reviewing efforts do not pay off; publishing houses and funding agencies assume the (cheap) position that this work is an integral part of scientific duty and paid off already be the scientist's salary. This lack of acknowledgment appears to be more absurd in the perspective of the rising yields of scientific publishing houses (see below).

In particular, authors who are materially and organizational insufficiently secured; which work with temporally limited contracts and in various other dependencies, find themselves in a treadmill: as authors, they have to kindly accommodate their reviewers in order to get their articles published; yet as reviewers they take pride to criticize manuscripts and research proposals, without being able to reflect and comprehend them sufficiently. This observation is related to the finding that younger reviewers recommend more rejections than older peers [15]. Further studies reflect on the discrimination of women in the reviewing process [16], as well as on the dependence on the seniority and status of the author (see also the appendix).

Maybe one way out of this malady would be the issuance of "peer review certificates" by the publishing houses, which could be redeemed and credited for career purposes by the reviewers in a sort of coupon system. This would also make it easier to analyze the distribution of reviewers over the entire population of peers, which is another issue not properly addresses in the current literature. Maybe very few peers do most of the reviewing, thereby maintaining a huge influence over what appears to become scientific literature? So far, no investigation exists which tests this hypothesis.

C. Editor

If one considers the findings quoted above, according to which the choice of the reviewers is crucial for the fate of an article or research proposal, then the editor's role is central and influential. It is amazing how little concern is given to the choice of the editors; not only by the community of peers, but also by the political institutions which provide funding. An editor may ruin a journal or fund, or may make it prosperous. For instance, not much is known how exactly the Austrian or European funding agencies select their executive editors; and one is tempted to suspect that they are nominated to fit the interests of important groups within the scientific communities. Whether such procedures are optimal or even beneficial for the progress of science is questionable; in particular if one recalls Ursprung's verdict cited above.

Editors often have to "chase down" reviewers to write reports: the reviewers must be kindly

asked, reminded and admonished repeatedly, until they deliver. There are no leverages despite moral obligations.

Complementary to the author, the editor experiences the market situation “demand sided;” at least to a certain extent: among the submitted and received articles there are few very high-quality ones. As regards soliciting reports, there are often not too many reviewers willing to seriously get involved with a manuscript.

Just as the author’s and reviewer’s role, the editor’s role is characterized by financial austerity. However, this is to a certain extend compensated by the editorial status, as well as by the influence (in the “benign” sense;) exerted.

III. FINANCIAL CONTEXT

The financial conditions of scientific publishing are characterized by imploding library budgets accompanied by exploding journal costs. The *Create* web page of the *Association of Research Libraries* grants an eloquent insight into this situation. A case study describes the precarious situation in Australia with the following numbers: In the year 1993 had 38 university libraries in Australia subscribed to altogether 200.666 science journals. Until 1998, this number decreased to 112,974 subscriptions, a relative decrease of 43.7 %. During this period, the cost of an average journal increased from Australian $ 287 to Australian $ 485, a jump of 70 %.

In another confrontation, the consumer price index rose in the period from 1986 to 1998 by around 49 %, while the average journal cost rose around 175 %, more than three times the increase of the consumer price index. In 1999, the American scientific libraries bought of 26 % fewer books than 1986. During this time, world book production increased by about 50 %.

Also in Austria the situation appears precarious. As an example, consider the following numbers, which were recently sent to an Austrian university institute, characterizing the situation of a typical Austrian library budget in a snapshot: “sum planned (bound) expenditures: 259,345 EUR; literature budget assigned by the rector: 249,340 EUR; Therefore, budget available for literature acquisitions: -10,005 EUR. (The puzzled reader may ask if a negative library budget results in fewer books.)

But the problems of one party often translates into the benefits of another: The net profits of the commercial publishing houses marketing those journals, have reach all all time high, and are still increasing; the profit margins being higher than for fiction books. For example, according

to data of the *Reed Elsevier Annual Report* the science publishing division of *Reed Elsevier* has exhibited a profit margin of 35 %-42 % in the years 1995-1999. One is tempted to view the science libraries, which at least in Europe are to a large extent publicly financed, as the "cash cows" of the international publishing houses.

According to estimates, the average total cost (including university housing, salaries and additional expenses) per published article amounts to 50,000 EUR. The average profit per published article for the publishing house, depending on the magazine, is estimated to be 1,000 EUR to 20,000 EUR. These profits can be maintained only by the unpaid efforts of authors, reviewers and editors. Odlyzko [17] (see also [18]), comes to the following conclusion:

> *"... the monetary cost of the time that scholars put into the journal business as editors and referees is about as large as the total revenue that publishers derive from sales of the journals. Scholarly journal publishing could not exist in its present form if scholars were compensated financially for their work. "*

Most publishing houses require the transfer of the exclusive copyright (not merely the right to use the article) of a scientific report by its authors and institutions. This results in the absurd situation that the very authors and institutions giving away the exclusive copyright to the journals for free, are less and less able to pay for the rising journal costs.

The *Association of Research Libraries* speaks openly and candidly of a communication crisis in the sciences, amounting to the fact that the scholars have lost control. These developments have caused the *Association of Research Libraries* to request a radical re-orientation from its customers, the scientist of North America. This goes even so far as to suggest very bold moves; such as the invitation to refuse authorship, as well as a halt on reviews and editorial activities for the scientific publishing houses. As a consequence, forms are published, with which scientists should refuse to review, motivating their denial with the rising publication costs (see appendix).

However, quite understandably, such attempts show little effect: each individual scientist would be badly advised to proceed in "Robin Hood" manners against that very instance which is of crucial importance in career planning, and which is essential for the official justification and evaluation of the scientist's research work. Especially for the novice, not very well established, scientist, this refusal to publish in peer reviewed journals, would amount to scientific suicide. Career decisions are supported by and justified with publications in as prestigious a journals as possible. The higher a marketing value of a journal the better; without publication in a journal with peer review, there

is no career in science.

IV. E-PRINT SERVERS: THE IMMATURE ALTERNATIVES

Despite and because of the problems stated above, it should only be a question of the time until electronic forms of publication will become generally accepted. Inevitably, with the information revolution, also the forms of scientific communication will change. The large publishing houses already feel the course of the time and react ambivalently: on the one hand, they understandably do not like to lose the good business, on the other hand, an adaptation of the practices appears necessary for future profits.

Associated with this tactics is a careful, "snuggling approach" in handling "grassroots" initiatives such as for instance *arxiv.org*; even if its operators cope with copyright issues rather vaguely. In what follows we shall discuss this initiative in some details; partly because functionally, it is one of the most advanced ones, partly because it is one with the highest penetration among the communities involves, and partly because of its apparent success, the pressing problems appear clearly and visibly.

A. Example *arxiv.org*

arxiv.org is a reprint and a preprint server, which is freely accessible publicly to all those who have access to the world wide web. The speed and simplicity of the information flow, as well as the relatively small costs of access are important success elements. *arxiv.org* started as initiative in high-energy physics, and now covers almost all subfields of physics as well as larger parts of the mathematical and information sciences. Configurable daily email messages are sent out to the subscribers, containing the headline and abstracts of the articles submitted to the database. The links in these emails yield to the manuscript in various full text representations. This seemingly ideal situation copes with some difficulties, which will be mentioned below.

It is certainly not the intention of this article to excoriate *arxiv.org.* The following criticism should be understood as a feedback and attempt to make *arxiv.org* even better; maybe also to resolve its functions and content into some comprehensive archive, which may be able to lead the way to new, more effective forms of scientific publishing, serving the community even better that today's archives and peer reviewed journals.

1. Copyright skeletons in the closet

Due to the large popularity of *arxiv.org*, and to increase the dissemination of their research results, many authors do not only submit their drafts and preprints, but also manuscripts which have already been published. Although the layout mostly looks somewhat differently than "the original article" in the peer reviewed journal, these copies contain the identical "original text;" as well as all the illustrations, tables and so on of the original manuscript. For authors, this is a reasonable procedure, since self-publication in *arxiv.org* may reach a much larger audience than the journal publication. *arxiv.org* encourages its authors even to enter the explicit journal reference. (Of course, journal references should be added to a preprint only after final publication.) Indeed, the quality of the article in *arxiv.org* may be even higher that the one in the paper journal, since errata and further revisions can be easily included after post-publication. (All previous version remain in the database and can be accessed publicly.)

These copyright infringements by many authors make *arxiv.org* vulnerable to lawsuits of publishing houses. Virtually at any time one of the large publishing houses, in order to protect its profits and distribution channels, may sue *Cornell University,* the present operator of *arxiv.org,* because of breach of copyright. The American private university *Cornell,* in order to protect itself, might have no other choice that to shut down the operation entirely. So far, this did not happen; probably for two reasons: (i) on the one hand each publishing house cringes because of the negative publicity in and the affront to the scientific community, (ii) on the other hand, the losses caused by cancellations of subscription and substitution by *arxiv.org* on the part of the libraries are still relatively small. Stated pointedly, at the moment, the losses in terms of publicity may outweigh the financial gains. Maybe, in this sense, the publishing houses do not want to wake up the "sleeping tiger." But what would happen when more and more subscriptions are canceled by the research libraries and profit margins decrease, appears unforeseeably.

So, unfortunately, without clarifications of legal issues regarding copyright, the fate of *arxiv.org* seems to be uncertain. Only one letter of an attorney could cause *Cornell* to shut down the *arxiv.org* servers. This prospect, which particularly would affect the global physics community, appears as legal-organizational nightmare. Because *arxiv.org* thereby depends on the goodwill of the publishing houses, whose grace could be lifted at any time according to the discretion of commercial publishers and their economic considerations and interests.

B. Obscure procedures

arxiv.org still fights with a further problem: since the authors self-publish their manuscripts, *a priori* it cannot be avoided that some "quacks" self-publish their treatises as well. It is not always completely evident who exactly qualifies as "quack," and whose work does not deserve publication. Here also *arxiv.org* is hit in full hardness with questions of quality management; one answer being peer review.

At the moment, within 24 hours before the final publication, a decision for or against the final admission into the data base of *arxiv.org* is made. This is by far quicker that the standard procedures, which may take months to years. The decision is made by several, partially anonymous, moderators, which take over a kind of editorial or publisher role. *arxiv.org* explicitly states [20]: *"We reserve the right to reject any inappropriate submissions."*

At present, there does not seem to be any kind of official appeals policy, such as for the journals of the *American Physical Society.* The only possibility remains an informal request by email conversation with an anonymous censor. However, the censor's power of decision remains absolutely.

Rumor has it that "black lists" exist, which exclude "apparent quacks" from publication at *arxiv.org*. As a consequence, law suits have been filed against the operators of *arxiv.org* by authors who were excluded and who insist on their right of free speech guaranteed by the American constitution also in the scientific domain.

The censorship of the moderators is not completely incomprehensible: for legal ("criminal content") and technological ("huge data scrap") reasons, no archive of the world might get along without censorship. It would however be not totally unreasonable to take a very liberal position in these matters; after all, search engines in the web make already available, more or less nondiscriminatively, metainformation and hyperlinks to (parts of) the entire, "world wild" uncensored web.

There seems to be no objective demarcation criterion what exactly can be considered an "easily recognizable nonsense." Due to the informal character, this term can only be outlined heuristically and concretized subjectively. Many old Greeks would have recognized for example someone as a quack, who would have maintained the "easily recognizable nonsense" (at that that time), that the earth might be a ball, surrounded by a thin layer of air, circling the sun in an almost empty space; surrounded by hundreds of millions of galaxies, which again consist of millions of stars. Almost per definition, revolutionary scientific ideas are difficult to separate from emanations of "quacks."

So, the question arises whether or not risking to purge highly innovative approaches is worthwhile censoring a few "quacks."

Thus the hardly defined standards of self-publication, the alleged black lists and the arbitrariness of moderators make it necessary to develop an institutionalization of censorship.

1. Quality control

Closely related to the obscure censorship procedures just described are the quality criteria which a publication must meet in order to qualify for *arxiv.org*. The following declarative statement is at the starting page of *arxiv.org*: *" The contents of arXiv conform to Cornell University academic standards."* The reader is left puzzled what exactly is meant by "the Cornell University academic standards;" nowhere an explanation is given. Again, the necessity of more transparent editorial policies is evident.

2. Local organization, international goals

At the moment, *arxiv.org* is operated by Cornell, as it is stated clearly on the starting page: *"arXiv is owned, operated and funded by Cornell University, a private not-for-profit educational institution. ArXiv is also partially funded by the National Science Foundation."* Nobody alleges *Cornell* or any individuals to operate *arxiv.org* for marketing reasons alone, or for any kind of disrespectful intention. Nevertheless, the present form of organization of *arxiv.org* as the worldwide archive of literature in the physical and related sciences, operated by a private American research institution, appears hardly acceptable. The archive is just too successful to be owned and operated by a single institution, which may be much too susceptible to the possible arbitrariness of groups whose self-interests and selfishness might place the own benefit over the benefit of the international scientific community at large.

C. International e-print server of the UNESCO

Here we shall briefly outline a proposal for an e-print server operated by the *United Nations Educational, Scientific and Cultural Organization* (UNESCO), or at least by an international consortium. Such a service would have to meet the following criteria.

1. Copyright issues

The copyright status of archive entries should be consolidated and clarified. A model similar to the GNU Free documentation License (GDFL) would for instance be conceivable: Free availability of the full contents; any further development of which should be accompanied by a reference to the original source; a method which is a standard in the tradition of established scientific quotation practice as well.

2. Clear regulatory schemes

The complete deletion of articles and authors from the data base (if ever) should happen according to openly discussed principles. Here a co-operation should take place between scientists, layman judges, as well as philosophers of science, science historians, librarians and specialists in documentation.

3. Interactivity

Already *arxiv.org* has given its authors the possibility to revise the manuscript, while retaining the older versions. Readers could be given the possibility both to anonymous, as well as to not-anonymous discussion and criticism. Also, peers could be given the possibility for "article sponsorships ", in which other authors signal their non anonymous agreement with and promotion of an article.

4. Evaluation and quality certification

In the absence of traditional peer review, by far the most difficult problem is the consequence for career planning and certification of scientific achievements. New forms of quantification, as for instance the evaluation of the access data to a web server, for example "hit lists," are vulnerable to attack and does not offer any valid criterion.

Mixed scenarios of co-existence between peer reviewed and non peer reviewed articles together in one big database would also be conceivable. In this case, certain articles might get certification with special procedures and certificates, for instance similar to peer review, which distinguish it over other articles, which do not have this certification.

One could also conceive of a system of "peership" in a somewhat similar way as in the medieval trade guilds: if science "apprentices" acquire sufficient status, standing and experiences, for instance by collecting enough certificates, one could declare them to be "Peer." This is not as absurd as it may sound; the old system of academic lecturership (German "Dozentur") works in a similar way. Once in this status, they could provide certificates, or judge and certify work of others on the e-print server and elsewhere.

Alas, if one perceives scientific career planning in another light, these problems appear not so difficult and all-important: often, certifications and quantitative criteria are used merely for the post-justification and "objectification" of subjective opinions and career decisions.

V. DOES PEER REVIEW MORE GOOD THAN BAD?

In the long run, this question probably cannot be answered with a clear "yes" or "no." The connections with scientific career planning and business are too complex. Everyone should form an own judgment.

The author recognizes the great advantages and assistance, which peer review offered to him throughout his scientific activities; yet he believes that these advantages were at least partly nullified by the often senseless delay of publications, and sometimes associated with a distortion of contents and vain expenditures. This may be particularly the case for "original" and innovative contents, and may not be so urgent for well established research topics, where the quality improvement gained by peer review may be marginal anyhow.

Maybe the most decisive factor will be money; the libraries and public households, as well as the autonomous universities will not want or simply will not be able to pay the exploding costs of the peer reviewed printed media. It is unlikely that the scientific community will bring down peer review because of a widespread refusal to review and edit; rather there will be a constant, concealed deterioration of the quality of unpaid and unrewarded reviews.

Eminent scientific European organizations, such as for instance the *Centre National de la Recherche Scientifique (CNRS)*, the *Max-Planck-Gesellschaft (MPI)*, or the *Deutschen Forschungsgemeinschaft (DFG)*, just to enumerate a few, begin to acknowledge the new developments, which are tied to the new electronic media, and which are revolutionizing scientific publishing as these lines are written. In their "Berlin Declaration on Open Access to Knowledge in the Sciences and Humanities," they express their commitment to open archives, as well as their determination to

honor such publications for career planning and quality management [21]:

> *"[[…]] Obviously, these developments will be able to significantly modify the nature of scientific publishing as well as the existing system of quality assurance.*
>
> *[[…]] Therefore, we intend to make progress by [[…]] encouraging our researchers/grant recipients to publish their work according to the principles of the open access paradigm [[…]] developing means and ways to evaluate open access contributions and online journals in order to maintain the standards of quality assurance and good scientific practice [[…]] advocating that open access publication be recognized in promotion and tenure evaluation. …"*

It remains to be seen, if and how fast these organizations will actually adopt the principles to which they have committed themselves. Perhaps courageous prominent and financially secured authors with high status should make a first step. This would make necessary a type of open archives which makes sure that no "backup" from conventional publishing is necessary. However, at the moment, despite the early success of *arxiv.org* and others, no such commitment exist.

As concerns the funding of research proposals, radically different allocation and distribution strategies of are conceivable and appear not totally unreasonable; one possibility would be to distribute about 70 % of the funds via traditional peer review channels; 20 % over a system of lay judge; as well as 10 % completely at random. Such a strategy would have to be accompanied and adapted by additional evaluations, which would have to take place again via scientists as well as layman judges.

Finally again an aspect of scientific should should be mentioned, which one cannot value high enough: Science can remain only alive and productive if it is operated with passion and joy and is fun to pursue. It makes little sense to obstruct young people with little pay, who are willing to dedicated much of their lifetime to the scientific progress, through malign treadmills organized around the presently executed "objective" schemes of peer evaluations. That will simply not work, and will result in a waste of taxpayer's money.

It is really amazing, how indifferent the scientific establishment at large, reacted to the findings which clearly indicated substantial problems for instance in the funding of research projects. This neglect, for instance of the Cole report [11], suggests that the stakes of particular interest groups might be at work; stakes which might reflect self-dedication and self-interests, but might not serve the best purposes of the societies or the trusts which raise the money.

In the long run, everything is subjected to a historical change; this applies also to scientific publications and the methods how scientific achievements are evaluated. One is reminded of president Roosevelt's Address to the U.S. Congress in 1941 [19]: *"we have been engaged in change – in a perpetual peaceful revolution - a revolution which goes on steadily, quietly adjusting itself to changing conditions."*

Anhang A: ANECDOTES

The following authentic anecdotes were anonymized. This is neither a comprehensive collection, nor is it representative. These anecdotes should be understood as just a few of the more or less funny stories almost any author, reviewer or editor can tell.

1.

A publisher received two contradicting peer reviews of an article: The first report found that the idea was pointless and unrealistic, however its formal elaboration correct. The second referee found that the idea was extremely interesting, yet its formalization was bad.

2.

In his review, a peer stated that he would not recommend publication if the article came from a less established author and recommended the article with these *provisos*, for reasons of seniority. Afterwards, the "anonymous" peer reviewer contacted the author privately and attempted to direct him to his own work.

3.

After his retirement, a very renown author tried to publish some of his research under other "brand" names. He wanted to test the system. Thereby, he failed completely; most of the works of the seemingly "unknown" authors got rejected immediately. They were accepted with a breeze under the author's "brand" name.

4.

Another, very renown, author expressed his unwillingness to expose himself to peer review and stressed that he does not publish any more in peer reviewed journals. He had the feeling that the referee reports mostly missed the point and were mean and not helpful at all. He had enough invitations to write contributions for conferences and anthologies, for which the editors usually stated their criticism much more carefully. (See also the Einstein anecdote.)

5.

A team of researchers decided to publish a very important and original result not in a peer reviewed journal because of the danger of the delay and refusal by peer review, but rather "hid it" in a conference volume. This article was then cited by and based upon a vast number articles in peer reviewed high-ranking journals.

6.

Scientist A called Scientist B and asked for assistance. B should write an article, in which A's point of argument was defended against criticism of author C. B agreed, and wrote the article after consulting A many times. The first round of reviews of B's article claimed that B understood nothing at all about A's intentions (note: B was animated and consulted by A). Therefore, the article got rejected immediately. The second round of Referees produced a nasty referee report, in which the reviewer again did not deal with details, but called the paper "perverse." On the basis of this judgment, the paper was finally rejected. It was accepted almost immediately by another journal.

7.

The publisher of one " Letter" journal, which, according to its own understanding, is dedicated to the "rapid dissemination" of scientific results, needed one and a half months just to decide that the length of the article exceeded the permitted length by five per cent. The article was rejected for that reason without further review.

8.

On the door of a colleague a poster declares: *"Save a Tree, Reject a Paper."*

Anhang B: SAMPLE LETTER AGAINST REVIEWING ACTIVITY

The *Association of Research Libraries,* in the framework of her *Create*-initiative, has issued the following sample letter [22]:

> *Dear ——-:*
>
> *It is with great regret that I notify you that I am no longer able to serve as a reader/referee for articles submitted to Title of Journal.*
>
> *I am brought to this decision because your pricing policy for this journal is at odds with a fundamental value of scholarship, to make scholarly research as widely available as possible. Because of the journal's extraordinarily high cost and astonishingly high annual price increases, it has effectively been placed out of reach of many of my colleagues whose libraries can no longer afford it.*
>
> *I feel that you have lost touch with the core purposes of scholarly communication, and I cannot, in conscience, participate in an enterprise that apparently values profit more than the goals of scholarship.*
>
> *Moreover, I shall now seek to support, through my submissions and my reviewing activities, alternatives to Title of Journal that maintain affordable costs, as well as cost increases that are clearly related to actual production costs and added value – in short, costs that promote the widest possible availability of my work and the work of my colleagues.*
>
> *Should you change your pricing policies so that they are more in line with scholarly values, please let me know.*
>
> *Sincerely,*

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[17] A. M. Odlyzko, First Monday **2** (1997), http://firstmonday.org/issues/issue2_8/odlyzko/index.html, URL http://firstmonday.org/issues/issue2$_$8/odlyzko/index.html.

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[20] http://arxiv.org/help/general

[21] http://www.mpg.de/pdf/openaccess/BerlinDeclaration_en.pdf

[22] http://www.arl.org/Create/faculty/tools/letters/letters1.html und http://www.arl.org/Create/faculty/tools/letters/letters2.html