The Future of Scientific Publication

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A) Introduction.

It has become a truism to say that the Internet is changing the world. Tim Berners-Lee invented the Internet protocol in 1980 to facilitate sharing and updating information among researchers at CERN, the European high-energy facility in Geneva, Switzerland. Now, many scientists all over the world use the Internet to retrieve information. Yet the system of scientific publication is mainly still the traditional one: A scientist writes a research paper, a monograph, or a book, and submits the manuscript to a scientific journal or publisher. An editor sends the manuscript to referees who recommend for or against publication. The whole thing is called "peer review". More about it later.

The traditional process is extremely expensive, very slow, and deeply flawed. Scientific journals proliferate and libraries have difficulties subscribing to all of them. Books, especially textbooks, are extremely expensive, and students find it difficult to afford them. Therefore, I would like to propose a system that is much faster, much cheaper, much fairer, and much more efficient:

B) A proposal.

1) Every scientist should be encouraged to create his or her own website and publish all of his or her work on this website. First, most universities and research institutions now can easily set up such websites. Second, publishers and printing shops are no longer needed for typesetting. Most scientists already know how to do their own typesetting by using computer software such as TeX. Even now, almost all scientific journals require that papers be submitted in a typeset form.

2) The requirements for obtaining a Ph.D. should be modified. The doctoral candidate should submit his thesis on a website created by the university in his name. The members of the Ph.D. committee can then examine the thesis either by reading it on their computer screen or by printing it out. Also, instead of making a thesis a single document, it could be a collection of several research papers. The committee will have to decide whether or not there is enough material to justify a Ph.D. A lot of red tape

can be avoided in this manner.

3) Scientists should no longer feel that their work should be submitted to a journal. If it is on their website, it is accessible to everybody in the world who is interested, in fact more easily than through a journal. When it comes to evaluating the value of the work for deciding on promotion and tenure, this can be done by just sending a list of titles from the website to reviewers when soliciting recommendations. It will no longer be necessary to forward reprints or preprints. Some of the flaws of the "publish or perish syndrome" can be avoided because the promotion committee will be less tempted to just count the number of papers published by the candidate rather than having a thorough examination of the quality.

4) If a work has been published on his website, the author can easily make corrections and improvements at frequent intervals. If it has been published in the traditional way, it becomes frozen and it is difficult to publish corrections and improvements. Also, on a website, one can publish preliminary manuscripts and complete them eventually.

5) A scientist can more easily find out about papers that might be relevant to him by typing key words into Google or perhaps into a search engine specializing in science. This will lead him to websites with papers that might be worth looking at. Some indication of the value of a paper will be the number of times it has been looked at or linked to. This is analogous to the present citation index. (In fact the founders of Google used this citation index model to order the responses when typing in a search.)

I believe it is inevitable that the system I propose here, or a variation thereof, will eventually prevail.

C) My experience.

I will now describe my own publishing experience to show how and why I arrived at my proposal:

From 1952, when my first paper (as a co-author) was published, until about 1985, I had no difficulty getting published anything I had written. One reason was that almost one half the papers were published in the *Journal of Rational Mechanics and Analysis* or the *Archive of Rational Mechanics and Analysis.* Both of these were edited by Clifford Truesdell. He was my thesis advisor, and he invited me to be co-author of the *The Non-Linear Field Theories of Mechanics,* published in 1965 as part of the *Encyclopedia of Physics*, reprinted separately in 1992 and 2004, and translated into Chinese in 2000. Truesdell published anything I wrote without any request for changes. In fact, in 1974, he induced the Springer-Verlag to publish a book entitled *The Foundations of Mechanics and Thermodynamics, Selected Papers by W.Noll.*

My difficulties began in about 1985, when I finished a 393 page manuscript entitled *Finite-Dimensional Spaces: Algebra, Geometry, and Analysis* and submitted it for publication to the Springer-Verlag. It was examined by Paul Halmos and rejected. He had written a very influential book entitled *Finite-Dimensional Vector Spaces*, which had a great influence on me when I was a student. My book was a sort of improvement and extension of his. I cannot suppress the suspicion that he rejected my book because I was treading on his turf. Another publisher published the book in 1987, mainly because a friend of mine was one of the editors of a series called *Mechanics: Analysis*. However, the book has nothing to do with mechanics. The ISBN system put the book in the category *Functional Analysis* despite the fact that it has nothing to do with that, either. More about it later.

In 1993, Vincent Matsko, a doctoral student of mine, and I produced a 239 page manuscript entitled *Mathematical Structures of Special Relativity* and submitted it for publication to the Springer-Verlag. They promised to publish it in a series called *Springer Tracts in Natural Philosophy*. Then the series was discontinued and the Springer-Verlag reneged on its promise. Later, I received a letter from the Cambridge University Press telling me that they would be interested in publishing something I had written. So I send them the manuscript just mentioned. It was rejected without explanation.

In 1988, I received a letter from the *Reviews of Modern Physics* informing me that a 1961 paper by Bernard Coleman and me had become a *citation classic* and that they would welcome receiving other papers from me. In response, in 1995, I sent them a manuscript entitled *On Material Frame-Indifference*. The paper was rejected, and here is a quote from the reviewer:

“I enjoyed reading this paper very much and would like to see it published. I am afraid, however, that the Reviews of Modern Physics is not the appropriate place. I believe that the overwhelming majority of the readers of the journal will consider the paper unreadable. Not because the material presented is intrinsically difficult, but rather because the author's individual form of the ‘Bourbakian' style is far removed from anything that physicists are willing to digest. .... Professor Noll is highly respected in the mathematical community and has more than once proved himself to be ahead of his time. ..."

I decided to incorporate this paper in a 73-page booklet entitled *Five Contributions to Natural Philosophy,* which contained proposals for updating *The Non-Linear Field Theories of Mechanics*, published and reprinted 2 times by the Springer-Verlag as mentioned before. In 1994, I submitted it to the Springer-Verlag. It was rejected by an editor with the following explanation: "As I see it now, it is a kind of last will with a very personal character. Nowadays this is not enough for publication." My interpretation of this experience is that one reviewer thought that I am so far ahead that the audience is too dumb to understand me, and the other thought that I am too old and behind the times and should shut up. They cannot both be right.

There were several more run-ins I had with editors and reviewers. Here are two examples:

1) <u>From the Reviewer</u>: "This paper is written in a formal style that has long been out of fashion in the Journal of Rheology, and it will 'put off' many readers.

The paper at hand depends in a critical way on parts of [FC][1], which is an unpublished manuscript on the senior author's home page. To permit this citation would require a major change in the journal's editorial policy, which I would personally discourage."

<u>From my answer</u>: "It is not a matter of style, but a matter of mathematical infrastructure. Unfortunately, most physicists are still stuck with an outdated mathematical infrastructure, using variables, constants, and parameters rather than sets and mappings. I believe the new mathematical infrastructure will prevail, but it may take another 50 years.

Much of my recent work has been published only on my website. It is available free of charge to anybody in the world, and I reject the claim that it is 'unpublished'. I am 82 years old and no longer subject to the 'publish-or-perish syndrome'. I refuse to waste my time wrestling with high-handed editors and reviewers that are not my peers."

2) Here is a complaint of mine that was published In the *Notices of the American Mathematical Society.*

"I recently submitted a manuscript for publication to the *Bulletin (of the American Mathematical Society)*. It was rejected with the following quote from one referee: 'One might tell him that elementary results couched entirely in his own non-standard notation won't be read by anyone'. I have had papers rejected before, but never with such insulting language.

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[1] *Five Contributions to Natural Philosophy*

My complaint, however, is more general. Mathematical journals will publish anything that contains ‘new results’, especially ‘deep’ new results, no matter how obscure, incomprehensible, and insignificant outside a very narrow ‘field’. However, new perspectives, insights, ideas, and concepts, especially if they span more than one ‘field’, have a very hard time gaining respect.”

The editor personally wrote to me in response, and his response was also published in the *Notices.* I refuted his response but never got a reply. The Notices not only refused to publish my refutation but even refused to publish the fact that I had written one, giving the readers the impression that I meekly gave in.

By 2004, I became very frustrated. Then I found out that my university, Carnegie Mellon, made it very easy for me to establish a website. Since then I put all my recent work on my website. It now lists 27 items with a total of about 1200 pages, most of them not published elsewhere. I decided that I would never again submit anything to a journal or publisher, although I will not object if a co-author does so.

My book *Finite-Dimensional Spaces: Algebra, Geometry, and Analysis* mentioned above is still available by mail order but only at the ridiculous price of about $350. It is intended as a textbook for an advanced undergraduate of beginning graduate course, but no instructor would dare to require his students to buy it. Fortunately, a very good secretary at CMU has been able to revive the original TeX input file, and a corrected version can now be printed out from my website for free.

Several of my articles have been picked up by other websites. For example, I wrote an essay called *The Role of the Professor.* I submitted it to the *Chronicle of Higher Education* in 1997. It was rejected but is now on my website. I found about 10 websites where it is linked to and discussed, in the US, India, Brazil, Greece, and France.

D) Peer Review

My experience shows that the present system of peer review is not the impartial impersonal process that some people believe it to be. There is already a vigorous debate about it, as I just found out by googling “peer review”. For example, I found an article in the *Financial Times,* published on June 11, 2008 with the title *Science stifled? Why peer review is under pressure*. On the website *blogs.nature.com/peer-to-peer* there is a discussion of this issue with the title *Stifling innovation or filtering for excellence?*

It should be clear that the system proposed here would make it impossible to stifle anybody. I am afraid that filtering for excellence will remain extremely difficult. Here are some suggestions:

1) Every author should put an invitation like the following on his or her website: *Any comments, reviews, critiques, or objections are invited and should be sent to the author by e-mail*. (I have this on my website.) The author should reply to any response and initiate a discussion.

2) Every author should notify his or her worldwide colleagues as soon as a new paper has been published on the website.

3) The traditional review journals (e.g. *Mathematical reviews* and *Zentralblatt*), or perhaps a new online journal, should invite the appropriate public to submit reviews, counter-reviews, and discussions of papers on websites and publish them with only minor editing.

4) Promotion committees in universities should give credit to faculty members for writing reviews.