Stevan Harnad, University of Southampton, May 8, 1999

[This is a revised draft of comments on the ebiomed proposal, sent to Harold Varmus earlier.]

The following are my comments on:
http://www.nih.gov/welcome/director/ebiomed/ebiomed.htm

This extremely welcome and important initiative is deserving of the strongest support. The following recommendations are made in the interests of strengthening the proposal by clarifying some crucial central aspects and modifying or eliminating some minor, weaker aspects.

> ***E-BIOMED: A PROPOSAL FOR ELECTRONIC PUBLICATION IN THE BIOMEDICAL SCIENCES***
>
> *Prologue*
>
> *The full potential of electronic communication has yet to be realized. The scientific community has made only sparing use thus far of the Internet as a means to publish scientific work and to distribute it widely and without significant barriers to access.*

This generally accurate assessment of the current failure to exploit the full potential of the Internet for scientific publication has one prominent and extremely relevant and important exception. It would be much more accurate as well as helpful to note this explicitly from the outset, as this notable exception is very likely to be the model for all the rest of the disciplines:
Physics is the exception (and to some degree, mathematics). It is now both an empirical and a historical fact that well over half of the current physics (journal) literature is freely available online from the Los Alamos Archive and its 14 mirror archives worldwide, and is being used by perhaps 50,000 physicists a day.
http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

It would be misleading in the extreme to describe this as "sparing use"! Instead, it should be acknowledged that this has been a revolutionary change in Physics, and if there were a way to extend it to the other sciences (and the other learned disciplines) then the full potential of electronic communication WOULD indeed be realized.

I stress this, because to pass over the revolution in Physics as if it had not happened is not only to fail to give historical facts their due, but it is to miss an important lesson for the rest of the scientific and scholarly world in general, and the Biomedical Sciences in particular.

Insofar as the other disciplines are concerned, the paragraph quoted above is a fair

description of the status quo. The only bit of ambiguity is the word "publish" in: "sparing use thus far of the Internet as a means to publish scientific work and to distribute it widely and without significant barriers to access."

"Publish" has two meanings in this context. One is "to make publicly available in written form" (whether on paper, tape or screen), and the other is "to appear in a refereed journal." It is best to distinguish these two, as many people these days, usually well-meaning but extremely under-informed about the nature of peer-reviewed publication, have been suggesting that the latter (refereed publication) be watered down or abandoned entirely in favour of the former (making publicly available online).

I think that such proposals (to modify peer review or substitute for it the mere public online distribution of papers -- I am not speaking of the E-biomed Proposal here, but of the need to distance it from such proposals) are both (1) risky and (2) counterproductive

(1) Proposals to modify peer reviewed publication are based on armchair speculation about publication and quality control, rather than on any real experience with peer review or any tested alternatives to it (there are none at the moment). Hence armchair proposals put the quality and reliability of the research literature at risk without any proven alternative, should any substantial number of well-meaning people decide to go ahead and implement such proposals on any scale without first carefully testing them out empirically.

Peer review can certainly benefit from study and improvement, and it is indeed being studied empirically, but not by the armchair (or screenside) tacticians. This research takes time and careful experimental trials. And it is COMPLETELY INdependent of the medium -- paper or online -- in which the publication will take place. (The online implementation of refereeing can be much faster and more efficient, but this is just as true for paper publication, and indeed more and more of classical peer review is being implemented online already).

It is accordingly arbitrary and erroneous to couple changes in quality control mechanism with changes in medium a priori. Not only is it impossible to sort out the effect of two empirical variables if you change both of them at the same time, but if quality control is compromised by the implementation of untested alternatives, then the effect could be misattributed to the online medium with which is was coupled, thereby setting back the day when the learned community finally realises the full benefits of a free online corpus. This is why such proposals are not only risky (1), but counterproductive (2): They can set back the online agenda instead of advancing it.

Change one variable at a time. If one's mission is to reform quality control, then study and test new alternatives empirically. But if one's mission is to make the current quality-controlled literature, such as it is, freely available to everyone, everywhere online, rather than having access to it continue to be obstructed by toll-barriers (Subscription/Site-License/Pay-Per-View, S/L/P), then there is no need either to await the reform of peer review, or to test whether free access would be a good thing! The Los Alamos Archive has already proved that it is a good thing; the world Physics community has already voted with its eyes and fingers (and its papers, which are being self-archived in the LANL Archive at an astounding and accelerating daily rate).

So: About "publishing" vs. "distributing": the picture is clear now: Authors can now publicly self-archive their unrefereed preprints as well as their refereed reprints. There is no reason to redefine "publication." Let it continue to refer to acceptance by a refereed journal. And let authors continue to submit all their papers to the established refereed journals. But let them also self-archive them (both as unrefereed preprints, and, once accepted, as refereed reprints) in both their local institution's archive and in a global archive such as LANL, with which E-biomed should COLLABORATE, emulating its dramatically proven strengths, rather than trying to re-invent them or modify them a priori with untested incursions into either peer review or publication.

Preprint and reprint Archives are collective services to the world scientific community; their efforts and resources should be pooled to take advantage of economies of scale as well as to share the momentum of the faster moving disciplines.

See: http://www.library.yale.edu/~okerson/subversive.html

> ***E-BIOMED:***
> *Informative and even visionary essays have explored this topic (see, for example, articles by*
> *Ginsparg http://xxx.lanl.gov/blurb/pg96unesco.html,*
> *Walker http://www.amsci.org/amsci/articles/98articles/Walker.html, and*
> *Harnad http://www.princeton.edu/~harnad/nature.html,*
> *and references cited therein, as well as other recent proposals*
> *http://library.caltech.edu/publications/scholarsforum and*
> *http://www.arl.org/newsltr/202/intro.html.*

I have done some critical commentary on both the Walker proposal and the CalTech proposal. It all appears in the American Scientist Archive:

http://amsci-forum.amsci.org/archives/september-forum.html

In a nutshell, Walker proposes financing free online eprints of published journal articles out of journal offprint page charges; but why should an author want to pay those charges if he can already self-archive, in his local institutional archive and the Global Archive (LANL/E-biomed), for free? There are some issues about how to pay for the quality control, and page charges are indeed the right way, but not author offprint charges levied by a journal that still blocks access via S/L/P!

The ARL initiative is largely backing new forms of licensing. Inasmuch as these retain the author's right to self-archive for free, they are commendable; inasmuch as they help to preserve S/L/P barriers -- in the form of L alone -- they are counterproductive.

The shared desideratum of all these initiatives is this:

"It is easy to say what would be the ideal online resource for scholars and scientists: all papers in all fields, systematically interconnected, effortlessly accessible and rationally navigable from any researcher's desk worldwide, for free."

The way to arrive at this optimal outcome is through online self-archiving by all authors (locally and globally). THAT is what needs to be encouraged and facilitated. The rest will then take care of itself (although we do need a rational transition strategy to cushion the conversion of publishers from hybrid paper/online publication with costs covered through S/L/P access barriers, to online-only publication with the scaled down cost covered by up-front page charges, and the literature then barrier-free for all).

> ***E-BIOMED:***
> *Before describing our proposal, it is important to acknowledge the strengths of the current system for published scientific work, because it has served the scientific community well for over 300 years.*

I agree completely with the description that followed this passage, of the value of the classical system of peer reviewed publication. I would just add that even mentioning it risks introducing a red herring, because there is no need whatsoever to tamper with this proven system of quality control in order to achieve the optimal outcome above.

> ***E-BIOMED:***
> *No proposal to change the way scientists publish their results and ideas should ignore these and other virtues of the current system. But we believe that current practices also have many liabilities and that these can be addressed by an evolutionary approach that need not threaten most of the*

> *benefits attributable to the print-based publication system that is now in place. More importantly, electronic publication can offer several remarkable benefits that could never be achieved through the current system. Many of these benefits depend on low-cost, barrier-free access by scientists to all of the contributions of their fellow scientists in a conveniently displayed electronic format.*

I think that to formulate it as if realising the full potential of free networked online communication somehow depended on modifying classical quality control IN ANY WAY would be erroneous and would invite misunderstanding. Free, public, self-archiving is a SUPPLEMENT to classical peer review, not a SUBSTITUTE for it. We can have the optimal outcome while keeping classical peer review 100% intact.

Nor is there any reason to talk about changing the way scientists publish their results! The only thing wrong with the way they publish their results is that they are not available to everyone online for free. To achieve that, all they need to do is make them available to everyone online for free! All "current practises" continue: Papers continue to be submitted to the author's refereed journal of choice, as before, they continue to be refereed, as before, and if accepted, they continue to be published in the journal, as before. But IN ADDITION to this, the author self-archives the unrefereed preprint at the beginning of this quality-control process, and, still more important, also the refereed reprints at the end of this quality-control process.

Here is where E-biomed plays its crucial role: in providing a reliable, lasting archive for the author to self-archive in, one that authors can trust, one with the authority and prestige to draw the entire literature to it, and one that will support authors in exercising their right to self-archive.

Unfortunately, the E-biomed proposal is a little vague on some of the critical points, as I will try to show below. These critical points concern the status of the established journals and classical peer review. As for the rest of the above, it is all fine, feasible, and desirable, but merely a SUPPLEMENT to current practises. This should be made crystal clear, for attempting to change current practises, particular along the lines of an untested alternative, can only induce confusion and opposition.

> ***E-BIOMED:***
> *A proposal for E-biomed*
> *In the plan presented here, the National Institutes of Health----through the National Center for Biotechnology Information, a component of the National Library of Medicine at the NIH---would facilitate a community-*

based effort to establish an electronic publishing site, called "E-biomed." It is important to emphasize at the outset that in no sense would the NIH operate as the owner or rule-maker for this enterprise. We are proposing this plan in an effort to accelerate much-needed public discussion of electronic publication in the United States and abroad and to provide the financial, technical, and administrative assistance to initiate such a program.

Here is the question to ask at this point, before misunderstandings accumulate and wrong assumptions and inferences are made:
Track A: Is E-biomed going to be a GLOBAL EPRINT ARCHIVE (like LANL), where authors can self-archive their papers? If so, that is fine, highly desirable, and should receive the highest encouragement. (And it should pool resources, experience and expertise with LANL, which is already supported by NSF/DOE and colossally successful
http://xxx.lanl.gov/blurb/ups.html

as well as with other current archiving initiatives such as the Scholar's Forum

http://library.caltech.edu/publications/scholarsforum

and NCSTRL http://www.ncstrl.org/

Track J: Or is E-biomed instead (or also, which is almost as bad) meant as a rival to the established, peer-reviewed journals -- essentially a RIVAL JOURNAL OR JOURNALS, providing peer review and certification? For if so, you are again needlessly changing two empirical variables at once: (1) free online self-archiving (proven, good) and (2) new online journals, with new, untested forms of quality control (again, risky, and counterproductive, for we already have plenty of journals, and there is no need to cast the new MEDIUM's lot with that of a new journal or journals, competing with the established ones).

E-BIOMED:
In the plan we envision, E-biomed would transmit and maintain, in both permanent on-line and downloaded archives, reports in the many fields that constitute biomedical research, including clinical research, cell and molecular biology, medically-related behavioral research, bioengineering, and other disciplines allied with biology and medicine.

So far this is compatible with public, self-archiving, LANL-style, Track J.

E-BIOMED:
The essential feature of the plan is simplified, instantaneous cost-free

> *access by potential readers to E-biomed's entire content in a manner that permits each reader to pursue his or her own interests as productively as possible. We have attempted to endow the plan with the flexibility necessary for evolution as patterns of use become established and as new opportunities for enriching the system are proposed. And we suggest a mechanism for governance (the E-biomed Governing Board) that involves all of the parties concerned---the scientific community (readers and authors), editors, computer specialists, and funding agencies.*

It is unclear what entities this passage refers to. Deliver a universal Biology Archive of self-archived unrefereed preprints and refereed reprints, LANL style and LANL scale (Track A), and the online frills will take care of themselves. They are not the controversial part. Don't overstructure this, especially on the basis of untested, unproven structures: Deliver the free online literature and everything else will follow suit.

> ***E-BIOMED:***
> *Copyright to reports posted in E-biomed would be retained by the authors, with the provision that intact versions would be freely available for transmission, downloading, and publication. Portions of reports could be reproduced only with the permission of the authors.*

Of course authors should and will retain ownership of the intellectual property they self-archive in E-biomed, just as when they self-archive in LANL, or in CogPrints (the Archive I founded in order to extend the LANL revolution to the Cognitive Sciences -- Psychology, Neuroscience, Biology [!], Linguistics, Computer Science, Philosophy -- all destined for eventual subsumption by LANL, once they attain critical mass).

http://cogprints.soton.ac.uk/

But this all goes without saying. (This report is a combination of substantive and important steps toward achieving the optimal outcome described above, together with solemn statements of the obvious! It might fare better without the latter, I think.)

> ***E-BIOMED:***
> *Scientific reports in the E-biomed repository would be submitted through either of two mechanisms, as described in more detail in the succeeding sections. (i) Many reports would be submitted to editorial boards. These boards could be identical to those that represent current print journals or they might be composed of members of scientific societies or other groups*

> *approved by the E-biomed Governing Board. (ii) Other reports would be posted immediately in the E-biomed repository, prior to any conventional peer review, after passing a simple screen for appropriateness.*

This is the core of the potential problem, and (i) is unfortunately profoundly ambiguous:

If the "boards" are indeed IDENTICAL to those of current print journals, then submitting to E-biomed would be tantamount to submitting to one of those journals, which is perfectly fine, but then this is merely the "overlay" system already being worked out at LANL: One can submit to the American Physical Society (APS) journals by depositing the preprint in the LANL Archive and specifying which journal it is submitted to. The journal then proceeds with the refereeing of the article, as usual (except online, which is faster and more efficient, and is the way all journals are moving anyway).

If, however, the "boards" are not just the current journals (plus any new startups that might fledge), but RIVALs to them, then this proposal is conflating the establishment and encouragement of free public online archives with the establishment of new online journals -- a different proposition altogether, and definitely not one to which the fate of online self-archiving should be linked, for reasons I described in my critique of the CalTech proposal.

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/

In contrast to (i), which concerns submitting to journals (old or new) through the Archive, (ii) simply refers to self-archiving in the archive. The latter is the generic category, however; the rest is just about TAGGING (is this self-archived paper "U," an Unrefereed Preprint, or is it "R," a Refereed Reprint? -- and if the latter, what Journal "X"?). The rest is just about sectoring the Archive: If Journal X has its own overlay, as the APS journals will have in LANL, then the author can submit to it via E-bionet, and if a final draft is accepted, it can receive an authentication tag not only from the author, but from the publisher, certifying that it is indeed the published, final draft. Search engines can then filter with that tag.

But underneath, generically, all we have is (i) self-archiving and tagging of preprints and reprints by the author, and (ii) authorised overlays by the journal publishers (whether the "new" rival journals, or the established ones, but if it is to include the latter, you must work to include them, as the APS was included in LANL: there will be resistance, though, as Floyd Bloom's Science Editorial indicates; NIH, however, can be a great asset in helping to persuade publishers to collaborate rather than compete with the optimal outcome for research and researchers).

http://www.cogsci.soton.ac.uk/~harnad/science.html

> ***E-BIOMED:***
> *(i) Submission to E-biomed through editorial boards*
> *The first of the two mechanisms that authors would use to enter new scientific reports into the E-biomed database is closely aligned with current practice and retains scientific review as a prerequisite to publication. Authors would submit reports electronically to the central server, requesting review by the editorial board of an indicated journal in an appropriate field.*

If this refers to the current established peer reviewed journals, it is a splendid idea, exactly along the lines of the APS/LANL overlay. But if it refers only to "new" journals one hopes to spawn along with the Archive itself, it will only lead to trouble. Submitting for publication through the Archive is only attractive to authors if they can submit to the prestigious, high-impact journals of their choice -- not if it is to new, untested entities.

Work out agreements with a sufficient proportion of established journals, as in the case of APS/LANL, and this will be a highly attractive feature, and will hasten the success of the E-bionet Archive. But provide only the promise of some sort of peer-reviewed publication, and conflate it with the primary goal, which is self-archiving itself, and the only result can be confusion and resistance. This point MUST be clarified.

> ***E-BIOMED:***
> *If, after review, the report is accepted for publication in either its original or a revised form, the edited version would be posted immediately in E-biomed, and its title and list of authors would appear for a fixed period in the current table of contents for that journal. Later, it would continue to be accessible through the E-biomed search engine or through the journal's home page, annotated with the dates of submission, revision, and acceptance.*

This has the same ambiguity as the prior passage. If we are talking about new entities, prospects are bleak. If we are talking about the established journals, then don't you first need their collaboration in this? Will they agree to allowing their authors to self-archive their preprints in the first place? They ought to agree, but currently many explicitly do not (Science, and the New England Journal of Medicine are examples that immediately come to mind). Rather than announce it as a fait accompli, a priori, that established journals will allow their authors to self-archive online preprints and to submit to the journal via the Archive, I suggest you confirm this with a sufficient number of them so that you have a viable and

attractive package to offer prospective self-archivers. (And if not, then drop the option for now.)

And it's not over with the unrefereed PREprint and the submission, for the journals will also have to agree to having the refereed REprint appear publicly for free in E-bionet. Again, they OUGHT to, and I don't doubt that they eventually will, in view of the optimality of the outcome for science and scientists, but we are not there yet, and there are quite a few critical transition points we still need to get through to get there.

The APS have officially granted all their authors the right to self-archive both their unrefereed preprints and their refereed reprints; this is partly because the APS is a very enlightened and progressive Learned Society, with an especially progressive and benign Editor in Chief, Marty Blume; but it is also true that Physics is the discipline which spawned LANL, and LANL is a fait accompli: Hence the whole field was, de facto, archiving all its preprints and reprints online already, well before any official overlay, collaboration, or PERMISSION from APS.

That is why the path of a prior agreement with journals in Biology is a much less sure one than one in which we set aside any promise of offering the capability of submitting to refereed journals through the E-bionet. Authors should simply be encouraged to self-archive all preprints and reprints, as they did in LANL. The rest will take care of itself. Waiting to build in, in advance, what LANL only gained by first SUCCEEDING as a self-archive risks preventing E-biomed from hastening us on the road to the optimal.

> ***E-BIOMED:***
> *If an editorial board judges the report unsuitable for inclusion among its own listings, the authors could resubmit the report for review by another board, defer further attempts to disseminate the findings, or publish in E-biomed through the alternative mechanism described in part (ii).*

This passage is beginning to compound the ambiguity I mentioned above, and to build it into a hypothetical structure with less and less basis in reality.

What is the "editorial board" above? One of the established journals? Then you are simply referring to conventional rejection and submission to another journal. If you mean "new" entities, it is not at all clear what all this is about. So I will assume you mean conventional journals. In that case, this passage is just stating the obvious: Of course, as always, if one journal rejects your paper without requesting revision and resubmission, and you still believe your paper worthy of publication, you submit to another journal. Why restate the obvious in this context? Stick to what is relevant and unique to free online self-archiving: An

unrefereed preprint can always be archived, tagged and accessed as such: an unrefereed preprint, "U".

"Listings" somehow slipped in here too, and again it sounds like some sort of fantasy to the effect that there is more to all this than just self-archiving and journal submission: But journals publish, they do not "list." They publish what their referees and editors judge to be acceptable, and they tag it accordingly. The tag "JX" (eventually authenticated by an official journal overlay but good enough for now if the author so tags it) is all that's needed so that someone wanting to search through (or to point an automating alerting agent at) only the contents of Journal JX can do so, via the Archive.

("Content lists" and "issues" are outmoded papyrocentric concepts, not relevant to the online medium. To make this proposal credible, these should be eliminated, as they only encourage others to think in old and incompatible ways too!)

> ***E-BIOMED:***
> *Electronic publishing provides an opportunity to offer a third outcome to the review process, one that provides a novel solution to one of the most commonly encountered problems in current editorial practice. If a submitted report is deemed by an editorial board to be worthy of attention by some segment of the scientific community, but judged not to meet the criteria set for inclusion among a limited number of prime listings, the editorial board could still accommodate the report by choosing to maintain one or more additional listings. These additional listings might be grouped by specialty or simply designated as a larger, less exclusive version of the primary listing. Authors of reports that meet the criteria set for these listings---which, while less prestigious, still denote review and endorsement by the journal's editorial board --- could then elect immediate posting in E-biomed.*

This begins to become more and more hypothetical. Current established journals' only "listings" are the contents of the issues and volumes of papers they have accepted. They do not have different "levels" of acceptance. There do exist different levels of publication, however, and these correspond to the established hierarchy of journals: They differ in prestige, impact factor, rigour of peer review, and specialty/generality. If we are talking here about established journals, then these distinctions will continue to exist in the Archive, and will be marked by the journal tag, JX, JY, just as they are now, in paper. If a paper is not good enough for journal JX, it can be submitted to JY etc. If it is eventually accepted, that will be its tag, and that will be the journal that "lists" it.

But I am afraid that some "new" kinds of journals are being imagined here,

untested ones, based on probably incoherent notions such as "listings" at different "levels" in the same "journal" by new kinds of "editorial boards."

To get into this is to get into open-ended experimentation with quality control and tagging -- a worthy long-term endeavour in itself, but not relevant to the much bigger and more immediate objective of freeing the literature for one and all online! There, it can only confuse and retard, with armchair notions, when the path to the optimal outcome (see your own first paragraph) is much clearer if unencumbered by these irrelevant side-issues.

> ***E-BIOMED:***
> *(ii) Submission to E-biomed through the general repository*
> *Authors would also have the option of entering scientific reports directly into the E-biomed repository without soliciting endorsement by the one of its editorial boards.*

This is of course one of the primary functions of the archive: Apart from (a) submitting preprints for refereeing directly to journals via an Archive Overlay, and apart from (b) self-archiving already refereed reprints, one can also simply (c) self-archive unrefereed preprints.
That is exactly what it is; that is how it should be portrayed. The importance of these functions, by the way, is exactly the reverse: The most important is (c), for that is what a free, online, refereed literature consists of. The unrefereed preprints (b) are important too, and will speed communication and research in many cases. But the overlay agreements with the journals (a) must await developments, is not critical to the success or functions of the Archive, and certainly must not be waited for (or promised in advance).

> ***E-BIOMED:***
> *Before publication in the database, each report would need to be approved by two individuals with appropriate credentials. These credentials, to be established by the E-biomed Governing Board, should be broad enough to include several thousands of scientists, but stringent enough to provide protection of the database from extraneous or outrageous material. (Such credentials might be membership on any approved editorial board or receipt of a research grant from a reputable funding source. The Governing Board would establish mechanisms to ensure that authors need not personally know two validators in order to have their submissions considered for deposition in E-biomed.)*

This is potentially a bit confusing. Are there to be self-archived, unrefereed preprints, with no one's endorsement, plus self-archived, unrefereed preprints with some specialists' endorsement too? Fine, but why add these arbitrary extra features

a priori? Perhaps people will want extra tags like this in calibrating their online browsing and reading: They might want a restriction that is somewhere between looking only at (1) papers that have been accepted by specific refereed journals, and (3) papers that have been accepted by no journal at all, in the form of (2) papers that have been vetted by an informal set of specialists. (But (2) is only possible on the untested assumption that peers are available to do more refereeing, and more levels of refereeing, than they already do now -- for peer review, at every level, is a scarce resource, and cannot be assumed to be compliant and available.)

But this all seems to be a pig-in-a-poke. We don't know whether peers will do (2); we don't know whether authors will want (2); we don't know whether readers will find (2) useful. Whatever is the case, (2) is a an uncertain extra feature, and certainly not a precondition for Archiving!

(And is all of Biomedical Science really just "several thousands of scientists"?)

> ***E-BIOMED:***
> *Criteria for approval of reports must be sufficiently firm to guard against gross abuse of the E-biomed repository, but sufficiently flexible to permit rapid posting of virtually any legitimate work.*

[Note added later: Paul Ginsparg has explained to me that this level of vetting was meant to be very rudimentary, simply to filter out crank and crackpot deposits. That is certainly a good idea and should be practised by every Global Archive, but especially a Biomedical one, where considerations of public health are involved.]

> ***E-BIOMED:***
> *At any time thereafter, the authors would be free to solicit review and endorsement from a specific editorial board as a means to provide greater prestige and visibility to a paper. Alternatively, interest in such reports could be enhanced by attaching to them informative commentaries written by other investigators.*

UNLESS the above is simply stating the obvious -- which is that papers self-archived in E-biomed as unrefereed preprints can also be submitted for peer review to journals, possibly through Journal overlays in E-biomed itself, and if accepted, can then appear in E-biomed also or instead as refereed reprints -- this again sounds like needless armchair fantasizing (neither necessary nor helpful to the Proposal, in my opinion). It just mixes up what is attainable and important with speculative scenarios that may or may not prove viable and useful some day, but on which nothing now is or ought to be dependent.

[There is an echo here of a naive proposal we here over and over again, that open commentary might somehow substitute for peer review: I would suggest emphasizing that the Archive will contain self-archived commentaries too, both unrefereed and refereed, and that these may be linked to the articles, but don't associate the Proposal any more closely with the quackish idea that spontaneous opinion polls could serve as a basis for calibrating one's reading.]

A word: If the entire preprint and reprint literature were freely available online, MUCH better ways of "publicising" one's work as well as of finding the work of others will evolve. Don't try to constrain it with the weak papyrocentric intuitions we have about this now, when the literature is mostly still on paper, and the little that is online is still behind financial firewalls.

> ***E-BIOMED:***
> *Initially, some authors might hesitate to try this route or might use it only to report information perceived to be difficult to publish in current journals. With experience, however, this mechanism is likely to become commonly employed because of its simplicity, flexibility, and speed; because electronic search engines are much more powerful than visual scanning of tables of contents to find relevant articles; and because other instruments (novel peer review mechanisms, appended commentaries, citation counts, and accession data) can be used to enhance the status and prominence of a report.*

And all these potential powers of the online medium are just as valid without these little speculative variants on peer review: They would be there if we just got the classical preprint and reprint literature online and freely available in E-biomed!

Commentaries are a whole new dimension:

Harnad, S. (1979) Creative disagreement. *The Sciences* 19: 18 - 20.

Harnad, S. (1984d) Commentaries, opinions and the growth of scientific knowledge. *American Psychologist* 39: 1497 - 1498.

Harnad, S. (1998) Learned Inquiry and the Net: The Role of Peer Review, Peer Commentary and Copyright. Learned Publishing 4(11): 283-292
http://citd.scar.utoronto.ca/EPub/talks/Harnad_Snider.html

> ***E-BIOMED:***
> *Open access to scientific reports and assembly of personalized journals E-biomed would allow each user to invent his or her own "virtual" or personalized journal, by downloading the reports he or she would like to read that week.*

This too is an outmoded papyrocentic idea. Searching with the help of tags and links is the online way, along with automatic personalized alerting agents. Journals just become quality control tags; otherwise, they are an outmoded concept.

> ***E-BIOMED:***
> *Improved format for publication of modern biology*
> *Obviously, especially with online searchability and availability of the entire corpus, including citation interlinking.*
> *More rapid dissemination of scientific information*
> *E-biomed would markedly speed up both the review and production processes currently used in scientific publishing.*

Yes, immediate electronic availability speeds up availability itself, and online peer review is faster than paper/mail. But referees' work stacks will not get smaller, and there are still only 24 hours in their days. As it stands, parts of this draft of the proposal would entail using up MORE of the finite pool of scarce referee-hours doing things that were not done in classical peer review. That could actually slow the whole system down, if it really caught on (but there is reason to doubt it would catch on, and no real reason to speculate about it one way or the other, for the core purposes of this proposal).

> ***E-BIOMED:***
> *Moreover, many fewer reports would be sequentially reviewed by more than one editorial board in order to find a publishing outlet;*

It is not at all clear why this proposal would lead to this, rather than the opposite! But I strongly suggest that E-biomed stay out of the peer review business (leave to the experts, who are doing well enough already) and our of peer review reform (leave that to quality control researchers, who can apply their tested findings to improving peer review, once they have some findings!).

> ***E-BIOMED:***
> *Reduced costs*
> *Scientific journals are inherently costly. The price of publication and distribution is presently levied on users in a variety of ways: subscriptions to libraries and individual readers for print and electronic versions; page charges to authors; and the time and labor required to maintain and use libraries. (The expenses currently incurred by institutions have recently been the subject of a much publicized scholarly report---accessible at http://jan.mannlib.cornell.edu/jps/jps.htm---and have even been held responsible for the decline in publication of academic monographs [see "The New Age of the Book" by Robert Darnham in The New York Review of*

Books, pp.5-7, March 18, 1999
http://www.nybooks.com/nyrev/WWWarchdisplay.cgi?19990318005F].)
While our proposal cannot eliminate all of the costs associated with scientific publishing, movement to an electronic format is likely to reduce those costs dramatically (see an essay by Odlyzko for one account [http://www.research.att.com/~amo/doc/competition.cooperation.pdf. The most crucial effect of cost reduction would be the opportunity to remove price as a barrier to individuals seeking any of the vast information deposited in E-biomed. It would also offer savings to individuals, laboratories, institutions, funding agencies, and the editors and publishers who move to electronic formats.

I think this proposal is extremely vague on the subject of cost and cost-recovery. One CAN be much more specific about this subject, but for E-biomed's immediate purposes there is no need at all to be more specific -- in which case one should not claim to have done so:

This proposal is mute on (1) how to make the transition from paper to online-only publication of journals, (2) how to recover the remaining costs of quality control. I have tried to sketch out a way http://www.cogsci.soton.ac.uk/~harnad/nature.html based on switching to cost-recovery from up-front page-charges, but the promotion of universal self-archiving by authors does not require a commitment to any specific transition scenario. It is best, though, not to claim to have helped solve cost-recovery problems that publishers are very likely to energetically dispute!

E-Biomed is offering authors an archive to self-archive their unrefereed and refereed papers. If they use it, this will, inter alia, provide the refereed journal literature for free. Only then will we have to worry about how to restructure journals to keep them afloat. But E-biomed certainly does not have any proposal for this, so it would be much better to drop any mention of a problem for which no solution has been provided.

E-BIOMED:
Other possibilities
E-biomed is designed to evolve in ways that might affect the way we practice science.
In an electronic publishing system, it is possible to engage electively in a more open reviewing process---one in which critiques of the scientific reports are accessible and possibly signed. This development, if widely accepted, could offer many benefits: more responsible reviews, an instructive and ongoing public conversation about published work, and career rewards for useful commentaries about work done by others. These

reviews could be part of the vetting process that awards authors with a place on a table of contents of an E-biomed journal or they could be post-publication reviews appended to entries in the general E-biomed repository.

See references on the differences between Peer Review and Peer Commentary, above. As in the other cases, the latter is a supplement to, not a substitute for, the former. And there is an empirical literature on the role of factors such as anonymity to supplement one's armchair intuitions on such questions. The Proposal does not gain strength and credibility but loses it when it is weighed down with unsupported armchair speculations.
The substantive core of the E-biomed Proposal -- an archive for author self-archiving -- is rock solid, feasible, and has strong empirical support. The speculative reforms are best left for another project, where there is no risk that they will drag down an already seaworthy and urgently needed vessel.

E-BIOMED:
Electronic publication could allow the amendment of reports, permitting authors to transmit additional information that might not warrant a separate report. Versions of reports containing supplementary information would be announced and clearly denoted as such, while the original versions are preserved as a 1.0 file for the historical record and downloaded for safekeeping

Yes, self-archiving includes the possibility of self-archiving of updates and revised new editions of a paper, refereed and unrefereed, and linking to them, as well as to comments and responses (and to papers cited).

E-BIOMED:
The active E-biomed process might be accompanied by a much-needed effort to convert material already published on paper to digital text and image format, with hyper-linked citations. This additional initiative would ultimately allow all users of E-biomed to move seamlessly through the entire body of reported information in biomedical sciences. And it would also enhance scientific productivity and reduce burdens on library facilities.

Very useful and desirable; copyright questions to resolve, however. And unless authors do their own scanning in and OCR for their own old texts, the cost of centralized scanning and digitization could be used as grounds for erecting permanent access-tolls to the retroactive literature, which would be a great pity for all. An outright subsidy would be preferable to reinstituting S/L/P for the pre-E-biomed retrospective corpus.

[Of course living authors can and should scan in and self-archive all their own retrospective work along with self-archiving their current work.]

> ***E-BIOMED:***
> *One further, less tangible benefit might also occur as a natural outcome of shared use of E-biomed: a heightened sense of community among biomedical scientists. This might be conducive to the adoption of uniform standards for sharing the data and providing access to the research tools described in E-biomed.*

Certainly sharing a uniform, universal resource, with shared metadata tags, will help to standardize and make the literature more interoperable.

> ***E-BIOMED:***
> *How do we guarantee equity in the new system?*
> *Although the current system of scientific publishing can be criticized for lapses of fairness, it has, in general, served us well. Thus any new system must be developed with concern for the ambitions of trainees, little-known scientists, or scientists at less prestigious institutions or foreign sites. Clearly, electronic communication has enormous advantages for people in all of these categories, because it is a democratizing force that makes distance and wealth nearly irrelevant. However, it is important to ensure that opportunities to enter reports into E-biomed are just as rich as the opportunities to access the reports filed by others. The editorial boards and the Board of Governors will need to give careful attention to this issue; for instance, it will be imperative to provide a means for any author, however remotely located or poorly known, to have access to two "members" of the system to validate reports submitted to the general repository.*

This seems to be a pseudo-issue, unless the worry is about authors who have no access to the Web at all. If quack-vetting is feasible at all, there is no reason why any worthy paper should be at a disadvantage. Much more relevant and important is the democratization provided by the very existence of the online corpus, free for all. The access barriers were the greatest inequity of all. (Best to put this in context, rather than create needless pseudo-issues.)

> ***E-BIOMED:***
> *How should E-biomed get started?*
> *Does the plan make sense? Is it likely to achieve the benefits we ascribe to it? Are there other (better) ways to achieve them?*

The self-archiving makes eminently good sense, and is a principle that has already been tried and has had resounding success in Physics. Plenty of empirical basis for

extending it to Biology.

The speculations about peer review the vague promises about collaboration with journals, old and new, are on much weaker ground, completely unnecessary, and in my opinion considerably weaken the proposal. The archiving has face validity; the refinements on peer review and the relations with journals are just notional. Best to completely uncouple the former from the latter, rather than dooming them to a shared fate.

> ***E-BIOMED:***
> *How should E-biomed be financed and managed? The NIH is prepared to provide funds and expertise to initiate the project. Should other funding agencies, in the U.S. and abroad, also support it? Or should funds be developed through other mechanisms, such as "submission charges" paid by authors?*

Only the archiving facility needs support (not the refereeing system innovations or the journal structures). This could be partnered with NSF/DOE's LANL Archive, the Scholar's Forum initiative, and the ACM's interoperable Gateway, NCSTRL, which will unify it with local institutional Archiving Initiatives. One foreign partner could be the UK's JISC eLib Electronic Libraries Programme, which is already partnering with NSF and LANL. INSERM in France is contemplating taking such steps too. The objective should be a worldwide, free archive for the current research literature in all disciplines. The first form this should take is an Archive for self-archiving by authors. CogPrints (funded by JISC eLib) has adapted the LANL interface to generalize it to other disciplines. This will be further adapted in conjunction with a new JISC/NSF project to citation-interlink the entire contents of the LANL Archive. NIH would be a welcome partner in all this, and all resources could be shared.

> ***E-BIOMED:***
> *What should be the composition of the E-biomed Governing Board? And how much authority should the Governing Board have over the functions of editorial boards that participate in E-biomed? What responsibilities should the Board have beyond developing rules of operation, producing an annual budget projection, negotiating with groups asking to establish editorial boards, and resolving disputes?*

In my opinion, this is all irrelevant. E-biomed should not get into the editorial-board and peer-review business. It needn't, and that has nothing to do with the goal, which is to make the entire reprint and preprint literature available online for free for all. I strongly counsel you to drop all this refereeing-reform and journal-overseeing if you really want to speed things on the road to the optimal. Otherwise

you will instead become involved interminably in Quixotic reform proposals that have little to do with the goal that motivated the whole undertaking, as described in your own Prologue!

> ***E-BIOMED:***
> *Once these and other questions have been considered, the NIH will publicize an appropriately modified proposal, assemble the Governing Board, and establish the E-biomed site with the Board's guidance.*

I have been at so many meetings, in so many countries, in so many disciplines in the past few years: Almost all of them want to make a "policy statement" about what to do in the "electronic communication revolution"; they want to make recommendations to governments; they want to publicize in the general press. But none of them has a coherent picture or plan. I just dissuaded the Commission of the German Learned Societies from drafting a letter to the Science Minister triumphantly announcing that the way of the future was "National Site Licensing (NSL)," along the lines of BIDS in Britain. (Utter nonsense: NSL is a Trojan Horse, one of the three horseman of the Trade Troika, S/L/P, which are actually responsible for keeping this entire literature -- (the refereed journal literature), unique for being freely donated by its authors -- separated from its readers by a spurious financial firewall.)

Please don't now tell the NIH's governing Board that the way to free the literature is to set up all kinds of new pig-in-poke reviewing and editorial structures, in the hope that editors/referees will collaborate and that authors will prefer them! The real message is simple: Provide the resources and incentive for universal self-archiving of preprints and reprints, as NSF/LANL does with such colossal success and utility, and the rest will take care of itself!

If you want a model, and proof of principle and practise, just copy (or better, collaborate with) LANL!

> ***E-BIOMED:***
> *Summary*
> *The advent of the electronic age and the rise of the Internet offer an unprecedented opportunity to change scientific publishing in ways that could improve on virtually all aspects of the current system. The NIH has addressed this opportunity by proposing a new system, E-biomed, that has many advantages over the existing means of disseminating research findings: open access, greater speed, reduced cost, and enhanced depth of presentation. We now welcome constructive comments from the scientific community, with the intention of putting a suitably revised plan into operation in the near future.*

All these virtues are already there, demonstrated and realised, in LANL. Don't take something known that works astoundingly well, and turn it into something unknown that may not work at all, by getting it mixed up with notional peer review reform, with which it has no intrinsic connection.

Stevan Harnad, University of Southampton, May 28, 1999

On Tue, 18 May 1999, Gene Garfield wrote:

> ***GARFIELD:***
> *2. Peer-reviewed depository - published papers.*
> *Research reports submitted for publication in a Current Science Group journal (irrespective of whether they have been placed previously in the pre-print depository of that or any other journal) will be fully peer-reviewed in the traditional way. The accepted research reports will be placed in the peer-reviewed depository and will be available free of charge to any individual through the web.*

Hi Gene:

Here is the fatal flaw (I think) in this approach: You are setting it up EITHER to compete with the classical journals ("submit there or here"), a competition you are certainly going to lose, OR to "collaborate" with conventional journals ("submit there AND here"), a collaboration you are also going to lose, because classical journals are not interested in publishing what has been published elsewhere, and they are insistent on retaining exclusive copyright over what they themselves have published.

(The latter will change, under pressure from author self-archiving, because the author has all the moral rights here, but it will certainly NOT change under pressure from a competing peer-reviewed journal!)

And, last, you are (wrongly) assuming, in a world where referees are already hard-pressed and doing what they do only because of the golden rule, that there is a large enough set of willing/able peers out there to referee a paper not only ONCE, but TWICE (one set for the classical journal, another set for yours).

> ***GARFIELD:***
> *Vitek Tracz, Chairman of the Current Science Group, said: "We believe science publishing is on the brink of some of its most exciting and far reaching changes.*

It is indeed, but it is important to realistically sort out just exactly what those changes will be, and why they will happen. The core insight is that journal authors, because they want to maximize access to their work, will self-archive it online, freee for all, and the user community will of course prefer that free version. This in turn will change the market for journals, and will force them to resturcture themselves. But this all involves a reform of the classical journals; there is no niche for rivals here unless the classics fail to reform in the face of self-archiving and give up the game to other players. But that's a bit of the way down the road and I doubt that that will be the way it goes.

> ***GARFIELD:***
> *#3 Dear Steve: I have finally had a chance to take a brief look at the Cal Tech proposal and I have been trying to follow your various arguments reported in Sci. Amer.,Nature,etc. The Scientist has an issue coming up shortly concerning publishing and I would like to see a commentary by you, if possible, that could state in 1700 words or less your basic ideas and make very few assumptions about what readers know, even though they are mainly research scientists. You can cut and paste or whatever to make a coherent piece and if you don't have the time to make the deadline we can schedule it in another issue. This topic will not go away and we will have to continue to educate the scientific community.*

I'll be happy to do it: When's the deadline?

> ***GARFIELD:***
> *As you know anything you publish with us will go up on the web free of charge and we would encourage you to include links to appropriate archives,etc. for those who want the full background.*

Fine.

> ***GARFIELD:***
> *I think there will be significant objection to having all this run by Los Alamos and even for that matter NLM were they to adopt the Ginsparg programs. Having it in government hands is problematic.*

That all depends on what you mean by "all this." I am now coming to realize that the idea expressed in my own Subversive Proposal of 93-94, which I then thought was just one hypothesis among many, is turning out to be EXACTLY what will and needs to happen. Self-archiving is the name of the game, not rival journals, or rivals to the journals. So all Los Alamos and NIH and others are providing is the infrastructure for universal self-archiving. In parallel, the universities (Scholar's

Forum) are ALREADY offering it, in the form of authors' local home-institution servers. The Los-Alamos/E-Biomed approach is a global one. But in reality, the result will be a virtual library that seamlessly integrates these two approaches, with one serving as a reliable backup for the other.

On the contrary, governments and big research institutions are PRECISELY the ones that authors know and trust, and will hence entrust their self-archived papers to. They are JUST the ones to provide the infrastructure, especially if it is suitably distributed, so as to assuage any fears of monopolism or imperialism.

> ***GARFIELD:***
> *Have you ever talked to Vitek Tracz? He is in London and I have urged him to contact you. I will try to send you his web site announcing his plans.His compnay is called CURRENT SCIENCE.*

Not yet, but I'd be happy to: What's his email?

> ***GARFIELD:***
> *Your comments on the role of citations are relevant and I wonder if you have seen the search engine developed by NEC of Princeton?? I think it is www.citeseer.com but I will also check that later and make sure you get the right address. They have developed what they call "Autonomous Citation Indexing" which means they take anything that is on the web and create all the citation links and display the citing context. The fellow is named Steve Lawrence, an Englishman I believe.*

I know the project, and know Lee Giles, his collaborator, from my years at Princeton; indeed, we cite their project in our own proposal (which will be formally announced by NSF/JISC next week). We will be in contact with them.

> ***GARFIELD:***
> *I think you somehow have avoided the issue of redundancy. I think you may argue too strongly against the idea of "virtual journals" assuming that profiling will do the trick.*

Not sure what you mean. I am a STRONG advocate of redundancy: backups, mirrors, distributed archiving, local and glabal archiving, interoperability, the lot! I'm also for "virtual journals," in the sense that the final, refereed, accepted paper is redundantly archived publicly, and the role of the virtual journal will have been to peer-review it and, if accepted, to certify it as such with the journal brand name.

What I don't agree with is needlessly trying to compete with the classical brand-name journals now. That will just fail, and so it should. What authors should do

now is NOT to switch from the high-impact, high-quality brands they know and trust; they should stick with them, but subvert too, by self-archiving. THEN let the chips fall where they may.

> ***GARFIELD:***
> *However, I think that you as an editor should know that journals are not just aggregations of closely related material.*

Of course not. And the items in an issue rarely have much to do with one another. But a known, trusted, quality-assurance brand continues to be what authors (and readers) want, and there is no reason why it should not continue to be.

> ***GARFIELD:***
> *If you are talking about invisible colleges then you are probably right.*

College-schmollege: The peers are the college, and each journal (and granting agency) has its own subset of them; that is medium independent.

> ***GARFIELD:***
> *Derek Price use to say that 200 scientists could support a journal, but even that number is too large for the kind of journal you are talking about. You are right in the sense that one can develop a profile as I have with the ASCA system for 35 years(now called Research Alert) but my profile includes dozens of Source journals as a kind of mini-Current Contents. If everything gets poured into one barrel I myself would worry about browsing such a huge collection. And knowing how scientists work I do not think they will maintain their profiles properly without the kind of rubrics that journal titles represent.*

I think you have misinterpreted me on this point: I'm for MAINTAINING the diversity and hierarchy of journals; that's one of the things I was criticizing E-Biomed for, rather than advocating it. But I'm also against trying to COMPETE with the existing journal hierarchy right now, when what we need is subversion (by self-archiving), which we can win, as Los Alamos has proven, rather than competition, which we can only lose.

> ***GARFIELD:***
> *Of course this will have to be tested, but I would urge you not to push that point too strongly, especially in the transition. Perhaps if the system includes a scheme for co-citation and word clustering we can create "virtual" journal contents for browsing.*

I'm not sure what point you're talking about. In the citation proposal, we will

interlink everything in Los Alamos. That does not make it into one big journal, it makes it into a better interlinked ARCHIVE which contains all the brand-name journals, suitably tagged (and free).

> ***GARFIELD:***
> *What has happened to the Links Project at Southampton? How often do you come back to Princeton? Best wishes. Gene*

The ISI links project was taken off-line, at ISI's insistence, after only a few months. A pity, because I would have liked to leave it on-line much longer, to give more people a chance to taste it.

It is understandable that ISI is ambivalent about this, though, for it takes only a little imagination to deduce that if there were a free, full-text archive of the journal literature, the citation linking possibilities and benefits would be much greater than the mere linking of ISI abstracts plus reflists (even if paired with click-through, pay-as-you-go links to journals' full-text proprietary data bases).

Do you see the fundamental rift between for-fee and for-free in this new world?

Stevan Harnad, University of Southampton, May 29, 1999

On Fri, 28 May 1999, Paul Smaglik wrote:

> ***SMAGLIK:***
> *ANALYSIS By Paul Smaglik It's going to be a preprint service. It's going to be a reprint repository. It's going to kill off society journals. It's going to save them. It's going to compete with commercial titles. It's going to complement them. There appears to be no consensus on the effect E-biomed, a potential government-backed electronic publishing service proposed by Harold Varmus, director of the National Institutes of Health, will have on other journals-both paper and electronic. Nor does there appear to be much agreement on what form that service will take. "There's a vagueness in Varmus' proposal," comments Stevan Harnad, professor of cognitive science at Princeton University and the University of Southampton in England.*

But you MUST add: "but once this is resolved, there is a viable core that can have the revolutionary impact of freeing the biomedical journal literature for one and all forever."

> ***SMAGLIK:*** *Varmus, who acknowledges that the proposal is young, calls*

> *that vagueness "evolvability" [see Varmus interview, page X]. While words like "vagueness" (and nonwords like "evolvability") are being applied specifically to E-biomed, they might well serve as accurate labels for electronic publishing as a whole. The field has splintered into a myriad of permutations. E-publishing now includes electronic reprint sites, such as a cognitive science one run by Harnad;*

Paul, to put this in context, you should really say:

E-publishing now includes electronic preprint and reprint sites, such as the remarkably successful Physics Archive at Los Alamos, run by Paul Ginsparg, and its emulators in other disciplines, such as CogPrints, the one in cognitive science run by Harnad;

If you don't put it this way, the statement is neither representative nor informative. There is NO DISTINCTION between preprint and reprint servers: they are for self-archiving by authors, who can put either preprints or reprints in there.

URLs:

http://xxx.lanl.gov/cgi-bin/show_monthly_submissionshttp://cogprints.soton.ac.uk/"

> ***SMAGLIK:*** *E-only journals, such as MedGenMed, run by former JAMA:The Journal of the American Medical Association Editor George Lundberg;*

To emphasize that there is NO INCOMPATIBILITY between running eprint archives and e-only journals, I suggest that you mention Psycoloquy, the e-only journal I have been running since 1990, the first peer-reviewed e-only scientific journal, and, paradoxically, sponsored by the American Psychological Association (APA), the biggest and most prestigious paper-journal publisher in Psychology. This just shows that Learned Societies can be extremely progressive (funding free journals) at the same time as being reflexively regressive (APA has one of the most restrictive copyright policies at the moment, along with Science and the New England Journal of Medicine; in contrast, Nature is more progressive, and the American Physical Society (APS), the APA's counterpart in Physics, has the most progressive copyright policy of all, one that will serve as a model for all the others):

Copyright:http://www.cogsci.soton.ac.uk/~harnad/science.htmlhttp://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

Psycoloquy:http://www.princeton.edu/~harnad/psyc.html

SMAGLIK: *preprint archives, such as a popular physics site sponsored by Los Alamos National Laboratory;*

Drop this in favour of the prior mention above. It is artificial and counterproductive to try to distinguish preprint and reprint servers. The only separating issue is copyright, and as noted, this varies from journal to journal, rather than from preprint to reprint.

SMAGLIK: *and commercial sites by publishers including Elsevier Science and John Wiley & Sons.*

Stress that sites like Los Alamos and CogPrints are for self-archiving by authors and they are FREE, whereas sites like Elsevier's and other journal proprietary archives are for FEE!

SMAGLIK: *Which of these disparate sites will thrive may depend on issues that affect them all. Who deservedly holds the copyright for research articles, what is the tolerance for "tolls" on the Information Highway, and how will the Web change the nature of peer review. Subversive Proposal Harnad suspects-and hopes-that Varmus' plan will eventually resemble the "subversive proposal" Harnad made years ago.*

how about "converge on" rather than "resemble."

Also, I strongly suggest, for completeness, that you also mention the Scholar's Forum, an initiative parallel to the NIH one, and potentially even bigger, because it includes ALL disciplines, not just the biomedical ones, and it includes the top US Universities (and potentially all of them), not just NIH:

http://library.caltech.edu/publications/ScholarsForum/

Don't underestimate this CalTech initiative. I have heard through the grapevine that there is a GREAT deal of muscle behind it, as it is being promoted by the Provosts of the US Universities, the ones most conscious of the huge drain on University budgets represented by learned journals, as well as the huge limitation on the potential impact of University research represented by the access barriers of the Subscription/Site-License/Pay-Per-View (S/L/P) system that they too would like to subvert.

http://www.chronicle.com/free/v45/i04/04a02901.htm

SMAGLIK: *That proposal calls for authors to archive their published, peer-reviewed papers on their own Web sites and give them away for free. Harnad notes that since authors don't get paid for their efforts, "there's no*

reason they shouldn't be able to give their own work away."

Note here that they already DO give it away, and always have done, in the form of reprints that they themselves pay to produce and mail (for free) to those who want them; the Net will just become a big, universal reprint distributor for the author.

> ***SMAGLIK:*** *To have their work mounted, authors-or institutions-could perhaps pay the journal that published it a fee that is less than the yearly subscription rate.*

NO, NO! Please don't attribute this completely counterproductive view to me! Authors have already given their papers to their publishers for free, so that their publishers can sell them. It would be absolutely grotesque that authors should now, like libraries, PAY to buy back their own work so that they themselves can in turn give it away for free! Please think before saying such things!

> ***SMAGLIK:*** *Such an arrangement would reduce the journal's role to peer review and a seal of approval.*

THAT is the service that authors' institutions can and should continue to pay for, but not through access-blocking S/L/P but through direct, up-front payment for this quality-control and certification service, out of only a small portion of the institutional S/L/P savings.

PLEASE get the logic and pragmatics of this straight, otherwise you are simply advocating another variant of S/P/L access-blockage (in which the author's institution is now paying L -- a global site license -- for the "right" to make specific papers of their own accessible to everyone for free: there is no justification whatsoever for that; only the quality control and certification service needs to be paid for!).

> ***SMAGLIK:*** *The drawback? Copyrights. Many journals do not let authors retain copyright. And commercial and society-based journals likely wouldn't voluntarily give up the subscription income that makes them viable. Still, Harnad thinks that may change. He notes that the American Physical Society recently gave copyright control back to authors who submit papers to its journals. "The game is over in physics," Harnad comments. If other societies followed suit, that could pressure commercial publishers to do the same. But that's a big "if," notes Helen Atkins, director of database development at the Institute for Scientific Information (ISI) in Philadelphia. "[Harnad has] been proposing this for a long time. The basic idea he has is very interesting. I don't see anybody doing it."*

APS is somebody; and they are not the only ones: Perhaps you should do a survey

of evolving copyright policy. My bet is that it's moving toward the APS model. Subversion from self-archiving in Los Alamos, CogPrints, E-Biomed and Scholar's Forum will help hasten the process.

ftp://aps.org/pub/jrnls/copy_trnsfr.asc

> ***SMAGLIK:*** *The top biomedical journals are especially protective of copyrights, but if E-biomed becomes enough of a force, that could change. Harnad thinks E-biomed will be more successful as a reprint repository for existing journals than a new, competing one. "We don't need more journals," he concludes.*

And continue: "We need infrastructures that will facilitate self-archiving by authors, as Los Alamos has done in Physics. The rest will follow suit, as it has with the APS."

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

> ***SMAGLIK:*** *E-only Options Lundberg would disagree, although he declined to comment on the need for E-biomed. He is the Northwestern University-based editor of Medscape General Medicine (MedGenMed), a clinical medicine Web site designed for both patients and practitioners; it will publish its first peer-reviewed articles soon. Lundberg hopes the site will trade on the brand name of Medscape, the popular clinical site that launched MedGenMed April 9. The journal will be a curious combination of old and new approaches to publishing. "We don't plan for this journal to be an annual, a quarterly, a monthly, or a weekly. We will publish articles as they are found to be of value. The date of publication will be the day it goes up," Lundberg says.*

This is not news. It is already the policy of hundreds of e-only journals, such as Psycoloquy (and many others).

You are here pitting something fairly humdrum and unenterprising against something revolutionary, as if they were somehow either on a par or alternatives. They are neither. E-only journals are one thing, free archives another.

The only common point is FREE e-only journals: Is Lundberg's free? If not, then it's just S/L/P barriers all over again, in a new medium. If it IS free, then it is probably premature -- like the new author-page-charge based Institute of Physics (IOP) free-only journal, which will, I fear, fail, because the culture is not yet ready for it: E-only journals can't be financed up-front until (1) the community has, and becomes addicted to, the journal literature for free, online, hence (2) S/L/P

cancellations occur, freeing a portion of those savings to pay for (3) up-front charges for quality control.

In other words, till the fields are first softened up by subversion, authors will neither see the point, nor have the institutional support, for up-front expenses; besides, page-charges have a bad reputation today, having added insult to injury as an ADDITIONAL expense, over and above S/L/P tolls, in the paper era; and they still have the smell of vanity press. All this will change, but subversion and its ensuing changes in user culture must come first.

http://amsci-forum.amsci.org/archives/september-forum.html

But the point is that Lundberg vs. E-Biomed is an unbalanced and incoherent opposition.

> ***SMAGLIK:*** *On the other hand, he will avoid the so-called "fluid" peer review with which some E-journals have experimented. "We intend to use a more traditional form of peer review-traditional in the sense of shielding the identity of the reviewers from the authors and readers and not doing open peer review, whereby you put up something ... and get everybody to shoot at it." Lundberg feels that such approaches dilute the authority of a journal.*

These are all platitudes. Most of the new e-only journals (e.g. Psycoloquy, for over TEN years already) use classical peer review, with anonymity, etc. Why parade these platitudes in the same breath as truly new and potentially revolutionary stuff?

> ***SMAGLIK:*** *Hosting such "fluid" documents can actually strengthen an electronic publication, argues Rick Luce, [**TITLE**], at Los Alamos National Laboratory. "One of the things that the medium clearly can do is turn static documents into living documents." That approach may explain why physicists have embraced the Internet as a research tool.*

I assume you are referring here to open peer commentary (as opposed to classical peer review): This too is one of my specialities. The PAPER journal I founded in 1978 (Behavioral and Brain Sciences, BBS, published by Cambridge University Press), specializes in peer commentary. And, yes, the online medium is infinitely better suited to that, and will make a lot more forms of peer commentary -- formal and informal, refereed and unrefereed, on unrefereed preprints and on refereed reprints -- possible and permanent.

http://www.princeton.edu/~harnad/bbs/index.html

But this is a side-issue! Commentary is a supplement, not a substitute for the classical system. And what is at issue here is the classical system, that is, the 14,000 refereed journals presently constituting the journal literature (or the 6500 subset of them covered by ISI). THAT is the literature that subversion could make online and free. Save the frills for another story; let this be about delivering the quality controlled goods such as they are.

http://citd.scar.utoronto.ca/EPub/talks/Harnad_Snider.html

> ***SMAGLIK:*** *The success of the Los Alamos preprint resource may have inspired Varmus to draft his own proposal. However, publishers in the two disciplines have different preprint philosophies. Physicists have used preprints long before the Internet to communicate rough ideas and then, with feedback from others, to shape those preprints into publishable papers.*

True, but this isn't just about preprints any more. Nor does Los Alamos contain only preprints!

> ***SMAGLIK:*** *In biomedicine, on the other hand, some of the most respected journals will not touch an article that has existed as a preprint in any form-sometimes even on a personal Web page. "There are plenty of biomedical publishers who won't accept a paper if it's been mounted anywhere," notes Atkins. But that, too, may change, especially if E-biomed shapes up as Varmus envisions.*

Indeed it will change, as this policy, quite simply, has no justification whatsoever; it is purely self-serving -- with the very minor exception of papers whose unrefereed dissemination might endanger public health: these are a minuscule subset of the biomed literature and can be treated as a special case, as E-Biomed is in a position to do; they are certainly no justification for holding all the rest of the literature hostage to such restrictions; that is done merely out of publisher self-interest: to protect a revenue stream by not allowing themselves to be "scooped." But once the service provided by journals scales down to quality control and certification -- with no question of S/L/P sales to worry about protecting -- this will become the non-issue that it always should have been: Except where public health might be put at risk by premature publicizing, it is no business whatsoever of a journal's whether or not an author has disseminated a preprint of the unrefereed draft.

http://trauma-pages.com/harnad96.htm

I might add that journals also will "not touch" a paper that has already been published in another refereed journal, and in this they are fully justified: For

referees referee for free, and it is an abuse of their services to ask them to referee a paper that has already been refereed and published. But in an online world free of S/L/P barriers there will be no incentive to "re-publish" work that has already been quality controlled and certified by some journal. It is already in the public eye, as accessible as anything else. HERE is where comments and citations from peers can draw attention to a paper that might have appeared in a journal that was lower in the prestige/impact hierarchy than it might have deserved to be. Peer commentary can help correct the oversights of classical peer review, but multiple submission, being an abuse of a scarce resource (referee time) will be as unacceptable with free e-only journals as it is now with S/L/P paper journals. Nor will page charges make it any more acceptable; for referees are, and will remain unpaid: there is not money enough in the world to compensate them for their heroic services, donated gratis to a prestigious journal or granting agency by reason of an academic golden rule.

> ***SMAGLIK:*** *However, E-biomed might have the opposite effect. Large publishers will continue to deny publication of material that appeared as a preprint, will resist giving up copyrights, and will do whatever they can to charge for full-text articles pulled from the Internet. "I think the focus has been 'Protect the revenue stream,'" Luce notes. "If you and I were the journals, we wouldn't want to go along with this," Harnad agrees.*

I agree. But now we come to what I have called the "Faustian Bargain": There is a profound conflict of interest in this, one that is unique to the refereed-journal literature (it is NOT true of books or magazines), and that places research and researchers on one side, and publishers on the other.

http://www.arl.org/scomm/subversive/toc.html

The Faustian Bargain in the past was that all authors assigned copyright to their publishers because that was the only way they could gain the immortality of PUBLICation. This was fine for the trade literature (all books and magazines), because those authors contributed their texts for fee or royalty, and shared in the take from the toll-gate receipts. But this was never true of the refereed journal authors, who wanted only to reach the eyes and minds of their fellow-researchers with the reports of their research findings, so their work could have its full potential impact, and be built upon as broadly as possible.

Yet they too had to assign copyright, because there was no other way to cover the real expenses of paper dissemination. The access-restrictions imposed by the toll-barriers were against their interests, but the only alternative was even worse, namely no access at all.

ftp://ftp.princeton.edu/pub/harnad/Harnad/HTML/harnad90.skywriting.htmlhttp://www.cogsci.soton.ac.uk/~harnad/Papers/Harnad/harnad91.postgutenberg.html

In the online era (what I've called the "PostGutenberg Galaxy of Scholarly Skywriting") this is no longer true. There IS a way of covering the much tinier expenses (of quality control and certification) without the need for any access barriers.

http://www.princeton.edu/~harnad/nature.html

And if, in this newly unveiled conflict of interest -- which could never be resolved in any other way in the Gutenberg era, but now can be -- the publishers insist on continuing to impose the trade model, with its S/L/P-barriers, when it is no longer necessary, then (and only then), the research community is in an excellent position to bolt -- for of course we are not only the authors and the readers, but also the referees and the editors.

I do not believe it will come to this, however, which is why I advocate subversion rather than confrontation or defection. For the copyright laws in this Faustian domain -- where authors don't WANT to be protected from the theft of their own texts! -- is not only unjustifiable, but also unenforceable. Everyone can post a preprint: Will journals be sending virtual agents around trawling for preprints 24 hours a day all over the Net, to compare with all incoming submissions? How alike must 2 texts be to count as the same draft? And once the preprint has been refereed and accepted, will the publishers then trawl for lookalikes on the Web again? How different does the self-archived version have to be from the accepted final version in order to count as just another preprint, rather than a "reprint."

This is all nonsense, of course, because there is not only no logical or practical basis for making such distinctions when the AUTHOR wants to give it away (there are plenty of bases for it when it is another author or publisher who is trying to steal the text-AUTHORSHIP rather than the text, but that's plagiarism, and not what's at issue here), but there is also absolutely no justification for it: It is against the interests of both research and researchers to try to enforce such arbitrary strictures in the PostGutenberg Galaxy -- for THIS special, give-away literature (often publicly funded already, with publication mandated by the funder).

> ***SMAGLIK:*** *Links to Success Large commercial publishers see E-biomed as a threat and a challenge. "As written, the Varmus proposal almost encourages a reduction in the number of journals available to authors," comments Brian D. Crawford, vice president and general manager, life and medical sciences, at John Wiley & Sons of New York. Karen Hunter, senior vice president of development at Elsevier Science, agrees. "If it's intended*

to replace journals, I think that's a concern."

That's just because of the vagueness of the initial draft of the proposal. As soon as it is brought more into focus, and the inessentials and incoherencies are dropped, it will become clear that SELF-ARCHIVING the entire literature is what is at issue, and this entails no reduction whatsoever in the number of journals: It is intended to reconstitute every single one of them (via author self-archiving) online, and for free, thereby ushering in the optimal and inevitable outcome of this process, and encouraging the publishers to restructure themselves so as to continue providing a useful service to this new, smaller niche.

It is the false impression that E-Biomed is trying to spawn a new breed of rival journals that has gotten publishers' hackles up, but this will be remedied in the next draft. The subversion, on the other hand, will continue to be inherent in the project, as it should be, but that is not something against which either a logical or an ethical or even a practical case can be made: It must be tolerated by the publishers, and adapted to. There is neither a means nor a justification for trying to stop it.

> ***SMAGLIK:*** *Both Crawford and Hunter agree with Harnad and Varmus that the publisher's strength is the name and reputation of its journal. Both publishers are trying to boost both, by taking advantage of interactive communication. They're building links to other references, adding sound and animation to Web publications when appropriate, and hosting online discussions-enhancements that, in many cases, first appeared in electronic preprint, reprint, and E-journal-only formats. Crawford and Hunter use the term "value-added" to describe those features.*

Ah me, the "value-added" argument. Here is the quick rebuttal:

ADD-ON enhancements for a fee are just fine. Add them and then try to sell them. But do not try to hold the refereed article hostage to those add-ons: Let a generic, quality-controlled, certified draft be self-archived for free, and then continue to try to persuade the user community that they are better off with an enhanced version, with add-ons, for a fee (S/L/P).

My prediction is that the user community will prefer the free, no-frills version. Then, and only then, will publishers realize that there is no hope of sustaining S/L/P barriers, and will scale down to up-front payment for peer review and certification.

Note, however, that subversion is an end in itself either way: The goal is to free the refereed literature. Self-archiving does that. WHETHER or NOT a parallel S/L/P version proves to be sustainable, the goal will already be attained by

providing the free version.

> ***SMAGLIK:*** *That potential added value could well spring from the competition of the other electronic information sources. And noncommercial sites, such as Stanford University's Highwire Press and Los Alamos' experimental "Library Without Walls" have been adding similar features. Highwire Press, founded in 1995, mounts 127 high-impact science and technology journals. Highwire has for years been adding many of the hyperlinked features that commercial publishers are now exploring. The "Library Without Walls" project lays one search engine over a multitide of databases, including PubMed, ISI's Web of Science and other massive journal repositories.*

This is all highly non-revolutionary stuff: It is merely about driving S/L/P prices down. Subversion is about eliminating them altogether, to produce a completely access-barrier-free literature for one and all. Cheaper S/L/P will solve some researchers' access problems, but freeing the literature will solve everyone's, everywhere. That's a difference between night and day.

> ***SMAGLIK:*** *The competition between the noncommercial sites and the commercial ones will likely increase as a result of the E-biomed controversy. "Some of that conflict, frankly, is healthy," Luce opines. "What I see are two spheres of where papers go or where you might access literature. One I'll call an informal sphere-which would include things like preprints and more informal communication. The other I'll call a more formalized sphere. And that would be where there's very careful peer review." That formal sphere touches both the public and private sectors, because online journal publishing is not now one single thing. And perhaps, E-biomed notwithstanding, it never will be.*

This is all exceedingly murky, and based on the vagueness of the current draft of the E-Biomed proposal. The real categories are these: Free self-archived preprints AND reprints vs S/L/P-toll-based reprints. That's all! It's not preprints vs reprints, informal vs formal, peer review vs peer commentary. That's all just fog and confusion.

Medscape General Medicine (MedGenMed)www.medscape.com/Medscape/GeneralMedicine/journal/public/mgm.journal.html

> ***SMAGLIK:*** *Elsevier Science www.elsevier.com Highwire Press www.highwire.org Los Alamos "Library Without Walls" projectlib-www.lanl.gov/lww/welcome.html Los Alamos Preprint site xxx.lanl.gov*

Stevan Harnad homepage (including links to "Cogprint" cognitive science reprint site; the "subversive proposal"; and discussions and essays about electronic publishing) www.princeton.edu/~harnad Wiley Interscience www.interscience.wiley.com [Q&A HED] Varmus Seeks Societies' Support for Electronic Journal

On May 5 Harold Varmus, director of the National Institutes of Health, unveiled a draft proposal for E-biomed, an electronic publishing system. The plan sketches out several routes for a U.S. government-backed system. It includes provisions for electronic preprints, perhaps resembling the Los Alamos National Laboratory-hosted physics site, as well as original publications, perhaps matching newer E-only journals, such as MedGenMed, or existing print journals' electronic versions. In a recent conversation with News Editor Paul Smaglik, Varmus hinted that he seeks to cooperate with society journals and perhaps compete with commercial ones. The following interview has been edited for length and clarity:

The commercial vs. learned-society, bad-guy vs good-guy dichotomy, is at best simplistic, at worst simply erroneous. The big, successful Nonprofits, whether Learned-Society or University, are virtually indistinguishable from the Commercials in their means, ends, policies and prices. Examples are the American Chemical Society and the American Psychological Association and, for that matter, the American Association for the Advancement of Science (which has one of the most regressive copyright policies).

Yes, the Learneds and Universities are more likely to come round once they smell the subversive coffee, than are the Commercials; but at the moment, it is as little in their immediate interests to do so as it is for the Commercials. So there is no point expecting a priori cooperation. The APS (Physics) is the most progressive, but note that that is only AFTER 8 years of resoundingly successful subversion by Los Alamos!

No: Self-archiving is the way, and not some attempt to separate the good guys from the bad guys and make a deal before there is any de facto pressure to make a deal.

SMAGLIK: *Q: How will E-biomed affect existing publishers?*

A: I think it's too early to say what the impact will be. We have to distinguish among the various kinds of publishers. We proposed this to stimulate discussion and to move toward developing a system of publishing that does at least three things: take advantage of the flexibility in electronic publishing; move toward providing full, unfettered, and seamless access to

the entire biomedical literature; and [create] a system that has the ingredients for evolvability in various directions. Things are changing very rapidly, and it's important that we have a system in which peer review and copyright holding and different means of raising money can evolve along with the mechanics of the system. We're obviously concerned about how much this methodology will cost and how it will be paid for.

We have to distinguish between two kinds of publishers: publishers that are private, profitmaking organizations and those run by societies that represent the interests of thousands of scientists. We've been working largely with some of the societies to discuss some of these issues, and we are certainly aware of and concerned about the possibility that revenues could decline for scientific societies. I think we have to have a culture change here that may take a while to develop; that is, people who are potential members in scientific societies-scientists like me-have to be reminded or taught that societies do a lot more for their members than simply give them cheaper access to a few journals. Societies lobby effectively for the concerns of scientists and many political and cultural venues. They run important meetings, they worry about the future of minority, female, or young scientists, and they advocate usefully on behalf of all those folks. They develop a sense of community within a scientific discipline. If any individual society's journals were no longer purchasable, but instead just available on E-biomed or some similar site, it would not be an issue of deprivation. Indeed, the societies should be showing their members that they are participating in a new wave of more useful dissemination and presentation of information. No one is being deprived of anything by the absence of a cut rate on a weighty journal that can now be accessed much more easily through the Web.

I don't think it's all about lowering S/L/P prices. It's about no longer holding the report of research hostage to S/L/P at all (not even in the interests of supporting learned societies' other "good works": their best work would be to free the research literature of needless and obsolete access barriers at last, to the eternal benefit of research and researchers).

SMAGLIK: *Q: How important is the issue of who holds the copyright?*

A: Some people, I think, overemphasize that. I think it's significant, and I would prefer to see authors hold copyrights-they've written their proposals that way. But it would not bother me if we had editorial boards that participated in E-biomed who wanted to try to hold copyrights while others didn't. I think that's where evolvability has a major role. People who feel strongly-there are probably a lot of them-will say, "I'll submit my electronic

publication to an editorial board that has high visibility and is highly respected and also allows me to retain my copyright." And others might say "I don't really care" and may go with another board that wants to hold their copyright. We'll have to see how important that really is.

Alas, this is still part of the vagueness of the first draft. People will continue doing EXACTLY what they have done till now, which is submitting to the highest quality/impact journals in their subject area. Copyright is critical ONLY inasmuch as it attempts to block free self-archiving. THOSE are the substantive issues.

E-Biomed is not and will not be a journal or journals. It will be a free archive for self-archived preprints and reprints, with the possibility of official "overlays," in which the paper is authenticated by the journal itself. That is merely a matter of tagging and encryption. There is still equivocation to be carefully resolved here about just what E-Biomed itself is meant to be and to deliver. I am suggesting that the coherent core is a reliable, permanent, useable infrastructure for self-archiving by authors, with the option of authenticated overlays by journals, if and when they are ready for it (as the APS is already with Los Alamos).

SMAGLIK: *Q: How closely will the final structure resemble the Los Alamos model?*

A: That's been the preprint system. Our proposal ... is built in a way that would allow our community to either have that system or to have a very high proportion of postings be reports that have been reviewed, edited, and stratified by traditional hierarchies. I think the way our culture works now-given its size and the number of publications-we're going to remain in the camp of reviewing and identifying journals with different status.

Still far too vague on the critical essentials: Los Alamos is NOT just a preprint archive. It is a SELF-archive, hence authors can put in whatever they like, preprint or reprint, and they do.

So THAT barrier has already been crossed. Exactly the same should and will be true of E-Biomed. Authors can, as a first approximation, self-tag their refereed journal reprints as such. That's good enough for subversion. Once the user community is addicted to E-Biomed as the locus classicus for the journal literature, instead of the S/L/P corpus, then "official" authentication overlays will come onto the horizon, but not before, or a priori, for (apart from newborn journals, which are irrelevant), for established journals such an a priori arrangement would be to shoot themselves in the foot before they had even had a chance to make alternate arrangements to restructure for the free Skywriting era.

SMAGLIK: *Q: What about cultural differences between the physics*

community and the biomedical community?

A: [Physics] has a hundred authors writing one paper and they don't publish as much; the need to stratify the literature by hierarchy and status is less of a problem. But there are some things that my lab does and I'm sure other labs do that might not ordinarily constitute a publication. But it's useful information if I can deposit it somewhere. It could be a conversation, a posting, say, that my other colleagues who work on wnt genes might want to see; I could never put it into a reviewed manuscript with space constraints. Nevertheless, it might be useful. And rather than put this into my own Web page, which everybody would have to consult one by one, it would go into a central repository, which a search engine could pick up.

Yes, yes, the preprint sector will hold many treasures. But the essence of it all is the reprint sector, which will free the entire biomedical journal literature.

SMAGLIK: *Q: What do you make of journal publishers' arguments that their major asset is their status, their seal of approval?*

A: I agree with that. I think people have gotten the idea that because we're proposing alternative routes, we don't value editing-I spent a lot of time as a scientist editing and reviewing. And I believe that's useful. I do think that, in the last few years, because there have been so many manuscripts submitted and because there's been such a tight view of the hierarchy, that people spend an awful lot of time revising papers, sending them to different journals. This is a very inefficient process, which we ought to be able to make more effective. I think all of us have a lot of quite significant papers that spend a year bouncing around and undergoing fairly minor corrections before our colleagues can see them. I don't like that. I'd like to see my colleagues' work earlier and I'd like them to see mine sooner. I think we all can envision ways in which the process could be speeded up in electronic format.

Again, too vague. Self-archiving unrefereed preprints solves part of this, and is highly desirable and commendable. But there is no need to speak about any of the problems of peer review here -- either its quality, its efficiency or its timing. There ARE such problems, to be sure, but they are not to be conflated with the project at hand, which is to free the peer-reviewed literature (such as it is!) for one and all.

There's room for projects to improve peer review, speed it up, and what have you. But let us not LINK the fate of the clearcut and eminently desirable goal of freeing the literature with the more hypothetical and conditional one of trying to improve peer review. Such issues should be disentangled completely from the plans for E-

Biomed or they will simply raise needless opposition from the defenders of classical peer review, or worse, will make the prospects of a free literature -- already highly desirable a priori -- depend on the prospects of various peer review reform schemes, schemes which may or may not prove successful (and certainly require a good deal of prior empirical testing before being implemented at all, let alone implemented en masse).

Stevan Harnad, University of Southampton, May 30, 1999

> Dear Stevan,> as guest editor for Brain and Language, the manuscript for> a special issue has been sent off to Academic Press and I> guess is in production. I wondered whether I could advice> the contributors of the special issue to post their articles> in the cog archives. AP has a paragraph about "Personal> Servers" in the Notice section at the end of B&L saying> "When an Academic Press journal accepts the work for > publication, the authors may post it, in its final accepted form, > on their personal servers (but not on any organized preprint > server) with a notice _Accepted for publication_ in ....etc.> After publication, authors may post their Academic Press> copyrighted material on their own servers without permission,> provided ....."> (Now they don't talk about "organized preprint servers"> any more.>

> I know that some of my contributors cannot post their > articles on personal servers. Are the cog archives> considered as a "organized preprint server"?>

> You may understand that I do not want to encourage my> authors to something that puts them into trouble. But> I would like to support free electronic availability> of articles.> Have you had any experience with AP on these matters?>

> I know you must be awfully busy with all your engagement> in on- and off-line publishing, so this is not an urgent> message.>

> best wishes,> Brigitte Stemmer

Dear Brigitte, sorry for the long delay: Overload. The answer is that the personal/public server distinction is completely incoherent and hence untenable: Every "personal" server is public -- reachable by anyone on the Web, indexed by all search engines, duplicated in countless public cache sites, mirrored, etc.; in addition, any "public" server in which the author archives himself, password-protected, and can add/delete as he pleases, is "personal."

So publishers who try to make and enforce this non-distinction are just playing

word games in trying (desperately, and doomed to lose) to hold onto something that cannot be held onto, logically, morally, or practically. (The real distinction they are trying to re-create here, but cannot, is the distinction between privately distributing one's own offprints vs. publishing them with another publisher, but of course that does not fit the new situation at all!)

So you can decide for yourself whether you want to collaborate with AP, and reinforce this incoherent, illogical distinction, which will have the effect of deterring some authors for a few more years, through ignorance and timidity on their part, or you whether you prefer to take a step for good sense and progress and what is indisputably infinitely better for research and researchers and will prevail sooner or later, by telling them that they have the right to self-archive online as they please, their own papers, for which they never received or requested a penny from AP! The only thing that should/can be forbidden is (1) to sell them, or (2) to publish them with someone else who sells them.

For a model of the copyright agreement of the near future, see the self-archiving policy of the American Phsyical Society, publisher of the most prestigious and highest impact journals in Physics:

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

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Stevan Harnad, University of Southampton, May 30, 1999

> Christophe Pallier wrote:> > Dan Ellis posted your advertisement for Cogprints on the AUDITORY list.> Two of us had an argument about the idea that authors should make their> work freely available on the net. I include part of the exchange at the> end of this message.> > This led me to the 2 following ideas:> > 1. If not already done, one could maintain a web page containing a list> of the journals which accept free dissemination of papers on the web.> (I can volunteer to create and maintain such a page). We would> recommend authors to send their papers to these journals. But how many> of these journals are there?

A natural idea, and a benign one, but it is doomed to fail, and so it should. Authors will (and should) continue to prefer to submit their papers to the highest quality, most prestigious and highest-impact journal for which it is eligible. Hence a black-list is likely to have very little effect (though it will do no harm and might do a little good).

The much better and simpler thing to do, which will succeed, and could succeed very quickly, is simply to encourage all authors to publicly self-archive all their

papers (both unrefereed preprints AND refereed reprints). The attempt to block self-archiving is so completely in conflict with the interests of research and researchers, and so unenforceable, that it is certainly doomed to fail -- and has already failed in Physics, because of a de facto "class action" (in the form of massive self-archiving) that is irreversible, and has now led to the most progressive and enlightened copyright policy of all on the part of the American Physical Society, publisher of the most prestigious and highest impact journals in Physics, a model for all future learned journal copyright policies:

http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

> 2. Why not try to write a generic copyright notice for scientific work,> in the same vein as the GNU Public License from the Free Software> Foundation (www.fsf.org).> It could one or two paragraphs, stating that the work we want to> publish must be made freely available to reproduce by anybody. We could> then, as authors, insist on having these paragraphs inserted in the> copyright transfer agreements we sign with publishers.> I don't expect this to work easily, but hey, why not try? We coudl try> to launch a campaign like the one for "free speech".

APS are already well on the way to providing this model. As a start, see:

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

> The best would be if some publishers endorsed the idea of free> scientiific work, and have the authors pay the copyediting & formatting> job.

That is indeed the target, but the hope is that these will be the SAME publishers that now publish the established journals, but restructured for this new online world.

http://www.cogsci.soton.ac.uk/~harnad/nature.html

> A compromise may be to leave the exclusivity of publication to the> publishers for a short period, say one year.

Absolutely not! What nonsense! What researcher would or should agree to a needless one-year embargo on research findings, particularly in the critical initial year! Don't under any circumstances ever accept a Trojan Horse like that.

> This may be very naive. You have a lot more experience than me with> issues.

What do you think we can do as authors and reviewers?

It is indeed naive, though well-intentioned. See further comments below.

> Christophe Pallier> http://www.ehess.fr/centres/lscp/persons/pallier/> > ----------
----------> Christophe Pallier wrote:>> Pierre Divenyi wrote:>>
How beautiful and Platonic an idea: an electronic preprint archive where> everybody could post his/her new opus within minutes, to be read by tens of> thousands of pairs of interested eyes!

Don't confuse preprints with reprints. Unrefereed preprints can be posted at once; but refereed reprints will still first have to undergo peer review, which can be accelerated a little online, but will continue to be a retardant for as long as referees (donating their services graciously and gratis in accordance with the academic golden rule -- for there is not enough money in the world to compensate them for their heroic services, so don't even think of that) have other things to do with their time besides instantly evaluating every one of your papers and mine ("they" are, after all, US).

> Unfortunately, as long as our own mainstream auditory journals oppose> on-line dissemination of pre-publications, and enforce their opposition> through automatic rejection of papers disseminated this way, and as long as> our mainstream granting agencies insist on peer-reviewed publications as> representing the major (if not the sole) proof of scientific productivity,> Professor Harnad is putting the cart before the horses. Moreover, even a> cursory visit at the web sites he suggests makes it clear that, should an> unsuspecting colleague except his offer and post his/her paper on the> preprint archive, he/she may shoot him/herself in both feet at once.

Don't be so fatalistic. Look instead at the empirical data. No feet were shot in Physics, where the game is now over:

http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

> This kind of short-term, individualistic rationale distresses me.> > If we all shoot, then the bullets may not reach our feet but,> hopefully, the heart (or rather the wallet) of the sharks of scientific> publishing.

Don't demonize the publishers. You would do the same in their shoes.

They will only scale down to what is optimal and inevitable for research and researchers when they clearly feel that they have to, and for that, WE researchers have first to realize what is optimal and inevitable for us, and act accordingly. Class action, in the form of universal self-archiving, will accomplish both goals: to

free our journal literature and to send our publishers the message that they must restructure themselves to accommodate it.

http://www.arl.org/scomm/subversive/toc.html

> True: If only a small proportion of us follows Harnad's lead, we might> end up in troubles...> It is a case of the well-know prisoners' game: if we cooperate, we all> win (maybe less than if we play alone), but if we don't cooperate,> some, maybe the majority, will lose a lot.

There was no prisoner's dilemma in Physics. See URL below. Moreover, we now have the advantage of the precedent of Physics already in place. They are, after all, kin of ours, hence part of the "class action."

http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

> It doesn't take a complex demonstration to be convinced that now that> the cost of publishing scientific results has dramatically dropped, the> existence of publishers who charge huge prices and prevent widespread> dissemination of the papers, is an anomaly.

It is not their existence that is an anomaly, but the continuing needless expenses. Subversion through self-archiving will bring this into line with reality. The demand for paid paper journals will not vanish at once (no significant cancellations have yet been detectable in Physics, though they will no doubt come, eventually); there will be time for rational restructuring; but meanwhile the free online literature will already be there for us all.

http://amsci-forum.amsci.org/archives/september-forum.html

> It seems obvious that a very small amount of the funds that are devoted> to research could be invested in paying the few people needed to> maintain scientific electronic journals, which content could be> accessed freely by anyone. I am not against private enterprise and> indeed, this job might very well be done by private publishers, if they> can offer a better service than public agencies. (Why not have the> source, that is the author(s) pay a reasonable amount to have the paper> published. The price would pay for the few hours (or less) of work> needed to format the paper for electronic publication, and maintain> servers).

There is no need for new entities to take this service over from the established journals; they have the experience and expertise; they need only restructure for the new circumstances, which are indeed likely to entail scaling down to online-only, and selling, instead of the journal itself (which will be archived free for all), only the service of implementing peer review and certification:

http://www.cogsci.soton.ac.uk/~harnad/nature2.html

> Note that in this scheme, there is of course no reason why the> reviewing process should be any different for these journals than for> the ones we currently have.

Correct.

> I am unsure whether this will ever happen: current publishers don't> want to lose the goose with the golden eggs, and are fighting hard to> prevent this from happening. Rather, some of them try to install a kind> a pay-per-view system. This makes me sick...

The cost-recovery model that publishers are attempting to retain is Subscription/Site-License/Pay-Per-View (S/L/P). I have dubbed S/L/P the "trade troika," because all three are predicated on access-barriers, because they are selling a product, the article/journal, rather than a service, the quality-control/certification. Up-front payment for this service makes most sense, because it frees the literature from toll-barriers. The author-institution, instead of subsidizing the literature by a huge S/L/P expenditure to buy it back, instead pays for it up-front, out of only a small portion of its own S/L/P savings!

So there is no need to look for outside subsidy (except initially, during the transition period). Because the cost of implementing quality control alone will be so much lower than the current costs of doing it all, the S/L/P savings themselves will be enough to cover the up-front costs with plenty left over to spend on essential things (such as books, which definitely do NOT fall under this nontrade model, because books-authors, like book-publishers, want fees or royalties from the toll-gate receipts, whereas with the journal literature this is not, and never has been, the case).

> At this point, we have the choice between two attitudes:> > 1) an egoistic attitude: putting our career before our scientific> ideals, and not caring about this issue: just compete to publish in> the "best" journals.> The tax-payers will pay the costs, and what's the problem if our> colleagues can't access the information?...> > 2) a responsible attitude:> - refuse to submit or review papers in journals handled by publishers> that refuse to allow free access to the papers (either on the authors'> web site or on their own).> - fight to convince journals editors to change publishers: why a> journal couldn't move to cogprints? The journal may consists of a web> page with links to the accepted papers.

There is another option, which is having your cake and eating it too: Continue to submit to your established journal of choice, but self-archive as well. This subversive path has been followed, with astounding success, in Physics:

http://xxx.lanl.gov/cgi-bin/show_weekly_graph

We need not keep debating it all, meanwhile falling prey to Xeno's Paradox. We need only stride ahead and self-archive. The infrastructure for it is in place (at all our home institutions) and more is on the way:

http://www.nih.gov/welcome/director/ebiomed/ebiomed.htm

http://library.caltech.edu/publications/ScholarsForum/

> If, as an author, this is impractical right now, the correct stance is> to *disobey inane copyright laws* (and convince your peers to do so):> publish accepted papers on servers like cogprints.> It is only if the scientific community does this massively that we have> a chance to prevent the pay-per-view system to win.

Correct. But don't be so sure you are disobeying laws either. There are massive untested and unreflective ambiguities and vaguenesses here: There is absolutely no law about self-archiving unrefereed preprints (only arbitrary and unenforceable policies on the part of some journal publishers), and the law about self-archiving of refereed reprints has a slippery slope with respect to the versions: How many changes in my unrefereed preprint constitutes stepping over the line and making it into a refereed reprint? Besides, authors need not and should not sign away their self-archiving rights; here too a class action is in order.

The critical factor (and everyone keeps forgetting this) is that copyright law is intended to JOINTLY protect the publisher and the author from theft of text. This is fine for royalty- fee-based books and magazines. But where the author wants to GIVE the text away rather than to sell it, it becomes a very different ball-game...

http://www.cogsci.soton.ac.uk/~harnad/science.html

> They are people who try to make a better world happen. Why not take the> example of the Free Software Foundation (www.fsf.org) to create a Free> Science Foundation?> > Christophe Pallier> > Response>> Cher Christophe,> > You have completely missed the point of my comment to Stevan Harnad's> suggestion on electronic publication. The main reason is that you see> the situation from your own environment which, thanks to a slew of> built-in protection for academics and researchers that you benefit from> in France as well as in the majority of Western European countries,> makes the question of whether to publish or not by-and-large optional.

No such thing. Publishing in rigorous refereed journals is critical everywhere in the active scientific/scholarly world. France cares just as much for "impact factors" as the UK or US do.

> Let me tell you that in the U.S. it is not. In other words, you are> quite pampered-spoiled by our standards. Maybe you should also be> informed that the majority of American contributors to the auditory> list is able to do research through the sole support of government> agencies that adhere to the publication policies I outlined in my note.> Thus, you should not try to admonish those of us for whom there is no> alternative but to adhere to these policies. If you want the policies> to change, address your criticism to the agencies.

Funding agencies mandate only that the research findings be published (in reputable refereed journals) -- not that those journals should black access to them in return for refereeing and certifying them. On the contrary, there is a strong move toward retention of self-archiving rights:

http://www.cogsci.soton.ac.uk/~harnad/science.html

> For your information, personally I would be quite in favor of having> all publications available electronically. My private opinion, however,> weighs very little in this matter which amounts to fighting windmills> stronger than even Stevan Harnad's personal opinion: according to what> I gathered from the information on the web pages he wanted us to see,> his many years of effort, alas, have accomplished very little. The> establishment is strong and you guys in France are unlikely to be able> to export a second French Revolution to conquer it. > > Pierre Divenyi

Courage, chers cocombatants! The battle has been won in Physics, and all that's needed to carry this on to the rest of the disciplines is to emulate what the Physicists did (thanks to Paul Ginsparg, to whom all power and glory!). Just self-archive, and the rest will take care of itself.

Stevan Harnad, University of Southampton, June 6, 1999

These are my comments and corrections on a June 7 article in The Scientistby Paul Smaglik about the NIH E-biomed Proposal. (The corrections were not made.) -- Stevan Harnad

URL for the article in question:

http://www.the-scientist.library.upenn.edu/yr1999/june/smaglik_p1_990607.html

> SCIENCE PUBLISHING EVOLVES: TANGLED IN THE WEB > By Paul Smaglik> > It's going to be a preprint service. It's going to be a reprint> repository. It's going to kill off society journals. It's going to save> them. It's going

to compete with commercial titles. It's going to> complement them. > > There appears to be no consensus on the effect E-biomed, a> potential government-backed electronic publishing service proposed by> Harold Varmus, director of the National Institutes of Health, will have> on other journals-both paper and electronic. Nor does there appear to be> much agreement on what form that service will take. "There's a vagueness> in Varmus' proposal," comments Stevan Harnad, professor of cognitive> science at Princeton University and the University of Southampton in> England.

Paul, you MUST add:

"but once this vagueness is resolved, there is a viable core that can have the revolutionary impact of freeing the biomedical journal literature for one and all forever."

Otherwise this does not reflect my own view on this, as we discussed in your phone interview.

Note also that I am no longer at Princeton, but at Southampton.

> Varmus, who acknowledges that the proposal is young, calls that> vagueness "evolvability" [see Varmus interview, page X]. While words> like "vagueness" (and nonwords like "evolvability") are being applied> specifically to E-biomed, they might well serve as accurate labels for> electronic publishing as a whole. The field has splintered into a myriad> of permutations. E-publishing now includes electronic reprint sites,> such as a cognitive science one run by Harnad;

Paul, to put this in context, you should really say:

"E-publishing now includes electronic preprint and reprint sites, such as the remarkably successful Physics Archive at Los Alamos, run by Paul Ginsparg, and its emulators in other disciplines, such as CogPrints, in cognitive science, run by Harnad;"

If you don't put it this way, the statement is neither representative nor informative. There is NO DISTINCTION between preprint and reprint servers: they are for self-archiving by authors, who can put either preprints or reprints in there.

URLs:

http://xxx.lanl.gov/cgi-bin/show_monthly_submissionshttp://cogprints.soton.ac.uk/

> E-only journals, such as> MedGenMed, run by former JAMA:The Journal of the

American Medical> Association Editor George Lundberg;

To emphasize that there is NO INCOMPATIBILITY between running eprint archives and e-only journals, I suggest that you mention Psycoloquy, the e-only journal I have been running since 1990, the first peer-reviewed e-only scientific journal, and, paradoxically, sponsored by the American Psychological Association (APA), the biggest and most prestigious paper-journal publisher in Psychology. This just shows that Learned Societies can be extremely progressive (funding free journals) at the same time as being reflexively regressive (APA has one of the most restrictive copyright policies at the moment, along with Science and the New England Journal of Medicine; in contrast, Nature is more progressive, and the American Physical Society (APS), the APA's counterpart in Physics, has the most progressive copyright policy of all, one that will serve as a model for all the others):

Copyright:http://www.cogsci.soton.ac.uk/~harnad/science.html
http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

Psycoloquy:http://www.princeton.edu/~harnad/psyc.html

> preprint archives, such as a popular> physics site sponsored by Los Alamos National Laboratory;

Drop this in favour of the prior mention above. It is artificial and counterproductive to try to distinguish preprint and reprint servers. The only separating issue is copyright, and as noted, this varies from journal to journal, rather than from preprint to reprint.

> and commercial> sites by publishers including Elsevier Science and John Wiley & Sons.

Stress that sites like Los Alamos and CogPrints are for self-archiving by authors and they are FREE, whereas sites like Elsevier's and other journal proprietary archives are for FEE!

> Which of these disparate sites will thrive may depend on issues that> affect them all. Who deservedly holds the copyright for research> articles, what is the tolerance for "tolls" on the Information Highway,> and how will the Web change the nature of peer review.

Don't conflate the special case of S/L/P access barriers to the refereed journal literature with the more general and irrelevant issue of people's willingness to pay for things on the Net or Web

> Subversive Proposal> Harnad suspects-and hopes-that Varmus' plan will eventually> resemble the "subversive proposal" Harnad made years ago.

how about "converge on" rather than "resemble."

Also, I strongly suggest, for completeness, that you likewise mention the Scholar's Forum, an initiative parallel to the NIH one, and potentially even bigger, because it includes ALL disciplines, not just the biomedical ones, and it comprises the top US Universities (and potentially all of them), not just NIH:

http://library.caltech.edu/publications/ScholarsForum/Don't underestimate this CalTech initiative. I have heard through the grapevine that there is a GREAT deal of muscle behind it, as it is being promoted by the Provosts of the US Universities, the ones most conscious of the huge drain on University budgets represented by learned journals, as well as the huge limitation on the potential impact of University research represented by the access barriers of the Subscription/Site-License/Pay-Per-View (S/L/P) system that they too would like to subvert.

http://www.chronicle.com/free/v45/i04/04a02901.htm

> That proposal> calls for authors to archive their published, peer-reviewed papers on> their own Web sites and give them away for free. Harnad notes that since> authors don't get paid for their efforts, "there's no reason they> shouldn't be able to give their own work away."

Note here that they already DO give it away, and always have done, in the form of reprints that they themselves pay to produce and mail (for free) to those who want them; the Net will effectively just become a big, universal reprint distributor for the author.

> To have their work> mounted, authors-or institutions-could perhaps pay the journal that> published it a fee that is less than the yearly subscription rate.

NO, NO! Please don't attribute this completely counterproductive view to me! Authors have already given their papers to their publishers for free, so that their publishers can sell them. It would be absolutely grotesque that authors should now, like libraries, PAY to buy back their own work so that they themselves can in turn give it away for free! Please think before saying such things!

> Such> an arrangement would reduce the journal's role to peer review and a seal> of approval.

THAT is indeed the service that authors' institutions can and should continue to pay for, but not through access-blocking S/L/P but through direct, up-front

payment for this quality-control and certification service, out of only a small portion of the institutional S/L/P savings.

PLEASE get the logic and pragmatics of up-front payment straight, otherwise you are simply advocating another variant of S/P/L access-blockage (in which the author's institution is now paying L -- a global site license -- for the "right" to make specific papers of their own accessible to everyone for free: there is no justification whatsoever for that; only the quality control and certification service needs to be paid for!).

> The drawback? Copyrights. Many journals do not let authors> retain copyright. And commercial and society-based journals likely> wouldn't voluntarily give up the subscription income that makes them> viable. Still, Harnad thinks that may change. He notes that the American> Physical Society recently gave copyright control back to authors who> submit papers to its journals. "The game is over in physics," Harnad> comments. If other societies followed suit, that could pressure> commercial publishers to do the same. But that's a big "if," notes Helen> Atkins, director of database development at the Institute for Scientific> Information (ISI) in Philadelphia. "[Harnad has] been proposing this for> a long time. The basic idea he has is very interesting. I don't see> anybody doing it."

APS is somebody; and they are not the only ones: Perhaps you should do a survey of evolving copyright policy. My bet is that it's moving toward the APS model. Subversion from self-archiving in Los Alamos, CogPrints, E-Biomed and Scholar's Forum will help hasten the process.

ftp://aps.org/pub/jrnls/copy_trnsfr.asc

> The top biomedical journals are especially protective> of copyrights, but if E-biomed becomes enough of a force, that could> change. Harnad thinks E-biomed will be more successful as a reprint> repository for existing journals than a new, competing one. "We don't> need more journals," he concludes.

And continue: "We need infrastructures that will facilitate self-archiving by authors, as Los Alamos has done in Physics. The rest will follow suit, as it has with the APS."

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

> E-only Options> Lundberg would disagree, although he declined to comment on the> need for E-biomed. He is the Northwestern University-based editor of> Medscape General Medicine (MedGenMed), a clinical medicine Web site> designed for both patients and practitioners; it will publish its first> peer-reviewed

articles soon. Lundberg hopes the site will trade on the> brand name of Medscape, the popular clinical site that launched> MedGenMed April 9. The journal will be a curious combination of old and> new approaches to publishing.> "We don't plan for this journal to be an annual, a quarterly, a> monthly, or a weekly. We will publish articles as they are found to be> of value. The date of publication will be the day it goes up," Lundberg> says.

This is not news. It is already the policy of hundreds of e-only journals, such as Psycoloquy (and many others).

You are here pitting something fairly humdrum and unenterprising against something revolutionary, as if they were somehow either on a par or alternatives. They are neither. E-only journals are one thing, free archives another.

The only common point is FREE e-only journals: Is Lundberg's free? If not, then it's just S/L/P barriers all over again, in a new medium. If it IS free, then (unless subsidized) it is probably premature -- as is the new author-page-charge based Institute of Physics (IOP) free-only journal, which will, I fear, fail, because the culture is not yet ready for it: E-only journals can't be financed up-front until (1) the community has, and becomes addicted to, the journal literature for free, online, hence (2) S/L/P cancellations occur, freeing a portion of those savings to pay for (3) up-front charges for quality control.

http://www.herts.ac.uk/lis/subjects/natsci/ejournal/iopjnl.htm

In other words, till the fields are first softened up by subversion through self-archiving, authors will neither see the point, nor have the institutional support, for up-front expenses; besides, page-charges have a bad reputation today, having added insult to injury as an ADDITIONAL expense, over and above S/L/P tolls, in the paper era; and they still have the smell of vanity press. All this will change, but subversion and its ensuing changes in user culture must come first.

http://amsci-forum.amsci.org/archives/september-forum.html

But the point is that MedGenMed vs. E-Biomed is an unbalanced and incoherent opposition.

> On the other hand, he will avoid the so-called "fluid" peer review> with which some E-journals have experimented. "We intend to use a more> traditional form of peer review-traditional in the sense of shielding> the identity of the reviewers from the authors and readers and not doing> open peer review, whereby you put up something ... and get everybody to> shoot at it." Lundberg feels that such approaches dilute the authority> of a journal.

These are all platitudes. Most of the new e-only journals (e.g. Psycoloquy, for over TEN years already) use classical peer review, with anonymity, etc. Why parade these platitudes in the same breath as truly new and potentially revolutionary stuff?

> Hosting such "fluid" documents can actually strengthen an> electronic publication, argues Rick Luce, [**TITLE**], at Los Alamos> National Laboratory. "One of the things that the medium clearly can do> is turn static documents into living documents." That approach may> explain why physicists have embraced the Internet as a research tool.

I assume you are referring here to open peer commentary (as opposed to classical peer review) and how it can help in the evolution of papers along the Scholarly Skywriting Continuum:

ftp://ftp.princeton.edu/pub/harnad/Harnad/HTML/harnad90.skywriting.html

This too is happens to be one of my specialities. The PAPER journal I founded in 1978 (Behavioral and Brain Sciences, BBS, published by Cambridge University Press), specializes in peer commentary.

http://www.princeton.edu/~harnad/bbs/index.html

And, yes, the online medium is infinitely better suited to that, and will make a lot more forms of peer commentary -- formal and informal, refereed and unrefereed, on unrefereed preprints and on refereed reprints -- possible and permanent. And it will contribute to updates and upgrades of papers, both before and after the officially certified version.

But this is a side-issue! Commentary is a supplement to, not a substitute for the classical system. And what is at issue here is the classical system, that is, the 14,000 refereed journals presently constituting the journal literature (or the 6500 subset of them covered by ISI). THAT is the literature that subversion could make online and free. Save the frills for another story; let this be about delivering the quality controlled, officially certified goods such as they are.

http://citd.scar.utoronto.ca/EPub/talks/Harnad_Snider.html

> The success of the Los Alamos preprint resource may have inspired Varmus> to draft his own proposal. However, publishers in the two disciplines> have different preprint philosophies. Physicists have used preprints> long before the Internet to communicate rough ideas and then, with> feedback from others, to shape those preprints into publishable papers.

True, but this isn't just about preprints any more. Nor does Los Alamos contain only preprints!

> In biomedicine, on the other hand, some of the most respected journals> will not touch an article that has existed as a preprint in any> form-sometimes even on a personal Web page. "There are plenty of> biomedical publishers who won't accept a paper if it's been mounted> anywhere," notes Atkins. But that, too, may change, especially if> E-biomed shapes up as Varmus envisions.

Indeed it will change, as this policy, quite simply, has no justification whatsoever; it is purely self-serving -- with the very minor exception of papers whose unrefereed dissemination might endanger public health: but these are a minuscule subset of the biomed literature and can be treated as a special case, as E-Biomed is in a position to do; they are certainly no justification for holding all the rest of the literature hostage to such restrictions; that is done merely out of publisher self-interest: to protect a revenue stream by not allowing themselves to be "scooped."

But once the service provided by journals scales down to quality control and certification -- with no question of S/L/P sales to worry about protecting -- this will become the non-issue that it always should have been: Except where public health might be put at risk by premature publicizing, it is no business whatsoever of a journal's whether or not an author has disseminated a preprint of the unrefereed draft.

http://trauma-pages.com/harnad96.htm

I might add that journals also will "not touch" a paper that has already been published in another refereed journal, and in this they are fully justified: For referees referee for free, and it is an abuse of their services to ask them to referee a paper that has already been refereed and published. But in an online world free of S/L/P barriers there will be no incentive to "re-publish" work that has already been quality controlled and officially certified by some journal. It is already in the public eye, as accessible as anything else. HERE is where comments and citations from peers can draw attention to a paper that might have appeared in a journal that was lower in the prestige/impact hierarchy than it might have deserved to be.

Peer commentary can help correct the oversights of classical peer review, but multiple submission, being an abuse of a scarce resource (referee time) will be as unacceptable with free e-only journals as it is now with S/L/P paper journals. Nor will page charges make it any more acceptable; for referees are, and will remain unpaid: there is not money enough in the world to compensate them for their heroic services, donated gratis to a prestigious journal or granting agency by reason of an academic golden rule.

> However, E-biomed might have the opposite effect. Large> publishers will continue to deny publication of material that appeared> as a preprint, will resist giving up copyrights, and will do whatever> they can to charge for full-text articles pulled from the Internet. "I> think the focus has been 'Protect the revenue stream,'" Luce notes. "If> you and I were the journals, we wouldn't want to go along with this,"> Harnad agrees.

I agree. But now we come to what I have called the "Faustian Bargain": There is a profound conflict of interest in this, one that is unique to the refereed-journal literature (it is NOT true of books or magazines), and that places research and researchers on one side, and publishers on the other.

http://www.arl.org/scomm/subversive/toc.html

The Faustian Bargain in the past was that all authors transferred copyright to their publishers because that was the only way they could gain the immortality of PUBLICation. This was fine for the trade literature (all books and magazines), because those authors contributed their texts for fee or royalty, and shared in the take from the toll-gate receipts. But this was never true of the refereed journal authors, who wanted only to reach the eyes and minds of their fellow-researchers with the reports of their research findings, so their work could have its full potential impact, and be built upon as broadly as possible.

Yet they too had to transfer copyright, because there was no other way to cover the real expenses of paper dissemination. The access-restrictions imposed by the toll-barriers were against their interests, but the only alternative was even worse, namely, no access at all.

ftp://ftp.princeton.edu/pub/harnad/Harnad/HTML/harnad90.skywriting.htmlhttp://www.cogsci.soton.ac.uk/~harnad/Papers/Harnad/harnad91.postgutenberg.html

In today's online era (what I've called the "PostGutenberg Galaxy of Scholarly Skywriting") this is no longer true. There IS a way of covering the much tinier expenses (of quality control and certification) without the need for any access barriers.

http://www.princeton.edu/~harnad/nature.html

And if, in this newly unveiled Faustian conflict of interest -- which could never be resolved in any other way in the Gutenberg era, but now can be -- the publishers insist on continuing to impose the trade model, with its S/L/P-barriers, when it is no longer either necessary or useful, then (and only then), the research community is in an excellent position to bolt -- for of course we are not only the authors and the readers, but also the referees and the editors (the quality controllers and

certifiers).

I do not believe it will come to this, however, which is why I advocate subversion rather than confrontation or defection. For the copyright laws in this Faustian domain -- where authors don't WANT to be protected from the theft of their own texts! -- is not only unjustifiable, but also unenforceable. Everyone can post a preprint: Will journals be sending virtual agents around trawling for preprints 24 hours a day all over the Net, to compare with all incoming submissions? How alike must 2 texts be to count as the same draft? And once the preprint has been refereed and accepted, will the publishers then trawl for lookalikes on the Web again? How different does the self-archived version have to be from the accepted final version in order to count as just another preprint, rather than a "reprint."

This is all nonsense, of course, because there is not only no logical or practical basis for making such distinctions when the AUTHOR wants to give it away (there are plenty of bases for it when it is another author or publisher who is trying to steal the text-AUTHORSHIP rather than the text, but that's plagiarism, and not what's at issue here), but there is also absolutely no justification for it: It is against the interests of both research and researchers to try to enforce such arbitrary strictures in the PostGutenberg Galaxy -- for THIS special, give-away literature (often publicly funded already, with publication mandated by the funder).

> Links to Success> Large commercial publishers see E-biomed as a threat and a> challenge. "As written, the Varmus proposal almost encourages a> reduction in the number of journals available to authors," comments> Brian D. Crawford, vice president and general manager, life and medical> sciences, at John Wiley & Sons of New York. Karen Hunter, senior vice> president of development at Elsevier Science, agrees. "If it's intended> to replace journals, I think that's a concern."

That's just because of the vagueness of the initial draft of the proposal. As soon as it is brought more into focus, and the inessentials and incoherencies are dropped, it will become clear that SELF-ARCHIVING the entire literature is what is at issue, and this entails no reduction whatsoever in the number of journals: It is intended to reconstitute every single one of them (via author self-archiving) online, and for free, thereby ushering in the optimal and inevitable outcome of this process, and encouraging the publishers to restructure themselves so as to continue providing a useful service to this new, smaller niche.

It is the false impression that E-Biomed is trying to spawn a new breed of rival journals that has gotten publishers' hackles up, but this will be remedied in the next draft. The subversion, on the other hand, will continue to be inherent in the project, as it should be, but that is not something against which either a logical or an ethical or even a practical case can be made: It must be tolerated by the

publishers, and adapted to, as it has been by the APS. There is neither a means nor a justification for trying to stop it.

> Both Crawford and Hunter> agree with Harnad and Varmus that the publisher's strength is the name> and reputation of its journal. Both publishers are trying to boost both,> by taking advantage of interactive communication. They're building links> to other references, adding sound and animation to Web publications when> appropriate, and hosting online discussions-enhancements that, in many> cases, first appeared in electronic preprint, reprint, and> E-journal-only formats. Crawford and Hunter use the term "value-added"> to describe those features.

Ah me, the "value-added" argument! Here is the quick rebuttal:

ADD-ON enhancements for a fee are just fine. Add them and then try to sell them. But do not try to hold the refereed article HOSTAGE to those add-ons: Let a generic, quality-controlled, certified draft be self-archived for free, and then continue to try to persuade the user community that they are better off with an enhanced version, with add-ons, for a fee (S/L/P).

My prediction is that the user community will prefer the free, no-frills version. Then, and only then, will publishers realize that there is no hope of sustaining S/L/P barriers, and they will scale down to up-front payment for peer review and certification.

Note, however, that subversion is an end in itself either way: The goal is to free the refereed literature. Self-archiving does that. WHETHER or NOT a parallel S/L/P version proves to be sustainable, the goal will already be attained by providing the free version.

> That potential added value could well spring from the> competition of the other electronic information sources. And> noncommercial sites, such as Stanford University's Highwire Press and> Los Alamos' experimental "Library Without Walls" have been adding> similar features. Highwire Press, founded in 1995, mounts 127> high-impact science and technology journals. Highwire has for years been> adding many of the hyperlinked features that commercial publishers are> now exploring. The "Library Without Walls" project lays one search> engine over a multitide of databases, including PubMed, ISI's Web of> Science and other massive journal repositories.

This is all highly non-revolutionary stuff: It is merely about driving S/L/P prices down. Subversion is about eliminating them altogether, to produce a completely access-barrier-free literature for one and all. Cheaper S/L/P will solve SOME researchers' access problems, but freeing the literature will solve EVERYONE'S, EVERYWHERE. That's a difference between night and day.

> The competition between the noncommercial sites and the> commercial ones will likely increase as a result of the E-biomed> controversy. "Some of that conflict, frankly, is healthy," Luce opines.> "What I see are two spheres of where papers go or where you might access> literature. One I'll call an informal sphere- which would include things> like preprints and more informal communication. The other I'll call a> more formalized sphere. And that would be where there's very careful> peer review." That formal sphere touches both the public and private> sectors, because online journal publishing is not now one single thing.> And perhaps, E-biomed notwithstanding, it never will be.

This is all exceedingly murky, and based on the vagueness of the current draft of the E-Biomed proposal. The real categories are these: (1) Free self-archived preprints AND reprints vs. (2) S/L/P-toll-based reprints. That's all! It's not preprints vs reprints, informal vs formal, peer review vs peer commentary. That's all just fog and confusion.

> Medscape General Medicine (MedGenMed)> www.medscape.com/Medscape/GeneralMedicine/journal/public/mgm.journal.html> > Elsevier Science> www.elsevier.com > > Highwire Press> www.highwire.org > > Los Alamos "Library Without Walls" project>lib-www.lanl.gov/lww/welcome.html > > Los Alamos Preprint site> xxx.lanl.gov> > Stevan Harnad homepage> (including links to "Cogprint" cognitive science reprint site; the> "subversive proposal"; and discussions and essays about electronic> publishing)> www.princeton.edu/~harnad> > Wiley Interscience>www.interscience.wiley.com> > [Q&A HED]> Varmus Seeks Societies' Support for Electronic Journal> > On May 5 Harold Varmus, director of the National Institutes of Health,> unveiled a draft proposal for E-biomed, an electronic publishing system.> The plan sketches out several routes for a U.S. government-backed> system. It includes provisions for electronic preprints, perhaps> resembling the Los Alamos National Laboratory-hosted physics site, as> well as original publications, perhaps matching newer E-only journals,> such as MedGenMed, or existing print journals' electronic versions. In a> recent conversation with News Editor Paul Smaglik, Varmus hinted that he> seeks to cooperate with society journals and perhaps compete with> commercial ones. The following interview has been edited for length and> clarity:

The commercial vs. learned-society, bad-guy vs good-guy dichotomy, is at best simplistic, at worst simply erroneous. The big, successful Nonprofits, whether Learned-Society or University, are virtually indistinguishable from the Commercials in their means, ends, policies and prices. Examples are the American Chemical Society and the American Psychological Association and, for that matter, the American Association for the Advancement of Science (which has one of the most regressive copyright policies).

Yes, the Learneds and Universities are more likely to come round once they smell the subversive coffee, than are the Commercials; but at the moment, it is as little in their immediate interests to do so as it is for the Commercials. So there is no point expecting a priori cooperation. The APS (Physics) is the most progressive, but note that that is only AFTER 8 years of resoundingly successful subversion by Los Alamos!

No: Self-archiving is the way, and not some attempt to separate the good guys from the bad guys and make a deal before there is any de facto pressure to make a deal.

> Q: How will E-biomed affect existing publishers?> A: I think it's too early to say what the impact will be. We have to> distinguish among the various kinds of publishers. We proposed this to> stimulate discussion and to move toward developing a system of> publishing that does at least three things: take advantage of the> flexibility in electronic publishing; move toward providing full,> unfettered, and seamless access to the entire biomedical literature; and> [create] a system that has the ingredients for evolvability in various> directions. Things are changing very rapidly, and it's important that we> have a system in which peer review and copyright holding and different> means of raising money can evolve along with the mechanics of the> system. We're obviously concerned about how much this methodology will> cost and how it will be paid for. >> We have to distinguish between two kinds of publishers: publishers that> are private, profitmaking organizations and those run by societies that> represent the interests of thousands of scientists. We've been working> largely with some of the societies to discuss some of these issues, and> we are certainly aware of and concerned about the possibility that> revenues could decline for scientific societies. I think we have to have> a culture change here that may take a while to develop; that is, people> who are potential members in scientific societies-scientists like> me-have to be reminded or taught that societies do a lot more for their> members than simply give them cheaper access to a few journals.> Societies lobby effectively for the concerns of scientists and many> political and cultural venues. They run important meetings, they worry> about the future of minority, female, or young scientists, and they> advocate usefully on behalf of all those folks. They develop a sense of> community within a scientific discipline. If any individual society's> journals were no longer purchasable, but instead just available on> E-biomed or some similar site, it would not be an issue of deprivation.> Indeed, the societies should be showing their members that they are> participating in a new wave of more useful dissemination and> presentation of information. No one is being deprived of anything by the> absence of a cut rate on a weighty journal that can now be accessed much> more easily through the Web.

I don't think it's all about lowering S/L/P prices. It's about no longer holding the

report of research hostage to S/L/P at all (not even in the interests of supporting learned societies' other "good works": their best work would be to free the research literature of needless, research-inimical and obsolete access barriers at last, to the eternal benefit of research and researchers).

> Q: How important is the issue of who holds the copyright? > A: Some people, I think, overemphasize that. I think it's significant,> and I would prefer to see authors hold copyrights-they've written their> proposals that way. But it would not bother me if we had editorial> boards that participated in E-biomed who wanted to try to hold> copyrights while others didn't. I think that's where evolvability has a> major role. People who feel strongly-there are probably a lot of> them-will say, "I'll submit my electronic publication to an editorial> board that has high visibility and is highly respected and also allows> me to retain my copyright." And others might say "I don't really care"> and may go with another board that wants to hold their copyright. We'll> have to see how important that really is.

Alas, this is still part of the vagueness of the first draft. People will continue doing EXACTLY what they have done till now, which is submitting to the highest quality/impact journals in their subject area. Copyright is critical ONLY inasmuch as it attempts to block free self-archiving. THOSE are the substantive issues.

E-Biomed is not and will not be a journal or journals. It will be a free archive for self-archived preprints and reprints, with the possibility of official "overlays," in which the paper is authenticated by the journal itself. That is merely a matter of tagging and encryption. There is still equivocation to be carefully resolved here about just what E-Biomed itself is meant to be and to deliver. I am suggesting that the coherent core is a reliable, permanent, useable infrastructure for self-archiving by authors, with the option of authenticated overlays by journals, if and when they are ready for it (as the APS is already doing with Los Alamos).

> Q: How closely will the final structure resemble the Los Alamos model?> A: That's been the preprint system. Our proposal ... is built in a way> that would allow our community to either have that system or to have a> very high proportion of postings be reports that have been reviewed,> edited, and stratified by traditional hierarchies. I think the way our> culture works now-given its size and the number of publications-we're> going to remain in the camp of reviewing and identifying journals with> different status.

Still far too vague on the critical essentials: Los Alamos is NOT just a preprint archive. It is a SELF-archive, hence authors can put in whatever they like, preprint or reprint, and they do.

So THAT barrier has already been crossed. Exactly the same should and will be

true of E-Biomed. Authors can, as a first approximation, self-tag their refereed journal reprints as such. That's good enough for subversion. Once the user community is addicted to E-Biomed as the locus classicus for the journal literature, instead of the S/L/P corpus, then "official" authentication overlays will come onto the horizon, but not before, or a priori, for (apart from newborn journals, which are irrelevant), for established journals such an a priori arrangement would be to shoot themselves in the foot before they had even had a chance to make alternate arrangements to restructure for the free Skywriting era.

> Q: What about cultural differences between the physics community and the> biomedical community? > A: [Physics] has a hundred authors writing one paper and they don't> publish as much; the need to stratify the literature by hierarchy and> status is less of a problem. But there are some things that my lab does> and I'm sure other labs do that might not ordinarily constitute a> publication. But it's useful information if I can deposit it somewhere.> It could be a conversation, a posting, say, that my other colleagues who> work on wnt genes might want to see; I could never put it into a> reviewed manuscript with space constraints. Nevertheless, it might be> useful. And rather than put this into my own Web page, which everybody> would have to consult one by one, it would go into a central repository,> which a search engine could pick up.

Yes, yes, the preprint sector will hold many treasures. But the essence of it all is the reprint sector, which will free the entire biomedical journal literature.

> Q: What do you make of journal publishers' arguments that their major> asset is their status, their seal of approval?> A: I agree with that. I think people have gotten the idea that because> we're proposing alternative routes, we don't value editing-I spent a lot> of time as a scientist editing and reviewing. And I believe that's> useful. I do think that, in the last few years, because there have been> so many manuscripts submitted and because there's been such a tight view> of the hierarchy, that people spend an awful lot of time revising> papers, sending them to different journals. This is a very inefficient> process, which we ought to be able to make more effective. I think all> of us have a lot of quite significant papers that spend a year bouncing> around and undergoing fairly minor corrections before our colleagues can> see them. I don't like that. I'd like to see my colleagues' work earlier> and I'd like them to see mine sooner. I think we all can envision ways> in which the process could be speeded up in electronic format.

Again, too vague. Self-archiving unrefereed preprints solves part of this, and is highly desirable and commendable. But there is no need to speak about any of the problems of peer review here -- either its quality, its efficiency or its timing. There ARE such problems, to be sure, but they are not to be conflated with the project at hand, which is to free the peer-reviewed literature (such as it is!) for one and all.

There's room for projects to improve peer review, speed it up, and what have you. But let us not LINK the fate of the clearcut and eminently desirable goal of freeing the literature with the more hypothetical and conditional one of trying to improve peer review. Such issues should be disentangled completely from the plans for E-Biomed or they will simply raise needless opposition from the defenders of classical peer review, or worse, will make the prospects of a free literature -- already highly desirable a priori -- depend on the prospects of various peer review reform schemes, schemes which may or may not prove successful (and certainly require a good deal of prior empirical testing before being implemented at all, let alone implemented en masse).

Self-archiving, in contrast, has face-validity, and has already been shown to be readily feasible and astoundingly successful by Los Alamos.

Stevan Harnad, University of Southampton, June 8, 1999

From: Stevan Harnad To: Declan Butler Subject: Re: Latest on E-Biomed from *Nature*

Comments on:

Butler, D. NIH plan brings global electronic journal a step nearer reality. *Nature*, 1999 APR 29, V398 N6730:735.

Declan,

Here is some feedback. You are doing fine if you wish to be a passive conduit of the opinions that are being voiced willy-nilly. But if you want to exercise some reflective judgment over the spectrum of reactions, you might consider some of the following:

> Axel Kahn, editor of *Médecine et Sciences*, the leading French-language> biomedical journal, says the proposal challenges the 'naked emperor' of> scientific publishing -- that "80 to 90 per cent of what is published is of> little real interest". Publish or perish "rather than intrinsic merit" has> become "the principal justification for much of the output," he says.> > Kahn claims that most journals are infrequently consulted, and that E-Biomed> would "allow you to have access to the articles you want, without having to> browse hundreds of journals". A shake-out of the journals system is long> overdue, he says, adding that there is only a real need for the cream of> journals, and in particular the best multidisciplinary journals.

Kahn's point about most of the literature being neither read nor cited is correct. It has been made by many others, including, prominently, Stephen Lock, former editor of the British Medical Journal:

Harnad, S. (1986) Policing the Paper Chase. (Review of S. Lock, A difficult balance: Peer review in biomedical publication.) *Nature* 322: 24 - 5.

But that point has absolutely nothing to do with online archiving or free archiving! An online archive accessible to all would be just as welcome if its contents were 80% chaff or if they were 100% wheat. Either way, the online access would make it more useful and navigable. If you want to consult only the "cream," set your browser to search only what is tagged "cream."

(Nor is the Ebiomed Proposal really claiming to solve the problem of the chaff/wheat ratio, which is a problem WITHIN-journals as much as it is between-, and there is no way for ANYONE to tag THAT for a browser. Moreover, human performance being what it is, we must expect a bell curve in every population.)

> One major concern is that the proposal could harm the best existing> journals, without accelerating improvements that might gradually occur in> any case as a result of market forces and more diffuse web efforts by> not-for-profit science publishers. Several observers say it might create an> unhealthy monopoly, erode the diversity of existing journals, and reduce> competition between journals for the best papers.

Have you looked at the proposal? How is it to create a monopoly without the collaboration of the journals? By competing with them? But that would just be a big attempt to found new journals -- and it would fail. (And if it had succeeded it would not have been a monopoly, but a successful rival journal(s) bid!)

And in any case it has nothing to do with the heart of the proposal, which is to provide the journal literature online for free.

> The Varmus proposal notes that the current journal structure has served the> biomedical community well for 300 years. "So the first question I ask is, if> it has served us well for 300 years, why change?" says Martin Frank,> executive director of the American Physiological Society, which publishes 14> journals and 36,000 pages of articles each year.

Because this structure, which has served us well for 300 years, and will continue to serve us well for 300 more, would serve us all even better if it were available (unchanged) online for free.

That is obvious to any reader of the proposal: What is the advantage of repeating

non sequiturs?

> "It [E-Biomed] is extremely cumbersome and is not going to be easily> implemented," says Frank. "It is so unclear in terms of process that it's> going fall under its own weight."

Based on its source, it is evident that this has a large dose of wishful thinking. For if one simply drops from the Proposal the needless parts I criticized in my comments, one is left with something as easily implemented and as sure of success as LANL.

> Frank and other non-profit publishers are irritated at what they claim has> been their omission from early discussions of the proposal, even though it> intimately affects them. Thirty non-profit publishers wrote to Varmus on 29> March, as word of the proposal began to spread, asking him for a meeting to> discuss the plan.Varmus points out that a series of meetings is being> scheduled with organizations worldwide such as the European Molecular> Biology Laboratory (see box opposite), but that these will take some time to> arrange and conduct. He also intends to post the proposal on his NIH web> page for comment.

Open dialogue is always preferable to subterfuge, although based on the depth of reflection in the feedback that you report here, a wider dialogue is going to generate far more chaff than wheat.

> Some observers note that the page charges collected by some publishers> provide them with a cash cow -- and that in the United States the NIH is one> of the largest that is milked. Under the existing system, page charges can> be passed on to the biomedical agency by investigators that it supports. The> potential loss of this lucrative system is alarming publishers -- especially> non-profit organizations --which rely heavily on it.

This is based on such a convoluted misunderstanding that it takes one's breath away:

There are three current senses of "page charges," and this passage completely conflates them: One of these is (1) page charges charged to authors in paper journals today. There are few of these, and they are dying out. The other is (2) the charges for paper reprints of one's journal articles. These exist, but are not a cash cow either. The only real cash cow is the (3) charge for the pages of the journals themselves, paid through Subscription/Site-License/Pay-Per-View (S/L/P).

Then there are my own proposals for (4) page charges for quality control, once all papers are available online for free.

Now how do these map onto the message of the above paragraph? It is not the Reprint cash cow that is at risk, but the S/L/P cash cow!

> Proposals that E-Biomed should coordinate peer review of its contents are> controversial. Noorman argues that centralization of peer review would> threaten the diversity of schools of thought provided for by journals.

The peer review component of the E-biomed Proposal was inchoate and incoherent in that draft; but it was also inessential, and the Proposal will look just fine once it's dropped.

> This concern is shared by many scientists and learned societies, who feel> that a centralized structure may obscure the well-defined hierarchy of best> science provided by journals, and that scientists may be more reluctant to> give their time and energy free to a central structure.

Correct, but it's not at all clear from the Proposal that that was what was intended. In the next draft that will no doubt be remedied. But the core idea of a free online journal literature as a centralized resource will remain, and that is what it is all about.

> Andrew Odlysko, a mathematician at the AT&T telecoms corporation and an> expert on scholarly publishing, argues that it would be simpler to separate> the distribution and peer-review functions of the repository, as is done at> the Los Alamos physics e-print servers, where peer review is provided by> journal 'overlays' to unrefereed papers.

Correct, and the first sensible remark adduced in this article so far.

(It was also the gist of my own critique of the Proposal, as you well know, and Andrew wrote what he wrote in response to my critique. I am not carping about being left out, by the way; just about the needlessly low wheat to chaff ratio.)

> Lynn Dobrunz, a postdoctoral neurobiologist at the Salk Institute in San> Diego, asks: "Would E-biomed be in addition to the current system of> journals, or instead of it?

Reasonable question, so far.

> If there was a consolidated site that published> online versions of all the articles that are currently published... that> would be fantastic.

Indeed, especially if it was not just consolidated online, but free.

Online (and consolidated, by a click-through monopoly) is already on the way from journal publishers anyway.

> If it's instead of, and especially if it has this> non-peer-reviewed track to it, I think that is a much less good idea."This is now a postdoc's uninformed opinion to the effect that:

(1) "I think preprints should not be archived." (Fine, don't archive yours; but 100,000 in LANL, for example, have a different opinion),

(2) "I think Online-Only will not be enough. Keep the paper corpus (and keep paying for it)." 5000 hard-pressed libraries may have another opinion; so might the 100,000 physicists who no longer use the paper version).

> The Varmus proposal suggests that scientific societies could be one source> of peer review. But the societies are worried that E-Biomed may undermine> the journal revenues on which many of their other activities, such as> fellowships and meetings, depend.

True. But then the thoughtful question is: Are those other activities worth holding the journal literature hostage to? For how much longer?

> The head of one society says he is open to change, but would need guarantees> that revenues would be preserved. Given such guarantees, societies might> consider joining the initiative, he says. "E-Biomed will only fly if learned> societies and their journals can be brought to the table," predicts Tony> Delamothe, web editor of the British Medical Journal, and a supporter of> E-Biomed's goals.

Or, if all Biomedical authors self-archive all their refereed reprints (and their unrefereed preprints, if they wish) in it.

> Another broader threat, expressed by many scientists, is that NIH might come> to dominate much of the biomedical literature, leading to homogenization or> to discrimination against scientists from smaller countries. "Who would> select the governing body?" asks an official at one European scientific> society. "Who would select the editors and decide what is allowed to be> published? Who will determine costs and access rights?"

Confusion caused by the current draft. This will no doubt be sorted out soon. Classical, pluralistic peer review will continue as before, and the Archive will simply be the free repository of its products (along with the pre-peer-reviewed drafts).

> Many are also uncomfortable with the prospect of public funding for> scientific publishing, an activity currently dominated by for-profit and> non-profit publishers in the private sector. At the same time, however,> there is growing resentment among scientists and librarians at the> spiralling inflation in journal subscriptions.

Journal publishing is currently subsidized completely by S/L/P funds from public and private institutional libraries. Networks and Archives (including LANL) are also subsidized by public and private institutions. The latter will simply be taking over the load from the former, saving a great deal of money, and providing the product for everyone for free.

> Competition between scientific publishers is less than in other industries> because of distortions in the market, and profit margins as high as 40 per> cent are not uncommon (see *Nature* 397, 195-200; 1999).

And inelastic demand from the libraries that have to keep subsidizing them for lack of an alternative. Ebiomed would provide the alternative.

> Graham Cameron, head of services at the European Bioinformatics Institute> (EBI) in Cambridge, England -- an outstation of the European Molecular> Biology Laboratory -- points out that public domain databases such as PubMed> and GenBank and the EBI are widely considered to provide high levels of> cost-effective service to the community.

True, but until further notice, publishers' journals are not public databases. (Authors' self-archives, in contrast, CAN be.)

> Many believe, however, that the wider and cheaper access promised by> E-Biomed may happen anyway as a result of market forces. "Most scientific> society publishers are already doing what Varmus is proposing," says Frank.> "We are putting our journals on the web. We are linking our journals through> PubMed to our sister journals on the web. We are developing interfaces for> the submission and review of manuscripts on the web."

Indeed, and the objective is a click-through monopoly, with the online journal literature continuing to be held hostage to S/L/P till doomsday...

> Similarly, consortia of library and other users are increasingly negotiating> electronic licences for journals for entire institutions and even countries.> Scientists at such institutions can already access much of the literature> online.

That's the "L" in S/L/P, in case you didn't notice, whereas the fundamental underlying issue here is FREE access.

> "My initial reaction to E-Biomed is, 'so what?'. Virtually every library has> almost all major journals," says Heinz Steiner, a neuroscientist at the> University of Tennessee College of Medicine in Memphis. What is the point of propagating this nonsense? He might as well have said "Let them eat cake!"

And show me an active, busy researcher -- even at the most prosperous university in the world, Harvard, which subscribes to them all -- who, every time he needs a paper, prefers to walk to the library or send a student to photo copy, and shuffle through piles of photocopied offprints rather than having the entire journal literature on his desk and at his fingertips at all times.

> Market forces are also driving a flurry of deals among publishers that may> enable researchers to move rapidly and seamlessly from a citation to full> text across journal boundaries.

Via a click-through monopoly involving L and P deals (out of the S/L/P troika), done by those institutions with enough money to make them; everyone else is out of luck, as before.

> Frank asserts, for example, that the web site of HighWire Press already> accounts for a large proportion of the biomedical literature. This> not-for-profit outfit was set up in 1995 by Stanford University Libraries> and Academic Information Resources to help universities and societies to> publish on the web at low cost. "So I don't know why we need to create> E-Biomed," says Frank.

Ah me! Substitute for the 35,000 daily users of LANL (augmented all their potential biomedical counterparts worldwide) what the access levels would be if regulated by HighWire's "low" S/L/P costs.

> Indeed, the head of one scientific society argues that resentment over the> huge costs of the current journals system is confusing the many complex> issues involved in scholarly publishing. "If publishers are charging too> much then we should attack this problem directly, but not attack the entire> system. E-Biomed is a not very selective nuclear bomb."

Ebiomed would attack the system by providing an alternative, free route to exactly the same literature. You can't get more selective than that.

> Noorman, while admitting that Elsevier's profit margins "are higher than the> average," says that the arrival of web publishing is putting pressure on> commercial publishers. "Scholarly publishing will become a proper [not> distorted] market," he predicts. "Elsevier is not in the world to keep that> profit margin high. We are in the world to stay in the market. If the web> causes us to have to agree to lower profit margins, then so be it."

But don't hold your breath.

Stevan Harnad, University of Southampton, June 9, 1999

On Wed, 9 Jun 1999, ALPSP wrote:

> Many thanks to Stevan Harnad for the unsolicited publicity! Anyone who is> interested in actually reading the research study, so as to discover what it> was attempting to do, what questions were actually asked, and what the> conclusions were, is very welcome to order a copy (100 pounds or 200 dollars> to non-members of ALPSP) - just contact me.

Note that no endorsement was intended; indeed, I was criticizing the ALPSP study, sight unseen, based on the little that was already said about it. The passage below evokes the same criticism:

> In brief, our primary aim was to understand the motivations and concerns of> authors in the current journal publishing situation. It was clear that the> standing of the journals in which they publish is of key importance - more> so than price or absence of page charges; this would seem to indicate that,> on its own, an 'author pays' model is unlikely to be attractive. What it> would need, I feel (and I'm actually very attracted by the model) is for the> publisher of a large, highly rated, successful journal to take the plunge -> new journals will always struggle initially, regardless of their economic> model.

What authors should be asked is not whether they are interested in paying page charges. That would be an absurd question, under current conditions, with access to the literature blocked by Subscription/Site-License/Pay-Per-View (S/L/P) tolls. What they should be asked is:

(1) IF IT WERE POSSIBLE, would you prefer (as authors) that everyone everywhere have constant, permanent access to your refereed, published articles for free (and would you, as readers, prefer to have such access to everyone else's refereed, published articles)?

Having elicited a clear and resounding "YES" to (1) (formulated without any false oppositions about a loss of the prestigious journals or of quality or of having to pay to get it), they could be asked the following question: (2) Would you prefer that your institutions continue subsidizing access to those refereed articles on the reader-end, via S/L/P tolls, or would you prefer that they pay on the author-end, thereby bringing about (1)?

(3) If your preference in (2) is conditional on which alternative would cost more, what if author-end payment proved to cost considerably less?

(4) Would you contribute to making (1) come to pass by publicly self-archiving your refereed articles on the Web free for all (just as you distribute paper reprints to reprint-requesters) if this right were part of your journal copyright agreement?

At this point it would become abundantly clear to one and all, that any author reluctance about doing (4) could ONLY be based on perceived or actual attempts by the Journals to formulate copyright agreements that explicitly forbade public online self-archiving by authors.

It is then that this glaring conflict of interest will be seen clearly, and head-on, and that the new models for copyright agreements in the online era provided by the progressive publishers, such as the APS, and perhaps soon the BMJ, will be there for everyone to see as not only the possibilities, but the actualities that they represent, for one and all.

And only then will it be clear PRECISELY what is at stake in the question of whether there is any justification for continuing to hold the journal literature hostage to access-tolls now that they are no longer necessary.

> We did ask some more open questions at the end about authors' (a)> expectations and (b) wishes for future journal publishing. Interestingly,> very few predicted, or hoped for, a radically different model from the> present one. So if we want it to happen, we clearly have some way to go> with authors as well as publishers!

You are simply polling habit and ignorance, if you do not make the new possibilities crystal clear before asking about satisfaction and preferences.

Stevan Harnad, University of Southampton, June 12, 1999

On Thu, 10 Jun 1999, D. R. Forsdyke wrote:

>sh> Archives are archives, a reliable, permanent place where all authors>sh> can self-archive their journal articles on-line for free for all.>> The key here is "permanent". Files can be deleted and interfered with.> Could you spell out what you mean by permanent?

Los Alamos is good enough for now. When the world's authors' eggs are all in the same virtual basket, collective interests will ensure that they are reliably upgraded

in perpetuo. There is NOTHING here to detain us.

http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

>sh> the only function left for the journals to perform will be>sh> quality control and certification.> > You might point out two kinds of certification...one for potential> readers who are unable for themselves to sort out the "good" stuff...one> to help those who judge the authors as meriting appointment, tenure,> promotion and research funding. In the former case, I think better> search-engines (see latest Scientific American) and a new class of> professional sifters/reviewers) reporting directly to the internet, will> replace journals. In the latter case, merit assessors ("peers") will> really have to do their homework...read the author's application, not> just skim through his/her publications and tick off how many are in> Nature or Cell. (see http://post.queensu.ca/~forsdyke/peerrev.htm).

Nothing at all to point out along those lines:

Both peer review and tenure review could certainly do with some reform. But that has nothing WHATSOEVER to do with my one specific goal, which is to FREE the peer-reviewed journal literature, such as it is, not to "FIX" it.

The main point of my recommendations about self-archiving has been that these two agendas should not be conflated. Free the peer-reviewed literature (through author self-archiving via E-biomed) OR fix peer review, but do not hold the fate of one hostage to the other, especially given that the benefits of freeing the literature are already dramatically demonstrated whereas the reform schemes are all still untested pigs-in-pokes.

http://xxx.lanl.gov/cgi-bin/show_weekly_graph

Stevan Harnad, University of Southampton, June 27, 1999

This is a reply to the objections voiced by The American Association of Immunologists to the E-biomed proposal.

http://www.nih.gov/welcome/director/ebiomed/com0627.htm#aaoi185

Similar objections have been raised by others in the E-biomed discussion; the following replies apply to those too, mutatis mutandis.

> The American Association of Immunologists, June 23, 1999> Jonathan Sprent, M.D., Ph.D. President> Frank W. Fitch, M.D., Ph.D. Editor-in-Chief, *The Journal*

of Immunology> M. Michele Hogan, Ph.D. Executive Director> > Peer-Review: First and foremost, we find that this proposal compromises> the cornerstone of scientific method: peer-review. The process> described in your proposal is vague, but if taken at face value it does> not ensure a rigorous peer-review process. Without this we compromise> our excellence, (at best) and (at worst), pose potential harm to the> scientific community as well as the public at large. Furthermore,> scientists depend on the current peer-review process to give their work> legitimacy and guidance; they do not want to be held to lesser> standards.

It is correct that the E-biomed draft proposal is vague and somewhat ambiguous on some points, but this is all easily remediable. Indeed, a simple remedy was already proposed in the first set of comments on the draft:

http://www.nih.gov/welcome/director/ebiomed/com0509.htm#harn45

The remedy is to make it clear that the Archive is intended for the SELF-ARCHIVING of the refereed biomedical journal literature by its authors, in the first instance. It is not meant to be a journal; it is certainly not meant to provide peer review; nor is it meant to bypass peer review. If all authors self-archive their peer-reviewed articles, it is evident that peer review is in no way being compromised or sacrificed.

Once the archive has established its raison d'etre in this way, a formal relationship with journals will also be possible, in the form of official journal overlays "authenticating" the authors' self-archived drafts, as has evolved in the case of the Los Alamos Physics Archive and the American Physical Society, publisher of the most prestigious and highest-impact journals in Physics.

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

Initially, however, there is no need for this official overlay, nor can such official relationships be established before the Archive itself is established, with a body of contents that the user community puts to heavy daily use, as it does in the case of the Los Alamos Physics Archive, the model for E-biomed:

http://xxx.lanl.gov/cgi-bin/show_weekly_graphhttp://xxx.lanl.gov/cgi-bin/show_monthly_submissions"

In addition to self-archived peer-reviewed eprints, E-biomed will, like Los Alamos, contain self-archived non-peer-reviewed preprints as well. Because such unrefereed papers in biomedicine (unlike physics) could conceivably pose public-health risks, E-biomed has proposed a novel peer "filter" to screen them. This filtering is not a substitute for peer review, it is a supplement to it, in the NON-

peer-reviewed domain of self-archived preprints.

This ambiguity too, is easily resolved.

> Please do not dismiss out-of-hand a process that has taken 300 years of> trial and error to evolve...

These admonitions seem entirely unnecessary as the E-biomed proposal takes pains to make it quite explicit that it is not dismissing peer review!

Moreover, it is a false opposition to imply that the central goal of E-biomed, which is a free, self-archived version of BOTH the peer-reviewed and pre-peer-reviewed literature (clearly and unambiguously tagged as one or the other) can only come at the cost of compromising peer review in some way. It is quite clear that one can have both a peer reviewed literature AND a self-archived, free version of it: One need not sacrifice one for the other.

> Creation of a Monopoly: We are concerned that the proposal would create> a monopoly. The publishing world successfully operates on free-market> principles and there is no evidence that monopolies guarantee a better> product at a lower cost in any market. There may be cause for the> government to supply a product or a service if there is evidence that> free markets are unable to do so. But this is not the case in> scientific publishing. The federal government has the prerogative of> perpetuating established monopolies, e.g., national defence, but they> have seldom replaced a successful free-market effort. A single review> and publication source offers no options to investigators. Has the NIH> considered the risks of creating a monopoly to replace a diverse and> successful enterprise?

No monopoly whatsoever has been proposed. The Archive is not intended to be a Mega-Journal or set of Mega-Journals, replacing the established journals. It is intended to be a reliable, permanent, free repository in which the authors of the articles in the established journals can make their research available online for everyone for free.

Now there is no doubt whatsoever that this service will force the established journals to restructure themselves in certain ways. (My own prediction would be that it will make journals scale down to providing only the service of peer review and authentication, and that this service will be paid for on the author-institution end instead of the reader-institution end, but THAT is for the market to decide. E-biomed merely provides authors with an infinitely more powerful and useful way of distributing their peer-reviewed findings to everyone for free.)

> Conflict of Interest: In step with creating a monopoly is what we find> to be a conflict of interest in NIH becoming a sole, centralized> publisher. That is, that

the funding agency charged with carrying out> the assessment of the scientific accomplishment of an investigator now> also carries out one of the most important signifiers of that merit:> publishing. Additionally, it seems that rather than stimulating> experimentation, oversight of both functions by one organization could> narrow the scope of what is "acceptable" science.

Again a misreading (though, again perhaps resulting from some of the vagueness and ambiguity mentioned already, but easily resolved):

E-biomed is not intended to be a publisher but a free Archive of both the refereed and the unrefereed literature. Journals will continue to be the publishers -- but their role may well shrink to that of providing and then certifying the quality control. The rest of publishing will vanish (for refereed journals).

> Service To the Community: The JI is a major scientific publication> ranking in the top 10 percent of peer-reviewed publications> concentrating on immunology. Approximately 9,000 subscribers receive> 13,000 text pages annually in both print and electronic formats. Almost> 4,000 new manuscripts are received per year, requiring about 12,000> reviews. The peer-review process and the quality of data published is> overseen by a 60 member editorial board and a dedicated in-house staff.> We continue to acquire the latest in technologies that employ the> Internet and enable rapid delivery of information to the community in a> cost effective manner without jeopardizing the peer-review process nor> diminishing our standards of quality. This is most certainly the goal> of major scientific publications and we believe we have accomplished> this it very well.

All of this is irrelevant. JI is not at risk if it indeed intends to provide essential services to the biomedical community. Those essential services consist of implementing the peer-review of the literature, and certifying those final drafts that successfully pass that filter as accepted and published in JI.

But now we face the real heart of the potential conflict between E-biomed and the journals: Are the journals prepared to try to prevent their authors from self-archiving their refereed papers? If so, on what grounds? What service is being provided to the biomedical community by preventing authors from distributing for free what they have given to the journal for free (unlike authors of, say, books or magazines, who provide their texts in exchange for a fee or royalties)?

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

THIS is the real question to confront, and, I believe, the real motivation behind the attempts to find fault with the E-biomed initiative on "free market" grounds: Let us face directly the question of whether this very special and unusual subdomain of

publishing -- one in which the authors themselves want only the eyes and minds of their readers, not royalties or fees for their articles -- has any justification for trying to continue to hold this literature hostage.

We must ask what advantage SCIENCE derives from having its users denied access to its research findings, when the researchers themselves wish to give them away. This is the REAL conflict of interest lurking behind this entire question, and it needs to be brought out.

For a discussion of the "Faustian bargain" at the heart of all of this, see:

Okerson, A. & O'Donnell, J. (Eds.) Scholarly Journals at the Crossroads: A Subversive Proposal for Electronic Publishing. Washington, DC., Association of Research Libraries, June 1995. http://www.arl.org/scomm/subversive/toc.html

> The JI has one of the highest acceptance rates among the larger> journals and it is still only 40%. This leaves thousands of manuscripts> not accepted for publication in the JI. We consider the publication, or> even posting, of these unaccepted manuscripts to be clutter and a> disservice to busy scientists who rely on expert endorsement to present> the progress of their field.

This is a separate issue, and can be discussed separately, and examined empirically. It concerns the non-peer-reviewed sector of the Archive. It is likely that clearly tagging that sector as non-peer-reviewed (and the peer-reviewed sector as peer-reviewed, with journal name, etc.) will go a long way toward allaying such concerns, and E-biomed's proposed extra peer-filter superimposed on top of that will go still further. (In principle, all unrefereed papers in E-biomed could be made retrievable only with an accompanying "health warning," if that was really deemed necessary.)

But at this point dwelling on the unrefereed preprint sector is a red herring. Launch E-biomed exclusively for self-archiving the peer-reviewed literature, if we like, and meanwhile work out the details about the preprints. They need delay nothing.

> On another level, scientists depend on the hierarchy of journals to> help them select the most important studies in the plethora of> information available to them. It is unclear how a single information> source would assist this sorting process.

And this hierarchy will remain intact, with a clear tagging hierarchy in E-biomed, including JI.

> Logistics As we stated earlier, the JI alone receives almost 4,000 new> manuscripts a year for review and 60% of those are reconsidered. Unless> some

other screening event is put in place, this will continue and> likely increase without barriers such as manuscript submission fees,> page charges, and rigorous peer-review. How would the NIH handle the> volume from thousands of scientific journals in a timely and quality> assured manner? What are the personnel requirements to carry out this> effort? What sort of editorial oversight would be required? Would this> oversight be voluntary or would professional level FTE's be required?> The lack of details in the proposal regarding the logistics make it> impossible to assess your plan.

This was indeed the heart of the vagueness and ambiguity of the first draft, but the answers should be quite clear now: The established journals will continue performing this function, funded by the current sources of reader-institution-end access-tolls -- Subscription/Site-License/Pay-Per-View (S/L/P) -- until such a time as the free versions in the archive capture the user market as they have in Physics, and S/L/P cancellations begin to make themselves felt. At that time an alternative way to recover the much scaled-down costs of providing only the service of peer-review and certification will be author-institution-end publication-charges, provided by authors' institutions out of only a small portion of their annual S/L/P savings.

http://www.cogsci.soton.ac.uk/~harnad/nature.html

So this too is a false opposition.

> Budget for E-Biomed: The proposal states that scholarly publishing is> costly. The implication is that, somehow, these costs are an> unnecessary barrier and can be mostly eliminated if only all> publication took place electronically. As much as we wish it were not> so, the reality is there are large associated costs in scientific> publishing regardless of the medium; at best your proposal appears to> only shift, rather than eliminate, cost.

This is a controversial issue, much discussed, for example, in the *American Scientist*'s ongoing September-Forum:

http://amsci-forum.amsci.org/archives/september-forum.html

Suffice it to say that no publisher has yet done a realistic estimate of what it would cost to provide only peer review, dropping all other functions, and all expenses associated with them.

At the very worst, the answer could be: not a penny less -- in which case the reader-institution budget would be entirely shifted to the author-institution budget in exchange for freeing the literature for one and all. But the truth is probably much closer to a saving of 70% or more, in which case the institutions too (and not

only all authors and readers, and research itself) will be much better off.

> However, our real concern is there is no substantive budget to support> your proposal. We request that the NIH carry out due diligence and> provide a realistic, detailed budget analyzing the start-up costs> including personnel, infrastructure support, outsourced editorial> support, hardware, development of software, and redaction as well as a> projection for continued costs and support for assurance of longevity> of the project and archiving the acquired information.

The budget need be only for the provision of a reliable, permanent self-archiving facility, like Los Alamos. The cost will prove to be remarkably modest, as the Los Alamos experience already reveals.

This too was a false opposition, making it appear that the free archive can only be had if NIH pays the equivalent of all biomedical journals' full current operating budgets, whereas in reality the cost of providing a permanent, free, global archive is infinitesimal, and will also force a downsizing of journals, whose remaining essential costs will be recoverable out of institutional S/L/P savings.

> Support in Perpetuity: The final effect of this proposal, were it> successful, would be to destabilize existing publishers. If truly> successful, this would eliminate journals as we know them today.> Without the backup of these journals, the NIH would have to carry on> scholarly publishing in perpetuity and commit to this venture> regardless of the funding circumstances befalling the NIH. In times of> limited budget commitments to the NIH, how would the financial support> of this venture be assured?

Nothing of the sort. Initially, E-biomed will make it possible for authors to self-archive their refereed research findings for one and all for free. Once that free Archive starts to cut into S/L/P revenues, publishers will downsize and eliminate obsolete and inessential services. Quality control will continue to be an essential service, however, and its costs will be recoverable from S/L/P savings by abandoning the reader-institution-end trade model for an author-institution-end publication-charge model that is far more suited to this very anomalous and special form of literature: the refereed research corpus.

Stevan Harnad, University of Southampton, June 28, 1999

On Mon, 28 Jun 1999, J.W.T.Smith wrote:

> This entire debate seems to have become hung up on whether or not the Los> Alamos Archive model is applicable to e-publishing or e-archiving in other>

subject areas (especially biomed). This has obscured the fact it is> perfectly possible to believe, as I do, that the Los Alamos Archive model> is not the way to go for many subjects yet also believe in a model where> the role of current journals is reduced to that of quality control only.> > My objection to the Los Alamos Archive model is that it is centralised and> such a model can easily degenerate into a monopoly.

A monopoly of what PRODUCT, on behalf of what PROVIDER relative to what MARKET? For Los Alamos is in the (government-supported) "business" of making it possible for authors to give away reports of their own scientific research away to one and all for free.

And what do you mean "centralised"? Los Alamos is open to one and all, reader and author alike, the world over; it is mirrored in 15 countries, cached in who knows how many other places and ways, incorporated into further Gateways such as NCSTRL and Spires, and there integrated with other archives. Anyone else can make copies of the archive too (that's part of what make the "product" free entails), and the authors who self-archive in it are encouraged to archive their papers elsewhere too, if they wish, including in their own institutional servers, which can then be gathered together as another backup of the "central" archive.

http://xxx.soton.ac.uk/servers.htmlhttp://ncstrl.cs.cornell.edu/http://www.slac.stanford.edu/spires/about_spireshep.htmll

As I have noted before, this central/distributed issue is a red herring, based in part on papyrocentric thinking (we are in reality talking about a distributed virtual library where locus has little meaning) and in part on proprietary thinking, based on the reader-end, access-blockage trade model (whereas we are talking about self-archiving facility in which authors distribute their own "products" for free).

This has all been discussed in: http://amsci-forum.amsci.org/archives/september-forum.html

See: HTTP://AMSCI-FORUM.AMSCI.ORG/scripts/wa.exe?A1=ind99&L=september-forum&F=lf&O=T&H=0&D=0&T=1#5

> You asserted in a> recent note (27 June) that there was no intention that any archive become> a 'mega-journal'. However if it becomes the place where academics in a> given subject expect to find relevant articles it will have become just> that and it will become *necessary* for authors to place their work there.

Nothing of the sort! The journal is the quality controller and certifier. There will continue to be the full spectrum and hierarchy of journals, varying in quality and

impact factor, each with its own distinctive "brand name." In the virtual archive, this will be designated by tags, so you can restrict your search engine to the refereed literature appearing in, say, American Physical Society journals only, if you wish.

An Author Archive is hence, as I said, not a Mega-Journal: It is an archive, in which the entire refereed journal literature (as well us the unrefereed preprint literature) is available for free for all.

Now who is monopolizing what for whom?

> Although I have long argued, e.g.,> http://www.ukc.ac.uk/library/papers/jwts/d-journal.htm> for the separation of the quality control role of the traditional journal> from the publication role I have always advocated a 'distributed' model> over a 'centralised' model for 'publication/archiving'. This at least> escapes the possibility of a monopoly by the operators of the central> archive. It also echoes the argument in Stuart Weibel's earlier note (11> June) about the redundncy inherent in the multiple copies of> books/journals in the current paper library model. That model may be> inefficient (too many duplicates are kept) but its robustness is clear.

Redundancy is a non-problem; we know all about backups, mirrors, distributedness, and even distributed coding. It is a waste of time to keep dwelling on these solved problems. Moreover, they have nothing to do with the "monopoly" issue, which is likewise a red herring.

Stop thinking in terms of a reader-end "product," with competition among access-blockers, and think instead in terms of a platform for author-end "freebies," with collaboration among access-providers, and things will come into better focus. This is the refereed journal literature, not trade books or magazines.

> we should take from past publishing models that which is> clearly of value like peer review (and maybe distributed archiving?) but> discard that which is clearly constraining (due probably to some feature> of the underlying medium of the old model) like the linking of quality> control and distribution.

Correct, but then what is all this needless fuss about centralisation and monopoly?

> Summary: It is possible to escape the problems of the 'trade model' of> current academic publishing without running headlong into the possibly> equally constraining model of a monopolistic central archive.

Yes. Change the vocabulary.

Stevan Harnad, University of Southampton, June 30, 1999

On Wed, 30 Jun 1999, Juan Miguel Campanario wrote:

jc>> I am sending comments on EBIOMED. The EBIOMED idea is very similar tojc>> an idea I have published before. I am sending an elaborated versionjc>> of my idea with references to THE SCIENTIST, the journal in which Ijc>> published it.>sh>Unfortunately, I cannot agree with your ideas about peer review.>sh>Please see:sh>http://www.cogsci.soton.ac.uk/~harnad/nature2.htmlsh>plus my critique of E-biomed:sh>http://www.nih.gov/welcome/director/ebiomed/com0509.htm#harn45> > Dear Prof. Harnad:> > Thank you for your answer. I know your opinions concerning E-biomed > and concerning peer review.> > My main interest in E-biomed is that I propossed a very similar> publishing outlet some time ago. The main diference between E-biomed and > my idea is that, while in E-biomed authors are entitled to choose the journal > to which referes are afiliated, in my proposal (a central database or > metajournal) authors would submit an abstract or a full manuscript to the > central facility or metajournal. Journal editorial boards would routinely > scan the metajournal to locate potentially innovative manuscripts and/or > papers of interest. Editors would then contact authors about publishing the > article. If more than one offer is made, the author would choose the > journal in which to publish. The task of shopping around could be > eliminated and left totally in the hands of interested journals. The new > system would inspire a new role in science: the journal scout or journal > agent who would seek out manuscripts for journals.Journal scouts should be > real experts in their fields and should be able to convice editors that > candidate papers are worthy of publication. I strongly believe that when > electronic publishing evolves, the > publishing system will be similar to the above I dreamed. Now, I am interested > in stating that a precedent to the idea of E-biomed was published by me some > time ago. > > *The Scientist*, > 1997, Vol 11, Iss 10, May 12, pag 9 (Internet: > http://165.123.33.33/yr1997/may/let1_970512.html > > Juan Miguel Campanario> GRUPO DE INVESTIGACION SOBRE EL APRENDIZAJE DE LAS CIENCIAS> Departamento de Fisica > http://www.uah.es/otrosweb/giac> Universidad de Alcala > Madrid (ESPANA-SPAIN)> ----------

Dear Prof Campanario,

The idea is interesting but has some problems.

(1) Peer-review is a "seller's" market and not a "buyer's" market (if the "market" metaphor is applicable at all -- and I rather doubt it). This means authors are trying to reach the acceptance threshold of the highest quality journal they can reach. Quality-control is a FILTER, not a MAGNET.

(2) There is nothing whatsoever wrong with the current quality and specialty hierarchy and network of journals -- except that access to it is blocked (by the access-constraints of paper and the toll-barriers of proprietary paper and online access) instead of being free.

(3) E-biomed's real mission (once the confusion about being, competing with or collaborating with journals is resolved in favour of what the archive should real be: none of these) is to provide a reliable, permanent facility for authors to self-archive both their refereed reprints and their unrefereed preprints, thereby freeing the journal literature for one and all.

(4) Megajournals and peer-review reform having nothing whatsoever to do with it. E-biomed will only come into focus when it dissociates itself from such interesting but irrelevant and potentially derailing issues.

Stevan Harnad, University of Southampton, June 30, 1999

The BMJ/Stanford Self-Archiving Initiative

British Medical Journal (1999) 318:1637-1639http://www.bmj.com/cgi/content/full/318/7199/1637http://chronicle.com/free/99/06/99063001t.htm

The BMJ/Stanford initiative is a welcome one, but make no mistake about the fact that it differs from the E-biomed proposal in one very critical respect: It is only intended for author self-archiving of unrefereed preprints, whereas E-biomed is also intended for author self-archiving of refereed reprints too.

This difference is like night and day (apart from one little slippery-slope factor to be mentioned in a moment), for the E-biomed Archive would free the journal literature for one and all, whereas the BMJ/Stanford Archive would only broaden preprint distribution.

Let 1000 flowers bloom, however; all self-archiving initiatives are welcome, as they will eventually subvert the access-barriers that hold the literature hostage at the moment, one way or the other:

Ann Okerson & James O'Donnell (Eds.) Scholarly Journals at the Crossroads: A Subversive Proposal for Electronic Publishing. Washington, DC., Association of Research Libraries, June 1995. http://www.arl.org/scomm/subversive/toc.html

Moreover, BMJ/Stanford may get off the mark faster than NIH/E-biomed (which presently seems enmired in endless discussion):

http://www.nih.gov/welcome/director/ebiomed/comment.htm

Successful innovations rarely await prior consensus; they lead the way, which Los Alamos has already done:

http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

The Scholars Forum Archive needs to move into high gear too:

http://library.caltech.edu/publications/ScholarsForum/

Now the slippery-slope that could turn the BMJ/Stanford Archive into one that helps free the refereed journal literature after all:

Where is the point of no return on the continuum from the unrefereed preprint to the refereed reprint? Will any rational author want to reserve the power of free public self-archiving for the unrefereed side of that continuum alone?

http://www.princeton.edu/~harnad/intpub.html

And where do copyright agreements stand on this? The Los Alamos Physics Archive, which began as an unrefereed-preprint Archive now contains more and more refereed drafts, just as predicted in the Subversive Proposal above. And why not? The outcome (note: not the prior cause) is that the American Physical Society, the publisher of the highest quality and impact journals in Physics, now has the most progressive copyright policy, a model for all other publishers. No attempt is made to prevent authors from self-archiving the refereed version. (In whose interests would that have been? Certainly not in those of authors or readers, nor of research itself, hence of the rest of society.)

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

Let me close with a plug for another brave new Archive, for the interdisciplinary field consisting of the Cognitive Sciences (Psychology, Neuroscience, Computer Science, Biology [sic], Philosophy, and Linguistics) which has quietly been following the Los Alamos model for over a year now:

http://cogprints.soton.ac.uk

The academic thoroughbreds have been led to the water; history will record how long it takes them to drink...

Stevan Harnad, University of Southampton, July 1, 1999

Caution Against Anarchic Archiving!

On Thu, 1 Jul 1999, Steve Hitchcock wrote:

> Not to forget free Web access to all papers, including e-prints and> refereed papers, recently announced by the commercial publisher Current> Science> http://current-science-group.com/Presscos.html> > ...not all> such initiatives will be positive for the academic community or will help> towards the goal of universally accessible, free e-print archives in> perpetuity... > ...other important factors to consider when> evaluating these archives are ownership and long-term plans for access,> distribution, mirroring, etc, to avoid the same hostage to fortune that> journals have come to represent. Multiple archives are fine, but there> should be scope for integration via distributed services too.

Steve Hitchcock is 100% right for sounding this important note of caution. What is not wanted is an anarchic-archivng period of fly-by-night archives in which authors' papers become orphaned (as they have in many other ephemeral web and ftp sites) or are stamped with a proprietary price-tag after an interval.

(I am not implying that this is the case with the Current Science Archive or any of the other nascent ones, but it is something that must be given explicit consideration. Let me add, though, that the Current Science Archive, like the BMJ/Stanford Archive, is for UNREFEREED preprints only; apart from that, it is in reality endeavouring to be a JOURNAL, not an archive, for it plans to offer peer review for papers if they are submitted on the refereeing track. As I have cautioned many times before, founding new archives should NOT be confused or conflated with founding new journals, nor with reforming peer review. The purpose of public online archives is to free the journal literature for one and all; there is a place for new online journals too, as there always has been, but that is an incomparably smaller matter, and only beclouds the free-archive issue if linked to it in any way. Peer review can do with some reform too, but that is a long-term empirical problem, requiring experimental testing, and hence likewise not to be linked in any way with the fate of freeing the journal literature through self-archiving, whose time has now come, and whose empirical success has already been resoundingly demonstrated by the Los Alamos Archive

(http://xxx.lanl.gov/cgi-bin/show_monthly_submissions).

Stevan Harnad, University of Southampton, July 5, 1999

On Fri, 2 Jul 1999, Frank Norman wrote:

> I have been following your perceptive and (to me) convincing > arguments about the merits and demerits of the e-biomed proposal > from Harold Varmus et al. > > In your posting on June 27th you said:>> > Quality control will continue to be an essential service, however, and its> > costs will be recoverable from S/L/P savings by abandoning the> > reader-institution-end trade model for an author-institution-end> > publication-charge model that is far more suited to this very anomalous> > and special form of literature: the refereed research corpus.> > If this comes to pass, I can envisage that the more highly > prestigious titles (Science, Nature, Cell etc) will demand higher > publication charges than journals lower down the hierarchy. Indeed, > it may be that there is a very substantial premium to be accepted > by the most prestigious titles. One effect of this could be to > prevent less well-financed research groups from publishing in high-> prestige journals. They'll be able to access the content of such > journals for free but will not be able to afford to publish in them. > > Is this how you see things working out? How could this outcome > be circumvented?>> Frank Norman > Acting Librarian > National Institute for Medical Research > The Ridgeway, Mill Hill> London, UK> http://www.nimr.mrc.ac.uk/personal/Frank

Nothing like this will happen; it is based on a misunderstanding of peer review -- and of what it is that makes the prestigious journals prestigious, and hence makes authors prefer to submit papers to them rather than elsewhere:

The prestige of the top journals is based on their quality, which in turn depends on their quality-control standards: They only accept the very best papers (and their typically high rejection rates and citation impact factors reflect this). (They are not "designer labels," for the patina of which a "consumer" is willing to pay more!)

The way high standards of quality are maintained is through rigorous peer review: One cannot BUY success in that process; authors must EARN it (by doing high quality work). Otherwise the prestigious journals would simply lose their prestige (and be replaced by other, more rigorously refereed journals, that recaptured their standards, and THEREBY the best papers [no, they will not LOWER their charges to capture to higher-prestige authors either! this sort of market-thinking is all based on the wrong, old, reader/consumer-end model: or, to put it another way, the "competition" in this highly anomalous, nontrade, research literature is for high-quality papers, not for author-dollars.]).

On the contrary, it is much lower down in the peer review hierarchy, as one approaches the vanity press, that some abuse of the author-end system is conceivable: Authors may try to buy their way into the pages of low-quality journals when they have failed to earn their way into the high-quality ones. But, frankly, I don't find this at all worrisome! Vanity publications are apparent to everyone; they wear the result (low quality) on their sleeves (and their contents, their authorships, their rejection rates and their impact factors); and such journals already exist today, where the "subsidy" currently comes on the reader-institution-end rather than the author-institution-end -- everyone knows which ones they are, and that "caveat emptor" prevails when it comes to deciding whether to read them or rely on what they report.

http://helix.nature.com/webmatters/invisible/invisible.html

The costs of submitting to the high-quality journals will be close to the true costs of implementing peer review, for that is all it will involve (and the peers do not request or receive remuneration; they referee according to the Golden Rule: it is only the IMPLEMENTATION of the refereeing that incurs some cost); it is the vanity press that may have to bribe referees (at the author's cost).

(Unaffiliated authors, with no institution to fund them out of annual windfall S/L/P-cancelation savings and no funding agency to cover the minuscule publication costs can be funded from collective publication slush funds established for this purpose at the journal or suprajournal level. So this too should not detain us.)

Let me close with two further points:

(1) It is inadvisable to try to second-guess outcomes in this way; it is all too hypothetical, and for everything we think of in advance, there will no doubt be several unexpected contingencies we didn't think of. This isolated single-variable second-guessing simply helps entrench the status quo, because so many people have vested interests in retaining it -- or merely prefer to do and change nothing.

So forget about author-end page charges! They will sort themselves out. The ONLY salient thing at this moment is the absolutely unambiguous desideratum that all authors should self-archive their refereed papers NOW and ensure that no copyright agreement ventures to block that capability. That will effectively free the journal literature. The rest will take care of itself.

(2) There is another Harnad, not myself, but a mathematical physicist by the name of John Harnad (and the one who first drew my attention to the Ginsparg Archive way back in the early '90's). J. Harnad has some further recommendations on the subject of referee answerability and compensation: He recommends that (a) all

referees should be paid to referee papers; (b) payment for a rejected paper should be minimal (say, $200), but payment for an accepted paper should be commensurate with the effort of seeing it through the successive revisions (say, $2000) to successful publication; and, to avoid the potential abuse discussed above, (c) if a paper is accepted, the name of the accepting referee(s) should be co-published with it, to share the responsibility, praise or blame. He feels this would raise the quality of the refereeing and make the entire process much more answerable, hence effective, than it is now.

Obviously this proposal is compatible with the transition from reader-institution-end payment to author-institution-end payment, but it is an as yet untested peer-review reform proposal; all such reform proposals need to be tested empirically and practically before being implemented on any scale. Hence it should not detain us on the road to freeing the CURRENT refereed literature, such as it is.

(I also think that there is not enough money in the world to pay fairly for the precious time that referees steal from their own research in order to do the mostly thankless task of peer review; hence the Golden Rule is probably the only one we can continue to rely on! SUBMISSION charges, creditable toward PUBLICATION charges should the paper be accepted, may not be a bad idea to levy on authors, though, with or without referee payment, for it to might help raise submission standards and even revision conscientiousness, hence perhaps even lightening some of the burden on the work-horse referees; but this too would need pre-testing.)

Stevan Harnad, University of Southampton, July 6, 1999

On Tue, 6 Jul 1999, Fytton Rowland wrote:

> Professor Stevan Harnad argued quite a while ago that the models that he> has advocated refer to "esoteric" publications (his term), which roughly> fit the old assumption that the authorship and readership of a specialised> scholarly journal are the same people. He has always recognised, I think,> that other types of publication are different, and will continue to operate> on a trade model paid for by a combination of income from advertisers and> from purchasers. Such publications often (but not invariably) pay their> contributors too. New Scientist would fit this description.

Under advice from Ann Okerson and others, the "esoteric" descriptor has now been dropped in favor of the (tautological) descriptor "nontrade," but in its place there is now a simple algorithm:

Does the author (1) seek/get any revenue for his text (royalties, fees) or does he instead (2) give it away, seeking only the eyes/minds of readers?

If (1), it is trade, if (2) it is not.

> However, Don King -- always an invaluable source of real, verifiable> *facts* about scholarly journals as opposed to opinions and attitudes --

Thanks for the implied compliment (read on)...

> points out that many scholarly journals have a far wider readership than> is necessarily indicated by their citation patterns.

Citation patterns are irrelevant to the trade/nontrade distinction. So is the size of the readership, according to the new, more precise algorithm above.

> It isn't true to say> that only the authors ever read the journals -- the reader community is> often wider.

It was never true to say that only the authors read even the most esoteric of journals. The authors (opting for (2)) always hoped to capture more eyes/minds than that, and occasionally even managed to do so.

But it was not just the rarefied subject matter of their articles that had conspired against these nontrade authors, who were seeking only eyes/minds for their texts; it was also the access barriers of (a) paper and (b) its economics, which necessitated toll-gates -- usually in the form of institutional Subscription/Site-License/Pay-Per-View (S/L/P) -- which denied entry for all unpaid eyes/minds to the author's freely given ideas/findings.

In the online era, both of these barriers to the eyes/minds of nontrade authors' potential readership have ceased to be necessary; this give-away literature can at last be freed for everyone, everywhere, forever:

http://www.arl.org/scomm/subversive/toc.html

> Examples would be: practitioners (physicians, engineers,> lawyers, etc.) who don't actually do research; high school teachers; some> of the educated lay public; and of course students, undergraduate as well> as postgraduate.

Completely irrelevant: Tell a nontrade author trying to maximize the eyes/minds that access his work that he should NOT self-archive it publicly for free for all, because in some magazines some people are willing/able to pay for it!

> So far as really esoteric journals are concerned I think Professor Harnad> is right; they do not belong in the commercial world at all, and an> "author-pays" system, with a moderate charge to cover the costs of peer> review and of maintaining the document on the WWW in perpetuity, seems> appropriate.

The only open question -- and, thanks to the algorithm mentioned above, this is a matter of FACT, not opinion or attitude -- is: "Which are the 'really esoteric journals' that fall into this category?". The answer will be loud and clear: The ENTIRE REFEREED JOURNAL LITERATURE, which the author gives away to his publisher for free, seeking only the eyes/minds of readers in return.

> At the other end of the scale, Nature, for example, is a very successful> commercial enterprise, and there is no way it will cease to be> "reader-pays" - but in any case, high circulations attract advertising> revenue and generally help to keep cover prices down.

Nature is hybrid. It has articles written by journalists for a fee, it has some borderline cases in which scientists are paid a very modest fee to provide commissioned articles, and it has the submitted, refereed reports of new research. The solution is simple: The trade portions can proceed apace, and the journal itself can continue to be sold via S/L/P for as long as there is a market. But the REFEREED articles can also be self-archived by authors for free for all.

Nature's copyright agreement regarding online self-archiving, unlike that of Science, is closer to the right direction on this, but eventually it will have to conform fully to the model provided by the American Physical Society, with full online self-archiving rights guaranteed for both the unrefereed preprint and the refereed reprint:

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

> There is a grey area in between, where journals such as those of the> American Chemical Society, for example, have a large sale to commercial> chemical and pharmaceutical companies. There is no reason on earth why> academia should subsidise *them*, so surely a "reader-pays" system should> stay. The argument comes down to this: how do we draw the lines between> the different types of scholarly journal? -- Fytton Rowland

Completely incorrect! The fact that institution X is willing and able to pay for an ACS journal via S/L/P is of absolutely no use to ME if I am in institution Y or country Z, which isn't. Nor is it of any use to the author of that article that my eyes/mind and countless others continue to be denied access to his work because there are still others who can afford not to be!

http://www.cogsci.soton.ac.uk/~harnad/nature.html

Stevan Harnad, University of Southampton, July 10, 1999

On Sat, 10 Jul 1999, Hal Varian wrote:

> Undoubtedly, [trade authors] would be happy to be paid for> writing, but academic authors would be happy to be paid for their writing> too.

I'm afraid I have to disagree. The answer is "yes" when the academic authors are wearing their trade hats (writing books or magazine articles) but the answer is "no" when they are reporting their research in peer-reviewed journals.

> No author *wants* to deter eyeballs, even trade authors.

You might as well say no producer of any product wants to deter greater consumption of their product (it's just that they would like to get paid for it!).

> If academic authors> were *sufficiently* compensated for detering eyeballs, I expect many would> be happy to do so. If a referred journal offered you payment for an> article, would you turn it down? (Several refereed journals in business> and medicine do pay for articles, by the way.)

I'm afraid I have to disagree again: It would require a MONSTROUSLY large amount of money to make a research author trade off his potential impact on research for the impact on his pocketbook. (And many scientists and scholars, still recalling why they chose the leaner path of Learned Inquiry rather than heading straight for the junk bond market in the first place, would decline even that!)

But no peer reviewed journal could (or would) afford to make it worth an author's while anyway.

Here's a test: How much would (should) the author of a refereed journal article accept to SELL his self-archiving rights to his publisher?

(My guess: a lot more than any publisher could ever afford to offer! This is not big-market literature we're talking about! And that's the point!)

http://www.cogsci.soton.ac.uk/~harnad/THES/thes.html

> Furthermore, even academic authors make money from their publications,> albeit indirectly. If you regress earnings on publications and citations> you find a large and significant coefficient on refereed journal> publications and citations.

Academic authors who publish more are paid> more, and part of the motivation for academic publishing is the prospect> of academic advancement and higher salaries.

Correct, but TOTALLY irrelevant! The author is not being paid out of the access-blocking toll-gate receipts from the sale of his papers by S/L/P!

On the contrary, if you properly regressed THOSE on earnings, you would find that they REDUCE impact and hence earnings!

This is a completely spurious, noncausal correlation, and the simple act of self-archiving shows it to be so. (Have the 100,000 authors who have self-archived in Los Alamos reduced or inhanced their impacts and incomes?)

http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

> The point of my note is that motivations are not as far apart as you> claim: there is an (indirect) financial motivation in publication for> academic authors and there is a large component of desire to communicate> in trade authors. There is certainly a difference in the economic model> in the two industries, but the divide in the motivations of the authors is> not as "profound and significant" as you claim. > > But again, I don't think this point makes much difference for the rest of> your argument.

I'm afraid I disagree, and I do think it makes a great difference. The similarities between the two populations are partial, superficial and, in the present context, misleading. The deep differences are in the means/ends: For most trade authors, self-archiving their work free for all is only a temporary means to an end (hopeful, eventual compensation); for (most?) refereed journal authors it IS the end (widest possible access to their findings for peer eyes/mind, present and future).

The reason stressing the similarities between the trade and nontrade literatures here is misleading is that the self-archiving model I have been advocating for refereed journal papers is decidedly NOT the right model for the rest of the literature, and conflating the two simply blurs the critical insight at the core of all this.

But this can be settled empirically: Let a line be drawn in Cyberspace, and let those who are interested in giving away their products (whatever they are) as freebies in perpetuo step to the left of it (say), and let those who are not step to the right.

The entire refereed journal authorship will be on the left. Perhaps some others will be too. Let's see wait and see who. I'm predicting that most book and magazine

authors will not (and note that I said "in perpetuo," not in "pro-tem promo"!).

In any case, that is the literature I am dedicated to freeing (from its hostagehood to the trade model and S/L/P) -- not every product of the human mind!

Stevan Harnad, University of Southampton, July 11, 1999

On Fri, 9 Jul 1999 mloeb wrote:

> Stevan,> > I'm touching base with you to review what you plan to say during your> talk at the upcoming CESSE conference in Cleveland. You and I are the> only speakers in the session. We are suppossed to be talking about the> future economics of journal publishing... looks like a perfect> opportunity to expose our respective visions on the future of> scholarly publishing in general.> > Have you thought about what you will say? I'd like to know so that I> can complement/challenge your points, and then we can have a> constructive, facilitated discussion/exchange with the audience for the> balance of the session.> > Let me know your thoughts; I'll share with you mine.> > Look forward to meeting/seeing you in Cleveland.>> regards,> Matt Loeb> publisher> ieee computer society

http://www.cesse.org/cesse99/program.htm

Hi Matt,

Nice to hear from you. I know (since it has been explicitly denied!) that this is to be something of a roast of me and my views, and that's fine!

Here they are (the details are in my published and posted writings, probably the two in Nature and Nature-online, below, tell it all as well as anything):

(1) I am speaking ONLY about refereed journal articles, not books or magazines.

http://www.cogsci.soton.ac.uk/~harnad/THES/thes.html

(2) Unlike all other literature, their authors write these papers to report their ideas and findings, not to make money on their texts. All they want is to reach the eyes and minds of a maximum of fellow researchers, present and future, once their findings have passed peer review.

http://www.cogsci.soton.ac.uk/~harnad/nature2.html

(3) They accordingly give them away for free to their publishers, and, after peer

review, give away free reprints to all requesters.

http://amsci-forum.amsci.org/archives/september-forum.html

(4) Online self-archiving now makes it possible for them to give away their refereed reprints to one and all forever on the broadest possible scale.

http://www.arl.org/scomm/subversive/toc.html

(5) Publishers should in no way to attempt to prevent free self-archiving by authors by trying to forbid it in copyright agreements. This is the eye of the storm. See:

http://www.cogsci.soton.ac.uk/~harnad/science.html

(6) The American Physical Society has already provided a model copyright policy: Authors may self-archive both the unrefereed preprint and the refereed reprint for free for all. The Publisher retains all rights to SELL either the paper or online version of the journal.

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

(7) The effect of online author self-archiving will be a transition of the reader/user community to the free online versions.

http://xxx.lanl.gov/cgi-bin/show_monthly_submissionshttp://xxx.lanl.gov/cgi-bin/show_weekly_graph

(8) Eventually this will produce cancelation pressure (although it has not done so yet in Physics, where it is most advanced). If/when it does, my prediction is that publishers will have to restructure and down-size so as to provide only the service of quality control and certification [peer review, editing, tagging as accepted by Journal X].

http://www.cogsci.soton.ac.uk/~harnad/nature.html

(9) The much reduced cost of providing solely this service will be recoverable from author-institution publication charges, which will in turn be recoverable from institutional savings from cancelling Subscription/Site-License/Pay-Per-View (S/L/P).

(10) The critical difference is that reader-institution-end payment (S/L/P) is access-blocking, whereas author-institution-end payment is not. But as long as

author self-archiving rights are guaranteed (5,6), the market can decide whether or not S/L/P can survive alongside it (and how long).

(11) The infrastructure for self-archiving is emerging as we speak, led by Los Alamos, soon to be followed by E-bionet, and then all the other disciplines.

http://xxx.lanl.gov/http://www.nih.gov/welcome/director/ebiomed/ebiomed.htmhttp://library.caltech.edu/publications/ScholarsForum/http://cogprints.soton.ac.uk/

The self-archiving initiative in this very special subdomain of literature -- the give-away refreed research literature -- is unstoppable, because ethics, prgamatics, and logic, as well as the inherent interests of research itself and hence of all of society, are all behind it. Its progress can only be slowed temporarily by playing on confusions and uncertainties in people's minds, simply because it is all so new and they have not yet thought it through. It would be to publishers' long-term advantage to try to see ahead rather and restructure accordingly, rather than to try to hold the literature hostage to the status quo. They must come to terms with what it in the best interests of research and researchers in the new online world, and design a new niche for themselves in the PostGutenberg Galaxy.

A tide-over consortial subsidy out of windfall S/L/P savings to smooth the transition from reader-institution-end cost-recovery via S/L/P to author-institution-end cost-recovery via quality-control/certification charges would be worth planning out with the library-institution and research-funding community in advance.

HTTP://AMSCI-FORUM.AMSCI.ORG/scripts/wa.exe?A1=ind98&L=september-forum&F=lf#26

Stevan Harnad, University of Southampton, July 12, 1999

The exchange with Hal Varian has been very interesting, but in the interests of hastening convergence, I will be more telegraphic in my quote/commenting in this round, but first a summary:

The only two substantive issues now are (1) an error (about "author charges") and (2) a disagreement (about who should pay for peer review).

(1) Hal speaks of AUTHOR charges, and I keep speaking of AUTHOR-INSTITUTION charges. The annual costs for quality control/certification (QC/C) (less than 1/3 of the total institutional S/L/P costs for full paper and online publication in the present, obsolete system) will not and should not come from

authors' pockets but from (less than 1/3 of) author-institution's annual S/L/P savings from total S/L/P cancellation.

The rest of the (marginally vanishing) costs of periodical publication in the new system are to be borne by centralized self-archiving facilities (Los Alamos, E-biomed, CogPrints, Scholar's Forum), backed up by distributed institutional self-archives, plus the increased offloading of word-processing (and soon tagging/mark-up) onto authoring software (which is on the rise anyway).

(2) Hal acknowledges the trade-off between direct benefits (royalty income) from the sale of texts and indirect benefits ("impact" income). (I put this in crass income terms just for the sake of simplicity.) The trade-off is that charging for access (royalty income) means loss of access to those who can't/won't pay (impact income). (I won't even mention that journal authors don't even get the pennies from the royalty income!)

Yet, despite acknowledging this loss of potential readership (hence indirect revenue) caused by S/L/P barriers, and despite agreeing that self-archiving may even be the way for many books, let alone journals, when it comes to the question of how to recover the much lower residual costs of quality control/certification [QC/C], Hal regresses on the S/L/P trade model, seemingly forgetting both the trade-off and the self-archiving option!

Part of this misunderstanding may revolve around institutional S/L/P, which currently SUBSIDIZES readers (at that lucky institution); Hal contrasts this with PERSONAL (out-of-pocket or out-of-grant) payment of author charges in the proposed system, whereas the most natural way to think of it is simply as rechanneling what is already an institutional subsidy from S/L/P costs to the much lower up-front QC/C costs!

If you must think in terms of who the "consumer" is and how he benefits, the consumer is the author-institution in both cases, the benefit in the former case (reader-institution end S/L/P) being to buy in SOME of the journal literature for the use of its scholars and scientists (to enhance their research impact), the benefit in the latter case (author-institution end QC/C) being to buy in ALL of the journal literature for the use of ALL scholars and scientists (thereby enhancing everyone's potential impact) -- and at less than 1/3 of the price!.

When you speak of retaining S/L/P from the author-institution's point of view, always keep in mind their lost potential impact on the eyes/minds of the many institutions and countries that cannot afford that particular journal...

In fact, the best intuition pump I have found for why charging S/L/P for access makes no sense for the refereed research literature is that it is for exactly the same

reason that charging for access to advertisements would make no sense!

Now to (telegraphic) quote/comment mode:

On Mon, 12 Jul 1999, Hal Varian wrote:

> ...You argue that the author should pay for this filtering> role. Perhaps that is right, but one could also make a case that the> reader should pay since he is the direct beneficiary of the filtering,> ...now that the other costs have been driven so low.

But this makes the author and author-institution the LOSER of all those potential eyes/minds (and impact income) -- and PARTICULARLY considering how low those "filtering" (QC/C) costs really are!

> ... the funding agencies pay researchers to produce papers which are sent> to journals to be evaluated and, in most cases, published, which are> then purchased by libraries for the free use of the researchers.> ... it is the role of intermediaries that has got us into the current> mess; if the authors/researchers faced the true costs of the current> publication system, they would find a way to reform it quite quickly!

Indeed they would, but it would not be by simply lowering S/L/P barriers, but by eliminating them completely, through author-institution end QC/C payment out of 1/3 of S/P/P savings, plus author self-archiving.

> There is something of a tradeoff, but perhaps less than you think.> There are ways to vary access and recover costs from customers. Even> now you can purchase a journal subscription directly or go access> it "for free" in your library. Here are two different kinds of access,> one more convenient than the other, and they are priced accordingly.> So it isn't "access" vs "no access" but different degrees of access.> The same can be done in the online world; whether this is desirable or> not is a different question.

And what about the many countries and institutions that can't afford either form of access? (And re-calculate that at least 14,000 times for each of the refereed journals in Ulrich's that some institutional author's work might appear in.)

"It is easy to say what would be the ideal online resource for scholars and scientists: all papers in all fields, systematically interconnected, effortlessly accessible and rationally navigable from any researcher's desk worldwide"
http://www.cogsci.soton.ac.uk/~harnad/citation.html

As an author, how many potential readers of my work would I like to deprive of this resource -- in the interests of a reader-end S/L/P model (from which I do not make a penny, and which costs my institution at least twice as much as barrier-free QC/C would)?

And what is the counterpart of personal vs. library access in the desk-top, networked world?

> You argue that author charges could pay for peer review. This maybe> correct, but I worry about the incentives in such a system. Under> reader pays, the publisher has an incentive to keep quality high in> order to attract readers. Under author pays, the publisher has an> incentive to get as many authors to pay as possible, and other> mechanisms must be used to maintain quality.

I have already replied to this, in response to Frank Norman at the National Institute for Medical Research. This "vanity press" model of author-pays profoundly misunderstands peer review!

Nothing like this will happen; it is based on a misunderstanding of peer review -- and of what it is that makes the prestigious journals prestigious, and hence makes authors prefer to submit to them rather than elsewhere:

The prestige of the top journals is based on their quality, which in turn depends on their quality-control standards: They only accept the very best papers (and their typically high citation impact factors reflect this). (They are not "designer labels," for the patina of which a "consumer" is willing to pay more!)

The way that high standards of quality are maintained is through rigorous peer review: One cannot BUY success in that process; authors must EARN it (by doing high quality work). Otherwise the prestigious journals would simply lose their prestige (and be replaced by other, more rigorously refereed journals, that recapture their standards, and THEREBY the best papers. [And, no, they will not LOWER their charges to capture to higher-prestige authors either! This sort of market thinking is all based on the wrong, old, reader/consumer-end model: or, to put it another way, the "competition" in this nontrade literature is for high-quality papers, not for author-dollars.]).

On the contrary, it is much lower down in the peer review hierarchy, as one approaches the vanity press, that some abuse of the author-end system is conceivable: Authors may try to buy their way into the pages of low-quality journals when they have failed to earn their way into the high-quality ones. But,

frankly, I don't find this at all worrisome! Vanity publications are apparent to everyone; they wear the result (low quality) on their sleeves (and their contents, their authorships, and their impact factors); and such journals already exist today, where the "subsidy" currently comes on the reader-institution end rather than the author-institution end -- everyone knows which ones they are, and that caveat emptor prevails when it comes to deciding whether to read them or rely on what they report.

And don't forget: The peers review for free... QC/C costs are only for IMPLEMENTING refereeing, not for the referees themselves (who, like the author, contribute for free). Vide supra.

> if an organization "can't afford" access, it is> likely an accounting illusion rather than actual lack of money.

I'd like to see the data for that, not even for all 14K journals in Ulrichs, but just, say, the top 6.5K indexed by the ISI. And please tell me the figures per journal, per institution, per country. As an author/advertiser, I would like to know how many potential customers I am really LOSING if I endorse the SELLING of my papers/ads, instead of having my institution pay up front to GIVE them away to one and all...

> The important principle is that the readers are willing to pay> something for the filtering services provided by the journal. And it is> this willingness-to-pay that supports the current business model. You> could be right that "author pays" is superior for the reasons you cite.> But my worry is that the economic incentives to provide value to the> reader (via filtering) are weakened.
Vide supra. Peer review will take care of itself (money is not involved in refereeing); focus on the my POTENTIAL readers whose institutions CANNOT pay...

Stevan Harnad, University of Southampton, July 12, 1999

On Mon, 12 Jul 1999, Hal Varian wrote:

> You may end up being right that S/L/P is no longer appropriate given> the change in costs. If it was so inefficient, how could it have> survived for so long?

The answer couldn't be simpler: Because of the technology and economics of print-on-paper! In that medium, S/L/P was the only viable option if one wanted to be published at all.

In the PostGutenberg, online-only era, for refereed journals, the days of S/L/P are over!

But S/L/P is STILL fine for the trade literature (books, magazines). It's only the anomalous, give-away literature that has been freed at last of the "Faustian Bargain" that held it hostage to S/L/P tolls until now.

>sh> And what about the many countries and institutions that can't afford>sh> either form of access? (And re-calculate that at least 14,000 times for>sh> each of the refereed journals in Ulrich's that some institutional>sh> author's work might appear in.)>> And what about countries and institutions that can't afford submission> fees? In the long run, the same costs have to be paid.

I knew, as I wrote that, that this would be the come-back!

The answer is:

(1) Those disenfranchised institutions are currently NET CONSUMERS of the literature (they aspire only to READ it, if they could only afford it!). They are not net providers (they are not publishing much). They could not afford most of the journals under the S/L/P system. So their researchers had much less basis for publishing anything either, being starved of access to the literature.

In the up-front system, these institutions will simply get a free ride from the NET PROVIDERS (research-active, high publishing-rate institutions), but no one will lose as a result of this. (Stealing my paper to read is a victimless crime in the post-print-photocopy age! Among other things, this is the end of the "Copyright Clearance Center" for the journal literature, which is merely a variant of the "P" in S/L/P.)

It should still average out to less than 1/3 of every institution's prior S/L/P budget being rechanneled toward up-front costs.

And as the institutions that were disenfranchised by S/L/P barriers begin to become more research-active as a result of free access to the literature, their research productivity and income should rise, as should their publication rate, and the resulting revenue available for covering those increasing publication costs. (Research and research-impact revenue should always be ahead of QC/C costs by at least a factor of two, if my < 1/3 figure holds.)

(2) What about institutionally unaffiliated scholars? I think a modest slush fund should be able to cover that minoritarian need quite adequately.

> Your argument is> that the author-institution pays system covers the costs and

allows for> broader readership, an observation with which I agree. However, there is> a more subtle issue. An economic system tends to favor those who pay. If> the authors pay, then the system will lean towards the author's goal> (getting published) whereas if the readers pay the system will lean> towards the reader's goal (effective filtering.)

This is the vanity-press argument again. Reply: Peer Review. The peer community will continue to maintain the standards, as always, for free! It is only the IMPLEMENTATION of peer review that needs to be paid for, not referee time/effort. And journal rejection-rates and impact-factors will continue to be the marks of quality (and the magnet for authors), not the money exchanged for implementing peer review!

> I'm not sure which effect is larger. But, of course, there is no> reason why both sides couldn't pay, if that turned out to be the> appropriate way to align incentives.

Heaven forfend! The worst of all possible worlds! You have to pay to read AND you have to pay to be published! Insult upon Injury!

> > "It is easy to say what would be the ideal online resource for> > scholars and scientists: all papers in all fields, systematically> > interconnected, effortlessly accessible and rationally navigable> > from any researcher's desk worldwide"> > http://www.cogsci.soton.ac.uk/~harnad/citation.html> > > > As an author, how many potential readers of my work would I like to> > deprive of this resource -- in the interests of a reader-end S/L/P> > model (from which I do not make a penny, and which costs my institution> > at least twice as much as barrier-free QC/C would)?> > The publication system shouldn't be designed only to serve authors--- it> has to serve the needs of readers as well (especially if they are the same> people!). One might add terms like "all meritorious papers, systematically> evaluated and vetted" to your "ideal online resource". (I realize that> you acknowledge elsewhere that refereeing is a critical part of academic> publication, even though it ends up being missing as a desideratum here.)

Precisely. It is and always has been the freeing of the REFEREED JOURNAL LITERATURE to which all these efforts have been directed. And as far as I can tell, that completely nullifies your objection here!

>sh> This "vanity press" model of>sh> author-pays profoundly misunderstands peer review!> >sh> The prestige of the top journals is based on their quality, which>sh> in turn depends on their quality-control standards: They only>sh> accept the very best papers (and their typically high citation>sh> impact factors reflect this). (They are not "designer labels," for>sh> the patina of which a "consumer" is willing to pay more!)> > I think that your subsequent analysis is a

more-or-less correct analysis> of the pressures for quality in the current system. Essentially, low> quality journals are cancelled since their benefits aren't worth their> costs. But in your proposed system, the reader bears no costs, so > this particular feedback is eliminated.

The way for a reader to vote is not with his (institution's) S/L/P dollars, but with his eyes, his citations, his refereeing, and his research! This is not commerce we are talking about, but Learned Inquiry.

> You may well respond that authors will want to submit to quality journals,> a point I accept. But what does "submit" really mean in this world?> I have argued elsewhere that when publication costs were expensive,> it made sense to evaluate ex ante. Now that publication costs are> cheap, it makes sense to evaluate ex post.

Untested speculations about replacing peer review by post-publication peer commentary are a can of worms on which I've written before:

Excerpt from:
Harnad, S. (1998) The invisible hand of peer review. Nature [online] (5 Nov. 1998) http://helix.nature.com/webmatters/invisible.html
Peer Commentary vs. Peer Review
"And is peer commentary (even if we can settle the vexed "peer" question) really peer review? Will I say publicly about someone who might be refereeing my next grant application or tenure review what I really think are the flaws of his latest raw manuscript? (Should we then be publishing our names alongside our votes in civic elections too, without fear or favour?) Will I put into a public commentary -- alongside who knows how many other such commentaries, to be put to who knows what use by who knows whom -- the time and effort that I would put into a referee report for an editor I know to be turning specifically to me and a few other specialists for our expertise on a specific paper?
"If there is anyone on this planet who is in a position to attest to the functional difference between peer review and peer commentary (Harnad 1982, 1984), it is surely the author of the present article, who has been umpiring a peer-reviewed paper journal of Open Peer Commentary (Behavioral and Brain Sciences http://www.princeton.edu/~harnad/bbs.html, published by Cambridge University Press) for over 2 decades (Harnad 1979), as well as a peer-reviewed online-only journal of Open Peer Commentary (Psycoloquy, sponsored by the American Psychological Association, http://www.princeton.edu/~harnad/psyc.html for what will soon be a decade too).
"Both journals are rigorously refereed; only those papers that have successfully passed through the peer review filter go on to run the gauntlet of open peer

commentary, an extremely powerful and important SUPPLEMENT to peer review, but certainly no SUBSTITUTE for it. Indeed, no one but the editor sees [or should have to see] the population of raw, unrefereed submissions, consisting of manuscripts eventually destined to be revised and accepted after peer review, but also (with a journal like BBS, with a 75% rejection rate) many manuscripts not destined to appear in that particular journal at all. Referee reports, some written for my eyes only, all written for at most the author and fellow referees, are nothing like public commentaries for the eyes of the entire learned community, and vice versa. Nor do 75% of the submissions justify soliciting public commentary, or at least not commentary at the BBS level of the hierarchy."
http://www.cogsci.soton.ac.uk/~harnad/nature2.html
Food for thought: Would you rather have an ailing relative treated on the basis of the traditionally peer-reviewed biomedical literature, with referees selected and their reports adjudicated by a qualified, answerable Editor, or on the basis of navigating a Netnews chatgroup peppered with "articles" and "comments" by God knows who (guided by hit rates?).
[cf. http://www.bmj.com/cgi/shtml/misc/peer/index.shtml]
> Furthermore, there is> no reason to use a 0-1, publish/don't publish scale any more---much> more sophisticated systems could be used.
On this topic, see:
http://amsci-forum.amsci.org/archives/september-forum.html 1999 Thread: Independent scientific publication - Why have journals at all?

Short answer: Peer review is not 0/1, red/green light. It is an interactive, iterative feedback cycle that sometimes leads to a paper that passes the threshold for THAT journal in the hierarchy (everything gets published SOMEWHERE eventually). But referees are a scarce resource, and journal quality is equivalent to referee quality and rigour (and rejection rate).

> One scenario is for public-archiving and self-archiving as the publication> mechanism and an essentially separate system of cataloging/ranking/peer> reviewing as the filtering system.

This is already covered by the dichotomy: "U" unrefereed preprint vs. "R" refereed reprint (+ journal name "JX"). BOTH can be self-archived (and suitably tagged).

> The question then is who should pay> for the peer reviewing? I submit that it may well be the readers, due to> the incentive effects described above.

No, the readers need merely CHOOSE to search only on items tagged "R" in the free Eprint Archive. The refereeing can be provided by peer review (which ain't broke, hence don't need fixin' -- let alone replacin' by untested alternatives).

> >hv> if an organization "can't afford" access, it is> >hv> likely an accounting illusion rather than actual lack of money.> > >sh> I'd like to see the data for that, not even for all 14K journals in>sh> Ulrichs, but just, say, the top 6.5K indexed by the ISI. And please>sh> tell me the figures per journal, per institution, per country.> > See Lemberg, Richard, 1996 thesis on costs of digitization, UC Berkeley.> JSTOR did some calculations with the same conclusion, which are reported> in part by a speech from Bill Bowen, which, I believe, is available> on the JSTOR Web site.

That does not answer my question: We are not talking about the costs of digitization, current or retrospective. We are talking about how many institutions/countries can and do afford how many journals!

You focus on capturing the available money (via S/L/P), whereas I ask "Why not give it away for free for all, and pay the small remaining cost -- quality control -- out of the S/L/P SAVINGS?"

If there is no other way to free your intuitions from reader-end market thinking, run your whole argument through on advertisements: Why shouldn't advertisers give their ads only to those who can afford to pay for it?

Answer: Ads are not the right PRODUCT to think of! It is ad companies' SERVICES that advertisers want to pay for. (But before this segues into the vanity-press argument again, note that it's only an analogy; for something closer to a homology, you would have to make it the services of a quality-controller/certifier (the FDA?), and one in which the quality assessment itself is done by independent and incorruptible -- because unpaid! -- assessors [referees]!)

Stevan Harnad, University of Southampton, July 12, 1999

On Mon, 12 Jul 1999, Hal Varian wrote:

> This is what strikes me as peculiar in your position. You argue that> the dramatic change in costs will have a big effect on the academic> publishing model, and yet the same S/L/P model "is still fine" for the> trade literature.> > Doesn't it seem strange that the same change in costs will have such a big> effect in one area and a tiny effect in another?

(It's not academic vs. trade publishing, but refereed journal papers vs. everything else: including books -- both academic and trade -- and magazine articles.)

I think it has become clear why this disagreement persists. I can't bring myself to

believe that books are really a give-away literature, in the way journal-papers always have been (for their authors).

The data you adduce in support of your position consist of the statistics on how little it is that book authors ACTUALLY make for their texts, on average, and I do not contest that, but I don't think it's the decisive evidence (and NOT just because I am a psychologist, concerned only with authors' hopes!).

I think the fact that books have always been commissioned with contracts specifying royalty payments (even if in most cases the royalties are tiny or never materialize at all), whereas refereed journal articles never have been, is evidence in my favour.

So too, I believe, would be the results of the following 3 thought experiments:

How many (1) book authors would have instead signed contracts that promised them limitless public archiving in perpetuo on the Web, in exchange for signing away all royalty rights? -- compared to (2) journal article authors offered the same proposition? [Prediction: (1) few; (2) all]

How many (1) book authors would be willing to PAY (and how much) to ADD to their existing, royalty-based contracts the right to self-archive in perpetuo? -- compared to (2) journal article authors offered the same proposition? [Prediction: (1) few (and little); (2) most (and surprisingly much)]

How much would book authors demand to be PAID in order to alter their existing, royalty-based contracts, so as to add a clause that this work can NEVER be archived free for all? -- compared to (2) journal article authors offered the same proposition? [Prediction: (1) suprisingly little; (2) (more than it is realistic to imagine anyone could ever afford to offer)]

> I argue that the change> in costs will also have a dramatic effect in the trade literature with> lots of free publication of unvetted material (see the Web) with people > paying for filtering/vetting. Hence the business model for the two > literatures will tend to become more similar.

I accept that an increasing number of things, including texts, will be (and are being) given away on the Web for free, over and above refereed journal papers!

But we are talking about conditional probability here, not raw probability: What is the probability that an item will be a Web give-away-wannabe, GIVEN that it is a book, vs. the probability that it will be a Web give-away-wannabe, GIVEN that it is a refereed-journal paper?

I predict that the latter will be 100% whereas the former will be much, much lower (and even if we don't add the "in perpetuo" clause, allowing temporary web-archiving for promotional purposes); and when electronic book technology becomes more friendly to linear bed/bath/beach reading, the discrepancy will become bigger, not smaller!

As to modular "vetting" -- I haven't a clear enough case in mind, other than peer review, so I'm not sure what would be on sale here: movie reviews?

> But, as you well know, peer review varies dramatically in its quality.> There are good journals (which tend to be more selective) and bad journals> (which tend to be less selective) under the current system. If authors> pay to get published, there will be a natural tendency to increase the amount> published and reduce average quality.

Please see prior postings on peer review: The journal quality hierarchy will remain in place. Their respective peers will continue to perform (independently, and for free) as now. High rejection rates will continue to prevail at the top; vanity-press, pay-your-way-in at the bottom. And everyone will continue to know the difference, as they do now.

So the effects you describe will only be felt at the bottom of the hierarchy, where they already are now.

>sh> Heaven forfend! The worst of all possible worlds! You have to pay to>sh> read AND you have to pay to be published! Insult upon Injury!> > But you are paying for different things: authors are paying for> the hosting, readers are paying for the filtering.

Hosting expenses have not even come up. They are part of the academic infrastructure already. I don't reckon the pennies it costs to archive my papers on my institutional server any more than I reckon the pennies it would cost to archive my personal photo albums there (as many do). Same is true with self-archiving on Los Alamos, E-Biomed or CogPrints. The marginal cost per paper is negligible. Hosting costs are a red herring (for academics).

The ONLY author (institution) costs I have ever mentioned have been those of quality control/certification (QC/C).

I don't believe for a minute in reader-end QC/C costs: QC/C costs are like the Cheshire cat's smile. They are all that is left of journal publication costs once production and dissemination are offloaded on public Archives. It is not at all clear what the READER would be buying if he wanted to buy THAT! It's a service (to the author-institution); its OUTPUT -- the refereed paper -- is already in the public

archive for free!

> This makes sense> since the authors value being published (as you repeatedly emphasize)> and the readers value selectivity.

I'm afraid this doesn't make sense to me at all! I know what the author-institutions are buying: the vetting and certification of their "product." But what are the reader-institutions supposed to be buying, given that that "product" is archived for free for all?

Yes, readers value selectivity. And it is (in part) because readers value selectivity that authors value the service of QC/C!

> I think that your 1/3 figure for > refereeing process is about right (I have made the same estimates in my > papers), so it's the same total amount paid, by essentially the same people,> in either case.

Yes, but if that same 1/3 amount is paid at the reader-institution end, then only the readers at THAT institution get access, whereas if it is paid at the author-institution end, ALL readers get access (including those at less solvent institutions). And, as we agreed, that enhanced visibility is a benefit to author-institutions! And since we're talking about the same institutions spending the same 1/3 in both cases, it seems to be a choice between spending it the access-blocking vs. the access-enhancing way. What possible advantage can there be to the former? (Please don't revert to the vanity-press argument!)

> And if there are those that don't want to pay for filtering, they are welcome> to go out and search for the material they want on their own, spending time> rather than money. But if there are those that want to pay money to have> the searching and filtering done for them, why should you object?

You keep imagining, basically, a refereed literature, paid by S/L/P, plus a free unrefereed literature, whereas what I am advocating is a refereed literature (first) FREED by self-archiving (of refereed papers) and then, when S/L/P revenue dwindles because users prefer the free archive, QC/C FUNDED up-front out of 1/3 of the S/L/P savings.

Your view is simply reader-centred. You keep seeing the text as the PRODUCT instead of seeing it as like a free ad, with the quality-control/certification being the SERVICE to be paid for.

And as we saw, the only dividend of continuing to see it your way is that the very same 1/3 expenditure, paid by the very same institution, is, on your model, paid for BLOCKING access to everyone outside the author's institution who cannot

pay, whereas on my model, it is paid for OPENING access to everyone outside the author's institution who cannot pay.

> My argument is that you seem to think that you can take the existing > refereed journal literature, reverse the payment system, and have nothing > else change.> To a social scientist, this is highly unlikely. The existing payment> system influences incentives, and changing the payment system will change > the incentives.

That would certainly be true if referees were paid, but they are not! The payment is just for implementing the refereeing system.

And the implementation is ultimately answerable to objective quality indicators constraining QC/C such as impact factors; so we agree that journals that dropped standards (selected less rigorous referees, for example) would soon be caught out and would lose their place in the quality hierarchy to more rigorously refereed journals. (So much for the vanity-press argument.)

[I am conscious that I come up for a drubbing below over what I said about Learned Inquiry and commercial factors, so let me say more carefully here that QC/C implementation is not "directly" influenced by "1st order" commercial factors -- such as maximizing the number of author-institutions paying the publication fees, or even of maximizing the publication fees themselves; it's more like a museum trying to maintain its standards by collecting only the very best art -- in a system where the best artists also want their art to go into the very best museums. -- Except that they GIVE it to them rather than SELLING it!]

But I am not saying that there may not be some surprises in the transition from S/L/P to up-front QC/C. That's why I think it needs to be planned in advance. See the thread "The Urgent Need to Plan a Stable Transition" in the 1998 file of: http://amsci-forum.amsci.org/archives/september-forum.html

> > The way for a reader to vote is not with his (institution's) S/L/P> > dollars, but with his eyes, his citations, his refereeing, and his> > research! This is not commerce we are talking about, but Learned> > Inquiry.> > Hmm. I thought we were talking about the economics of learned inquiry.

You got me on that one. (I'm just a psychologist!)

> I'm not talking about post-publication peer commentary; I'm talking> about post-publication peer review, just as you describe in the following> excerpt:

But my point in the prior excerpt was that post-publication "peer review" (which I called, I think quite correctly, peer commentary, because that's what it is!) is not a

viable substitute for prepublication peer review!

It's as if all the food is put on the market and then people have to listen to the cacophony of consumers to decide what's safe to eat! I'd rather sell (and buy) my eggs already graded by reliable experts!

> Here, I think, is a difference in our conceptions. You think of the "R"> [Tag for "Refereed"] and "U" [Tag for "Unrefereed"] as being bound to> the archive. But that seems rather limited. Why not have different> organizations providing the "R"s and "U"s with pointers to the> archive?

See the analogy above. I'd rather have my eggs graded by reliable experts, ONCE. Otherwise who will grade the "experts"?

> This way you get competition for rating services, with the appropriate> incentives for keeping quality up and price down. Some reviewing> services may charge for their services and some not; let the users> decide what they want.

But then who will rate the services for me? And meanwhile, what do I do about treating my ailing grandmother, or just getting eggs for tonight's omelette?

I think the current QC/C, though it could stand some improvement, is good enough so I would be satisfied with freeing the current journal literature SUCH AS IT IS now. QC/C reform schemes can be tested empirically, but for now, I want to stick with the current egg-grading methods...

> Certainly figuring out which institutions can afford journals> involves looking at how much they are paying now vs how much they would> have to pay for online access.

Correct, but we have already agreed provisionally on the 1/3 of current S/L/P expenditure figure and we know the bottom line: That SOME won't be able to afford it. But if those who can afford it pay it up-front, instead of as S/L/P, then all the have-nots will get the access anyway, and everyone will be better off.

> > You focus on capturing the available money (via S/L/P), whereas I ask> > "Why not give it away for free for all, and pay the small remaining cost> > -- quality control -- out of the S/L/P SAVINGS?"> > This is misreading of my claim. I don't have any quarrel with giving> the writing away for free, and having the authors pay for posting.> ...The question is who will pay for the filtering?

Posting WHAT? I am talking about posting (self-archiving) the REFEREED (= "filtered") papers. (And there is no cost to speak of for the self-archiving, just for the refereeing QC/C.)

> Just as the authors benefit from being published,> and will pay to do so, the readers benefit from filtering and will pay> for high quality filtering.

(Let me first us lay to rest the author "payment" for self-archiving, because there isn't any.)

Now, self-archiving WHAT? Unrefereed preprints? Fine. But why would I want to stop there? What do I want everyone, everywhere to see: just the raw draft, or the accepted, certified final draft? The latter, of course.

Now it's true that readers prefer the final draft too, and it's true that the QC/C will cost 1/3 of current S/L/P and will be paid by my institution either way. But if it is paid up-front, I, the author (and my institution), get the further benefit of infinite reach for my work. Whereas the only advantage to keeping payment on the S/L/P end is some hypothetical one that I can't even quite articulate! (Again, it sounds like no contest to me!)

> Of course, if the authors are willing to pay for the filtering (as you> assume), there is no need for the readers to pay. But the incentives> are poor in that system---I think that it would tend to degenerate into> the vanity press you correctly deride.

I think that is as far as we will get with this one, I'm afraid. We agree on the amount. We agree on who pays (the author's institution either way). But if it is paid up front I claim that it frees the literature for all without loss of quality, whereas you claim that it will compromise quality.

I think this can only be settled empirically. I will continue to try to promote self-archiving (of both unrefereed preprints AND refereed reprints). I expect you will want to be cautioning against the latter...

> In your system, the authors pay the journals to have their papers published.> The analogy is that the drug companies would pay the FDA to have their> drugs approved. Do you see a problem with the latter business model?

I actually have no idea how the FDA works; perhaps I shouldn't have mentioned it. It was just meant to be an example of a (reliable?) quality controller. But I do know how peer review works, and I know referees are not paid, and as long as they are not, they are incorruptible (incorruptible by how the implementation of peer review is funded: I am not claiming peer review is perfect, or could not be improved).

Peer review is what it says: Specialised work is reviewed by qualified fellow specialists with no financial incentive. I would have hoped that was how drugs

were reviewed too, but of course there is a critical difference there: Drugs (like books, and unlike journal articles) are produced to be SOLD not to be given away. If you want to look to a source of corruption, look there!

Stevan Harnad, University of Southampton, July 13, 1999

On Tue, 13 Jul 1999, Bob Parks wrote:

> 1. xxx.lanl.gov has about 100,000 papers and that archive does not seem> to have reduced the number of journals in physics, nor the quality of> the scientific literature. Hence we have at least one strong piece of> evidence that 'free access archiving' will not lower the quality. I> don't know of any evidence showing that quality has been lowered in> physics or elsewhere.> > 3. xxx.lanl.gov seems to have conditioned its audience to 'filter'> relevent articles from the large number of submissions. I would guess> that works much like the usual filtering process that any academics use> for 'hard copy working papers'.

The filter is even simpler than that: "R" (refereed journal, and perhaps journal name "JX") and "Author Name."

The peer review proficiency of the journals in question takes care of the rest.

> 4. When we have citation-linking for all scientific literature> http://www.princeton.edu/~harnad/citation.html it will be natural> and easy to 'value' writing - namely by the number of citations (and> possibly the 'quality' of citations). Such citation criteria are> already used in promotions and salary (at least in my small biased> sample). One can argue whether quality is better determined from> citations than from knowing that two or three referees and an associate> editor have passed judgement.

Fallacy: Apart from the limitations of citation metrics, the validity they do have is COMPLETELY parasitic on the fact that the papers in question are published in peer reviewed journals. The invisible hand of peer review is behind them. Hence they would implode if that hand were withdrawn.

Impact factors (like peer commentary) are a SUPPLEMENT TO, not a SUBSTITUTE FOR, peer review. (And peer review is not a go/no-go filter; it is an interactive, iterative, corrective process of submission, feedback, revision, resubmission, etc. between author and peer reviewers, adjudicated by a competent, answerable peer Editor: This is no passive numerical filter or box score on any blind metric.)

Harnad, S. (1998) The invisible hand of peer review. Nature [online] (5 Nov. 1998) http://helix.nature.com/webmatters/invisible/invisible.html
http://www.princeton.edu/~harnad/nature2.html
> 5. As an economist, I would have to argue that the resources devoted> to refereeing are misallocated because they are not compensated> directly. In the current journal model, there may be too much> refereeing (or there may be too little).

Peer review, like democracy, is not without its imperfections. And no doubt there exist ways to improve it. But those ways must first be found and tested and demonstrated to improve it -- especially the most radical one, of abandoning it entirely, in favour of self-archiving of unrefereed preprints alone.

Note that I do NOT advocate the latter: I have always aimed (subversively) at the self-archiving of the REFEREED paper, not just the unrefereed one. That will be tantamount to freeing the current peer-reviewed literature, SUCH AS IT IS (warts and all).

http://www.arl.org/scomm/subversive/toc.html

We can worry about ways of fixing the warts independently; but the powerful and proven benefits of self-archiving should NOT be linked in any way to speculative and untested notions for improving peer review -- least of all abandoning it altogether.

> If 'free access archiving'> means the end of journal refereeing as we know it, I am not sure> whether I (at least) could argue that there is a social gain or loss.

There is every reason to believe it would be an enormous loss, throwing the baby (a reliable research literature) out with the bathwater (access barriers to that very literature).

To put it another way, it's READER-ACCESS barriers that have to go; AUTHOR-ACCESS barriers (into the certified refereed corpus) must stay.

> Referees might spend their time writing/reading rather than refereeing,> which could result in better scientific literature than what exists> with their time spent refereeing. I am not arguing that refereeing has> no value, only that we do not know what that value is, and that> whatever that value is, it is not compensated (directly at least).

We know the value of the refereed literature, such as it is; every editor knows what

raw submitted manuscripts look like (90% of which will be rejected, if it is a high quality journal; and most of the 10% that are finally accepted will look nothing like their initial drafts, for they will have gone through the iterative corrective feedback cycle of peer review mentioned above).

It is the difference between these two literatures that is at issue (and the difference is even greater than that, because even those raw submissions are prepared with the PRESUMPTION of answerability to peer review -- yet another manifestation of its "invisible hand").

No, human nature being what it is, without answerability it quickly regresses toward the anarchic levels of the chat-groups NetNews, that Global Graffiti Board for Trivial Pursuit. (And neither the Pandemonium of post hoc "peer" commentary nor the still poster-hoc feedback from "citations" can provide that answerability. Pity the reader who has to navigate a chaotic corpus like that.)

> IMHO, the only reason to sort it out is to determine, given the goals> of the esoteric author (a term I like), whether 'free access archiving'> will lower or raise the quality of scientific literature.

IMHO, it would not be good empirical practise to test whether the FDA is really protecting our healths by scrapping it and seeing what happens!

The goal of self-archiving is to free the refereed literature from access barriers, not to free it from refereeing!

The way to test variants on or alternatives to peer review is locally, not globally: Do you know of any local experiments? I do, and as far as I know, it is not yet faring too well!

http://www.bmj.com/cgi/shtml/misc/peer/index.shtml

> Again, the point should be whether the quality of the scientific> literature is harmed by 'free access archiving'....> In the NO-JOURNALS world of 'free access archiving'> we write to attract others attention, and citation. Rather than> writing for three people (two referees and an editor) we now have to> write for a larger audience and have to write to attract a readership> (rather than attract an editor/referee). I don't see that deters us> writing. The goals of fortune and fame remain, its just the journals> no longer have a Faustian GRIP on us.

And it all becomes a vanity press, with no sign-posts for the poor reader and user as to what, in all this unregulated soup, is fit for consumption!

> The current business 'model' for scientific literature is, well,> absurd. Editors are

mostly not directly compensated, and those who are> are not compensated at the market value of their time. Referees are> not compensated ($35 or $50 is not compensation). Authors are not> compensated at all directly.

Correct. But it is also what vouchsafes us our current refereed literature, such as it is. Let us free THAT before toying with any notional improvements -- INCLUDING referee payment, which is potentially corruptive: They referee for free now, and that's just part of the system, such as it is.

The system, with its authors giving their papers away free, and its referees giving away their services for free, is best described as ANOMALOUS, not ABSURD. What is absurd is to continue treating it according to the access-blocking trade-model, instead of freeing it (from S/L/P) for the reader as well.

Here is an argument that could be invoked against me, but I don't think it's valid:

"Fine, I take you at your word. 'Don't tamper with the system, just free it.' Now I will show that that admonition is self-contradictory: I agree that altering or abandoning refereeing would be tampering, and would put quality at risk. I agree that paying referees would be tampering, and would put quality at risk. But then, isn't author self-archiving tampering too, and putting quality at risk? Might it not bring down the entire system, destroying the revenue base on which the quality control is built? Is it, therefore, not one of those untested "reforms" of peer review against which you always inveigh? Q.E.D."
My reply is simple: Authors have always given away reprints of their papers for free. Self-archiving simply increases the scale of this. And the waters HAVE been tested, for close to a decade now, by Los Alamos, and no sign of diminished quality has emerged (as Bob notes above).

http://xxx.lanl.gov/cgi-bin/show_monthly_submissions

So the free waters are safe for peer review. (And if and when another revenue source must be found for continuing to fund it, the up-front redirection of 1/3 of S/L/P savings to institutional publication costs is ready as a natural source for it.)

Hal Varian's worries about possible knock-on effects elsewhere in the quality control system are not entirely without basis, but there doesn't seem any compelling reason for alarm either. Let self-archiving proceed apace.

> So the university pays us to> author/edit/referee and then buys our product back from a 'publisher'.> Resources must be misallocated in that model. If our current world was> a 'free access archiving' with citation valuations (rather than journal> valuations), proposing such a business model would, well, be absurd.> We need to unshackle ourselves from the current journal Faustian Grip,> from that mental

model of the world, and proceed ahead. Nor should we> consider that scientific literature fits into other 'information'> products.

This seems a bit garbled to me, because it conflates freeing the refereed literature form S/L/P with freeing the literature from refereeing.

> Much of the discussion between Hal and Stevan side steps into business> models (ignoring any further words on motivations of authors). So what> is the business that requires a model? Production of (quality)> scientific literature.

Actually, the "business" is making an impact on research with one's research. The literature is just a means, not an end. Hence my analogy with ads.

> Must that be tied to the elsevier et al (I use> elsevier in lower case as a generic for profit and non-profit> presses)? elsevier does not pay the authors, nor the referees nor the> editors which is 95% to 99.9% of the real cost of producing the> literature. In the 'free access archiving' world, we do not need to> wory about whether elsevier survives. We do need to worry about the> quality of the scientific literature, and elsevier itself does not> provide that quality control. Editors and referees do. Citations do.

Peer review does; citations do not! Elsevier (and others) implement peer review. That will always cost something -- but nothing like what the whole papyrocentric, S/L/P-based system costs now.

> Whether universities are willing to compensate us for editing and> refereeing without the elsevier label is an open question (especially> if the citation linking proposal becomes fact). In fact it is a> question which should be asked - how much refereeing should be done?> If we have citations, do we need refereeing and editing? It is not> that refereeing and editing do not increase the value of an article, it> is whether the correct amount of resources are devoted to that> activity, and whether citations (or similar) would be a more cost> effective way to discern the quality (for promotion, tenure, etc.).

Vide supra. It seems to me naive in the extreme to imagine that delayed post-hoc citations can substitute for the substantive, interactive, quality-control process of peer review.

> Imagine a world with 'free access archiving' without journals. How> does one get promoted? Citations and review letters. Citation> analysis would be free, and universities would have to compensate for> outside review letters. Would that really change the quality of> scientific literature - for the worse? Not in my mind.

Have a peek at Usenet/Netnews for a glimpse of where things would head, human

nature being what it is...

Stevan Harnad, University of Southampton, July 14, 1999

On Tue, 13 Jul 1999, ransdell, joseph m. wrote:

> There is surely no question but that the significance of LANL is> commonly thought to lie in its success as a preprint server, not in the> many other facilities added or being added, and it is reasonable for> people to think that in adopting it as a model you are doing so because> of that for which it is famed, not for other features of it of no> special interest as innovations.

It is a historical fact that LANL begin as a preprint distribution network among 100 high energy physicists. It is a further historic fact that it rapidly grew to encompass more and more authors and users, covered more and more of physics and beyond, and came to include the refereed final drafts too. If we are going to take LANL as a model -- as we should -- let's make sure we use the full current model and not just the barest initial conditions!

And the fact is that the "other feature" (entirely predictable if the rapid growth of the scope and scale itself had been predictable) -- namely, that it ain't just about preprints any more, but is rapidly growing into the WHOLE PHYSICS JOURNAL LITERATURE -- is turning out to be (in my view) even more important than LANL's preprint function (though that continues to be very important too), because it demonstrates the revolutionary possibility of freeing the journal literature for once and for all, across disciplines and around the world, to the eternal benefit of Learned Inquiry through SELF-ARCHIVING.

So self-archiving is the "model" and the take-home message of LANL, and not merely, or primarily, the self-archiving of unrefereed preprints.

> The appeal to the invisible > hand does not lessen the import of the fact that the LANL system uses> unfiltered material, and it is bound to occur to many people that there> is not, after all, any rule that requires people to simultaneously> submit the paper to a referee: that is just a custom at LANL at best.

Well, isn't it odd that today, when LANL is up to 20,000 new papers annually, that that alternative still does not seem to have caught on, and Physics journal submissions continue to proceed apace?

Nor is it difficult to see why: For vanity publication (I am afraid I must persist in the pejorative usage, particularly in this context, to stress precisely what is at

issue) does not count very much toward the promotion of either one's findings or one's career. In general, the academic reward system relies on reliable, credible quality control and certification -- as do the users of the literature (which, as I keep recalling, is still both constrained and sign-posted by the invisible and visible hand of peer review); it is not only promotion committees, but readers who would be overwhelmed and helpless without it.

The preprint dimension is a splendid, indeed revolutionary supplement to the classical system: The scope and scale on which new findings and ideas can be disseminated immediately, even before being vetted for their quality and reliability by experts, is exhilarating and will no doubt increase the scope and scale of Learned Inquiry -- although I don't think it will do see nearly to the degree to which the even more revolutionary feature, the freeing of the refereed corpus, will. And the interactive possibilities -- commentary and peer commentary, on work both before and after peer review -- will be revolutionary too.

But quality control must persist -- and along the classical lines (until an alternative that does at least as good a job is first found and tested): The invisible hand of peer review must continue to be made visible, reliably sign-posting the literature for the otherwise hapless Hitch-Hiker in the PostGutenberg Galaxy, be he a member of a promotion committee or just a journeyman researcher trying to contend with the swelling literature.

> The reason I am pushing this to the fore is that I notice that you don't> any longer seem to regard the preprint server as an important part of> it. When the immunologists responded in tones of outrage to precisely> that feature of the E-biomed model you quickly advised the NIH people> not to worry about implementing the preprint server: that could be> figured out later; what is important is the refereed literature,> anyway.

I do consider the self-archiving of preprints to be extremely important and desirable. I was responding there to the special case of CLINICAL MEDICINE (in the context of the E-biomed initiative), where public health might be at risk from wide distribution of unrefereed claims.

I actually believe that the clinical community will be able to set up reliable safeguards without too much difficulty -- public-health vetting that is short of peer review but filters out dangerous errors and quackery from the unrefereed preprint sector of the clinical portions of the biomedical literature in E-biomed. And the "R" (for refereed; and "JX" for journal-name) sign-posts will also help to distinguish what is safe to take seriously in the clinical literature. (I even suggested, tongue-in-cheek, that a cigarette-like "potential health hazard" tag could accompany all unrefereed clinical preprints too).

The option to "forget about preprints for now" was hence only directed at clinical researchers, and only to dispel the red herring of public health risk as a rationale for opposing E-biomed self-archiving simpliciter: For the REFEREED clinical literature is certainly no health hazard (one hopes!), and hence should be self-archived forthwith...

I do not believe that the special case of this (soluble) health-risk problem for the clinical literature generalizes to the literature as a whole.

> > ... it [the invisible hand] constrains preprints to be> > drafted on the presumption of answerability to classical> > peer review, through conventional journal submission,> > usually concurrent with archiving.> > What constraint? What presumption? The constraint on the author is> that what he or she writes is to be in agreement with the facts, as> these are ascertained in the course of inquiry, not as they are> established through agreement with peer reviewers. The invisible hand> is just an awareness of a future contingency that can be handled in more> than one way or even ignored...

How much easier my job as Editor would be if all that authors had to do was hold their hands to their hearts and state that what they have written is sound, in conformity with the facts, competent -- in short, worthy of the finite reading time of every busy researcher attracted by the title who, in the old days, would have had that limited reading time guided by the intrepid experts who had first done the hard interactive work of getting the promising papers ship-shape and certifying them as such!

> I must say that the resort to the concept of> the "vanity press" seems to me just gratuitously contemptuous of people> and their motives and is very misleading as regards what is actually> happening when people try to communicate. How can we implement a> communications revolution with the use of simplistic denigrations like> this?

You know what they say about good intentions! But to change the metaphor: would you like to choose your daily food (or drugs) not after prior FDA vetting, but... what? post-hoc opinion polls?

Stevan Harnad, University of Southampton, July 15, 1999

On Wed, 14 Jul 1999, ransdell, joseph m. wrote:

>sh > [SELF-ARCHIVING] is the "model" and the take-home message>sh> of LANL, and not merely, or primarily, the self-archiving of>sh > unrefereed

preprints.>> what is of special interest [is LANL] as a means of primary> publication, with unfiltered preprints as the basic publication items.

As long as LANL co-exists, as it does, with a refereed journal literature, and virtually all of its contents are concurrently submitted to, and eventually destined for, that literature, it simply cannot be described as a means of primary publication. It is merely (among many other things, and I am here in no way belittling LANL but rather insisting that there is much MORE to LANL than this) a faster means of pre-publication.

Yes, there are remarkable things being done with that added lead time, and with the fact that it is all available online and for free everywhere, but that's far from the whole story. And the take-home message is certainly NOT that the rest (the submission of all those papers for peer review, the reliable follow-up of the revised, refereed drafts) is in anyway dispensable; on the contrary, the power and success of LANL are completely parasitic on that invisible and subsequently visible quality-maintaining constraint of classical peer review.

Here is my empirical prediction: Eliminate the classical peer review and LANL will devolve into the anarchic, uncharted and un-navigable anarchy of Usenet's NetNews (as would any domain of human endeavour if it ceased to be held accountable to quality standards.)

(I hope it is clear that the current quality and usefulness of LANL preprints is NOT evidence against this prediction; the prediction is simply not being tested while the invisible hand of peer review remains in place. It is pure speculation that LANL could continue to be what it is, if instead of being just a SUPPLEMENT to peer review, as it is now, it became a SUBSTITUTE for it.)

> you are arguing... that the fear of filters future takes the place> of filters present. But there is no reason why the physicists> depositing in the archives should fear future filters when the> publication of their work in an unfiltered form can provide the basis> for corrective improvements by eliciting critical feedback.

Let me count the ways:

(1) It is not "fear" but the knowledge and expectation of accountability (to editors, referees, promotion committees, granting agencies, etc.).

(2) Peer review is not and never has been just a go/no-go "filter": It is an interactive, dynamic, corrective feedback process, sometimes proceeding through several iterative revisions and re-refereeings, leading (if successful) to a certification that is more a go/no-go sign-post for the (otherwise besieged and bewildered) READER than the author; for the author it has been a much more

continuous and multidimensional upgrading process.

(3) Let us lay to rest at once the fantasy that in a world in which all there is is self-archived raw manuscripts, the (besieged, bewildered) "peer community" can be counted on to somehow sift through all that anarchic sludge (AT LEAST as conscientiously as classical referees did, when specifically selected to do so by a competent and respected peer editor, to whom they knew the author would be accountable in acting upon their referee reports) and FIND what needs the feedback, PROVIDE that feedback, and have it ACTED ON in such a way as to turn that sludge into something more like the refereed literature of today: On the contrary, without the invisible hand, one could not even rely on the raw sludge turning into what the LANL PREPRINT literature looks like today!

> ...there is a real sense in which peer review does occur at this> point, subsequent to the act of publication rather than preceding it.

Yes, GIVEN that the invisible hand is in place. But what if it were not? Vide supra.

> The people who download the preprints are peers and they do critically> review it. ...to avoid merely verbal dispute let us call it something > else: "critical peer response". ...it is not the invisible hand of peer> review that accounts for the maintaining of quality in the LANL> preprint server system but rather the prospect of encountering the > manifest reality of critical peer response.

Well, we are clearly at a point of factual and inferential disagreement here. "Peer response" to the unrefereed preprints is certainly an important, new, and even revolutionary dimension to the overall self-corrective process of science (which does not, by the way, stop AFTER publication either), but I don't for a moment believe that that is the PRIMARY force keeping LANL's preprint sector honest. That is the invisible hand. (And LANL's at least as important REprint sector is the visible hand!)

http://cogsci.soton.ac.uk/~harnad/Papers/Harnad/harnad90.skywriting.html

> That the desire for acceptance in the full-blooded sense is the> motivating factor underlying quality in preprint publication, is a> testable hypothesis, I would think, though some may think it too> obvious to require testing.

On the contrary: As I said in a prior response, this "hand-on-heart" substitute for classical quality control would decidedly need to be tested to be taken seriously. The road to Usenet is paved with good motivations...

> ... the reason why> it works for those fields is that the people who are there to

respond> to a preprint publication via the server are peers who are working at> the leading edge of the field and who recognize that what is made> available there via the server is to be treated as primary publication> and responded to accordingly.

I will await more systematic and empirical analyses of the remarkable historical developments in physics and preprint self-archiving in this decade before drawing conclusions about why it happened in physics first, what may or may not be unique to physics in it, and what relation it may have to the refereed physics literature, or the need for it.

ONE empirical hypothesis seems worth testing already: Will the value of PREprint self-archiving generalize to the rest of the disciplines?

And one obvious LOGICAL implication of the fact of LANL does not even require testing, but calls for immediate APPLICATION now: The value of (refereed) REprint self-archiving -- freeing the refereed literature online -- WILL generalize to the rest of the disciplines.

> Paul Ginsparg did not create the community of> preprint users that is the underlying reality of the science the server> system at LANL serves...> Thus neither NIH nor the BMJ nor the Caltech people have it within their> power to duplicate the Ginsparg achievement: it can't be duplicated by > building archives and inviting people to use them, but only by finding> existing practices, if there are any, that can benefit from the use of a> system like this and enabling them to do so.> ... if a field is not coherent and mature enough in its pre-existing> practices to use a preprint server effectively as a means of primary> publication, it just will not work...

Too many inferences and interpretations here. Here is what can be said with confidence: LANL has shown that PREprint self-archiving would be a promising thing to try in other disciplines too, and REprint self-archiving would be a sure thing to succeed in other disciplines too.

> Much of your response assumes that you have to defend peer review. But> I haven't questioned its importance or validity at all so there is no> reason for me to respond to that part of it.

Implicit in your interpretations about what is going on in LANL, and where it is going, are assumptions (in my view, incorrect ones) about the causal role of peer review in all this. I have simply made those assumptions explicit, along with the evidence and arguments against them.

Stevan Harnad, University of Southampton, July 19, 1999

This is a reply to Arnold Relman's much-cited NEJM critique of the NIH/E-biomed proposal.

> The NIH "E-biomed" Proposal -- A Potential Threat to the Evaluation and> Orderly Dissemination of New Clinical Studies>> *The New England Journal of Medicine* -- June 10, 1999 -- Vol. 340, No. 23> [EDITORIAL by Arnold Relman]

In summary, Dr. Relman has two objections to public archiving in E-biomed, one of them justified, but easily accommodated (as has already been pointed out repeatedly), and the other neither justified nor even, on the face of it, coherent.

The first objection is that public archiving of some unrefereed biomedical papers (chiefly clinical ones, or those with clinical implications) might pose a public health risk.

This is true, but there is no reason whatsoever why a safe and reliable system cannot be designed and agreed upon that would (1) vet all unrefereed E-biomed submissions to exclude what needs to be excluded to protect public health and to maintain whatever special standards are agreed upon for the unrefereed clinical literature in E-biomed, (2) clearly tag all E-biomed contents as REFEREED (with journal citation) vs. UNREFEREED, and even (3), if need be, prominently pre-pend a "health warning" to all the unrefereed papers in E-biomed.

These rather obvious cautionary measures for the unrefereed clinical literature, already partly sketched in the original E-biomed draft proposal, should also be weighed in light of the fact that public self-archiving on the Web is an option that is ALREADY open to authors in a variety of ways, entirely apart from E-biomed. So unrefereed clinical preprints can and do already appear on the Web, and are retrieved for one and all by search engines. The unrefereed sector of the E-biomed archive will have the virtue of filtering out substandard preprints, tagging all preprints clearly as such, in contrast to refereed reprints, and, if necessary, even flagging them with a warning as potentially hazardous to health.

But the most important reply to this first objection is that it presents no reason at all to delay the immediate implementation of E-biomed as a means of freeing the biomedical literature for one and all. For the self-archiving of REFEREED papers could begin at once, with no attendant health risks, even as the safeguard systems for the unrefereed clinical papers were being designed and tested. (Moreover, the unrefereed NON-clinical preprint sector of the E-biomed could likewise start at once, along the lines of the unrefereed preprint sector of the Los Alamos Physics Archive.)

Dr. Relman's second objection is that the public archiving of even REFEREED papers is unsafe too, in much the same way; they can only be safely published by the journals, in the traditional way (through print publication). Premature self-archiving, without the "benefit of simultaneous expert commentary and interpretation" would cause "confusion and misunderstanding."

Here I am afraid Dr. Relman is himself misunderstanding the new medium, and perhaps confusing the public archiving of refereed findings with premature press releases of unrefereed findings. The latter are indeed dangers to public health, for the same reasons as discussed above, but there is no corresponding risk for REFEREED findings -- otherwise even the traditional publication of refereed papers would have to be held back until it had had the "benefit of simultaneous expert commentary and interpretation"!

So the second objection has no basis whatsoever and can and should be discounted completely. Refereed findings can and do elicit peer commentary. They will certainly do this at least as effectively in the online medium as in paper. But peer COMMENTARY never has been a reason for holding up the PUBLICation of refereed findings: Peer REVIEW (i.e., refereeing) is supposed to have seen to it that they were ready to appear once they met the quality standards for acceptance.

So there is nothing at all in Dr. Relman's critique that should hold back the immediate implementation of E-biomed for either the refereed sector or the nonclinical unrefereed sector; his concerns about the vetting of the unrefereed clinical sector are legitimate, but were already explicit in the first draft of the E-biomed proposal, and can and will be satisfactorily accommodated.

I now proceed to quote/comment mode:

> The great majority of readers and users of the clinical literature are> practicing clinicians, not working scientists. They often know little> or nothing about the methods of published studies, and they depend on> the accompanying editorials in clinical journals to help them interpret> the data and place the studies in the context of their own practices.

If clinicians do not use published studies without consulting concurrent editorials then there is no reason they should not continue this practise when all studies are archived in E-biomed. They can continue to first consult editorials, whether they appear only in the print journals or are likewise archived in E-biomed. E-biomed provides the added option of enhanced access to some or all of the literature when and where it was lacking: So what?

> New clinical findings often attract wide public attention, and patients> need advice from their physicians on the relevance of such findings to> their own

medical problems. The release of important new clinical> findings can have an immediate social and economic impact and can> affect public policy.

This is a non sequitur.

> The best way to protect the public interest is through the existing> system of carefully monitored peer review, revision

Correct, and irrelevant, as peer review will proceed apace for the refereed sector of E-biomed.

http://www.cogsci.soton.ac.uk/~harnad/nature2.html

> and editorial commentary in journals

Editorials too will continue to be written, presumably, by whoever wrote them in the past, and will continue to be available to clinicians requiring them in order to interpret refereed clinical studies. There is no contingency here. (Perhaps refereed clinical articles too require a warning label: "Do not use without consulting editorial"...)

> and by timing public disclosure to coincide with the date of journal> publication.

This, unfortunately, has to be called the nonsense that it is! If it is not merely a reflexive bid to safeguard journal primacy and revenue (which would be deplorable, and one hopes that that is not what motivates it), then it is merely an expression of a superstitious adherence to completely irrelevant and obsolete features of the print-on-paper era for journal publication.

There is no need whatsoever for an author to hold back his REFEREED report once the final draft is done and accepted. The rest is just the unfortunate retardation of a bygone papyrocentric era.

> Mistakes, inaccuracies, and misinterpretations in clinical research> pose a far greater risk to health and the public welfare than do errors> in basic-science research. A system that allowed immediate electronic> publication of new clinical studies without the usual careful process> of peer review and revision would be risky at best and might well fill> the clinical data bases with misleading and inadequately evaluated> information.

This is a false opposition (conflating the two objections into one): We are talking about the REFEREED literature here.

> Even if E-biomed were to eliminate the second pathway and accept only> work

that had passed full peer review and revision by journal editorial> boards, immediate posting on the Web site before publication would> still be problematic, because the information would be made public> without the benefit of simultaneous expert commentary and> interpretation.

Repetition unfortunately does not strengthen this argument. Besides, for those papers that would benefit from simultaneous editorial commentary, there is no reason that synchronization cannot be arranged in the Archive too. (In other words, this entire issue is trivial, and a red herring.)

> The few weeks saved between acceptance and print publication would not> justify the confusion and misunderstanding that would often attend the> immediate electronic posting and subsequent publicizing of clinical> studies.

Again conflates the two objections, and sounds rather hyperbolic and alarmist as well.

(What proportion of the refereed clinical literature is accompanied by a concurrent editorial? Why can't similar arrangements be made for this subset in the new medium too? There could even be two different formal acceptance criteria for referees if need be: R = "accept unconditionally." RE = "accept only on condition of being accompanied by editorial." Is this a failure of imagination or animus against the new medium for some unstated reason?)

> And in any case, policies adopted by the Journal, and many other> leading clinical journals, already allow any studies with urgent major> implications for public health and safety to be released immediately> after final editorial review and acceptance.

And so...?

> most clinical journals already have their own Web sites, which do much> of what is proposed by E-biomed.

No doubt. But here I think we come to the heart of the matter (and perhaps to those "unstated reasons"), for those Web sites happen to be accessible only via Subscription/Site-License/Pay-Per-View (S/P/L), the very access barriers from which self-archiving is meant to free the literature.

In fact, the one thing "proposed by E-biomed" that no proprietary journal Web site can or will do is to free the literature for one and all, everywhere, forever.

> In addition, on-line data bases such as Medline are regularly used to> search the published clinical literature. Electronic data bases should,> and will, continue to

grow in clinical medicine

Except that alas many of these secondary providers do this for a fee, and none retrieve the full texts for free (because most are not yet self-archived, hence not yet available free).

But it is a foregone conclusion that once the entire biomedical corpus is self-archived in E-biomed for free, there will be (free) search and navigational capabilities that will threaten the niche of commercial secondary providers even more profoundly than they do the niche of primary journal publishers.

(The latter will be able to downsize and restructure so as to continue to provide the service of quality control -- peer review, editing, certification -- paid for by the author-institution out of a small part of the institutional S/L/P cancellation savings, but it is not at all clear what the secondaries and tertiaries will be providing in this new PostGutenberg world.)

http://www.cogsci.soton.ac.uk/~harnad/citation.html

> but they cannot replace the essential functions of peer-reviewed> clinical journals.

E-biomed cannot and will not replace the journals' essential function of peer review. But that is the ONLY function that journals will continue to provide.

http://www.cogsci.soton.ac.uk/~harnad/nature.html

> I imagine that the proponents of E-biomed would reply that there is no> intention to replace peer-reviewed journals. As long as accepted> manuscripts were posted promptly on the E-biomed Web site, the NIH plan> would not prevent the peer-reviewed clinical journals from continuing> to review and publish original research articles as usual, together> with whatever additional editorial and educational material they chose.> Journals could also continue to maintain their own Web sites if they> wished.

That would indeed be the quite obvious and correct response (indeed, it has essentially been made above). Journals are free to continue to sell an S/L/P version -- as long as dubious objections like the above ones are not used as an excuse for delaying the optimal and inevitable solution of self-archiving the free online versions of their (refereed) papers by their authors -- the same authors who have given those same papers to the journals for free.

Only the improvement and certification added by peer review needs to be paid for, and once S/L/P is no longer viable, institutional S/L/P savings can amply cover

those quality-control/certification (QC/C) costs up-front on the author-institution end, instead of at the access-blocking reader-institution end. But meanwhile E-biomed will also allow authors to give away their refereed eprints to one and all for free.

> That response, while technically true, ignores the probably> disastrous effects of E-biomed on journals. A flourishing E-biomed> system that included clinical studies would very likely reduce the> submissions, paid circulation, and income of most clinical journals> enough to threaten their survival. Were this the price to be paid for a> much better and less expensive clinical publishing system that would> serve physicians and the public at least as well as the present> arrangement and that would clearly facilitate the work of the clinical> research community, I would take a more favorable view of the proposal.> But I do not believe that most of the important functions of> peer-reviewed clinical journals can be adequately replaced by E-biomed.> And cost savings, noted by Varmus as one reason to adopt E-biomed, are> not an issue with the leading clinical journals. In general, they are> far less expensive than most basic-science journals.

Now it is becoming clear that concern about S/L/P revenue may indeed be what is behind these objections.

As noted, of Dr. Relman's two sole substantive objections here, the first only concerns unrefereed clinical preprints, and is a valid objection, but it is also readily accommodated by E-biomed. The second objection is invalid.

Is the revenue objection a third one? And is the fact that clinical journals are "less expensive" a justification for continuing to hold this literature hostage to S/L/P access tolls? Perhaps the simplest answer comes from putting the question in principle to the authors of all those clinical papers:

"Now that it is possible to make your refereed paper available online to everyone and everyone everywhere for free by self-archiving it in NIH's E-biomed, do you nevertheless prefer to continue instead to restrict access to it to those individual and institutional subscribers who can afford it -- even though you don't get a penny of the revenue and have always given away the reprints to all requesters for free, just as you gave the paper itself for free to the journal (to sell), presumably because your sole objective was to report your findings to one and all, once they had been approved and accepted by peer review?"

No, I don't think that the goal of sustaining journals' current revenue streams and modera operandi can or should persuade authors to keep their research findings behind a financial firewall that is no longer necessary -- particularly as there are

obvious alternative ways for journals to recover the surviving costs of their sole remaining essential service -- QC/C -- out of S/L/P savings, once they have downsized and restructured to provide that one essential service alone.

This "disastrous effect" sounds like a highly beneficial one for the research community (both basic and clinical), and it sounds like something the journals could successfully adapt to if they try, in the interests of the research community they serve.

> A final worrisome aspect of E-biomed is that its proposed organization> and management are so complicated as to raise doubts about its ability> to function. It would be a huge conglomerate of different scientific> fields, journals, editorial boards, and other "interested parties,"> overseen by a necessarily very large and disparate governing board.

This was indeed a weakness of the original E-biomed, but it is all quite easily and trivially remediable once it is recognized that E-biomed, like Los Alamos, is merely a reliable, permanent infrastructure for the SELF-ARCHIVING of all refereed and unrefereed papers by their authors in the first instance, and eventually a facility for official journal overlays, authenticated by the journals themselves.

http://www.nih.gov/welcome/director/ebiomed/com0509.htm#harn45

No new "complications" over and above the tried, tested and spectacularly successful features of the 8-year-old Los Alamos Archive are involved -- other than the special measures for the unrefereed clinical sector called for by (among other things) Dr. Relman's own sole valid objection!

> It takes a lot of work by editors to supervise a high-quality> peer-review system.

Correct; and this is precisely the work that the QC/C charges will cover out of the S/L/P savings.

> Even the simplified, two-reader system of approval envisioned> as the alternative to editorial review by a journal would prove to be> much more complicated than expected. How would differences between> reviewers or between reviewers and authors be adjudicated? Suppose a> reviewer's approval was conditional on suitable revision or correction> of the manuscript. Who would oversee such negotiations? And who would> be responsible for vetting the contentious issues that might arise> later concerning corrections and commentary? All such functions are now> carried out by the editors of peer-reviewed journals. I do not see how> any system concerned about the quality of clinical data and their> impact on the public health and medical practice could afford to ignore> these questions.

This is conflating peer review (which will proceed exactly as before) with the vetting of the unrefereed clinical preprint sector, a special case that will be handled as the peers at NIH see fit, but should not be confused with peer review, which will proceed apace.

> In clinical research, the best way to handle new data is to require> rigorous peer review before their dissemination and, with few> exceptions, to post the results in electronic data bases only after> they have been published in carefully edited, peer-reviewed journals.> That is because prepublication evaluation of the reliability of> clinical studies and impartial assessment of their implications for> health care are usually more important than the speed with which the> data are made available.

I hope this has all been put in context and answered by now. Let me close by saying that "speed" is only one of the virtues of E-biomed, and by no means the foremost, which is the freeing of the literature for one and all, everywhere, forever, to the eternal benefit of research and researchers the world over, especially those whose access to the research corpus was blocked or limited by its current system of financial firewalls.

I hope these remarks will be accepted in the spirit of cautionary editorial quote/commentary that sometimes needs to accompany even the nonclinical papers that appear in refereed journals...

Stevan Harnad, University of Southampton, July 19, 1999

--------- Forwarded message ----------

From: "Phelps, Charles" To: 'Stevan Harnad' Subject: RE: Arnold Relman's NEJM Editorial about NIH/E-biomed

Steve, I am really enjoying this ongoing dialog you've created. Thanks for doing it and carrying on the discussion. I am returning this to you alone, since I don't know who all your email lists go to, but I'd be happy to have you send this out to whomever you think appropriate. (It is not intended as a letter to the *NEJM* however). Discussion follows:

In part it helps for those not familiar with Dr. Relman and the standing of the *NEJM* to understand their position. They have a market stature so great that it dominates all other medical journals, and probably all other journals in the world (possibly only excluding Science and Nature). Their citation index is about 20 per article; the next best (in a not too recent look) was *JAMA* at 12. Most other

journals are in the realm of 2 - 4 or lower in the field. Thus the *NEJM* has an extraordinary stature and power that they are obviously loathe to give up. The new medium threatens them more than any other publisher/journal.

Dr. Relman (and his predecessor, Franz Ingelfinger, MD) carved in stone what was once known as the Ingelfinger Rule, which is now commonplace: "We won't consider a manuscript for publication in the *NEJM* if it's been published elsewhere." They have a very strict definition of "elsewhere" to include all sorts of things that many people would not consider publication.

Their current stature and the tight control of pre-release of content are self reinforcing under current rules. They highlight "top" articles with a concurrent editorial ("commentary") and often a press release. This keeps *NEJM* articles in high visibility and they are (because of the very high and hence attractive stature of the journal and very stringent refereeing standards) of very high quality generally.

Obviously the NIH proposal threatens a part of this because the immediate newsworthiness of documents already available on an e - server diminishes. Yet a widespread and widely used NIH system would make it impossible for the *NEJM* to boycott manuscripts placed on the e-server (just as the physics journals could not boycott articles posted on Los Alamos). This is the major source of Dr. Relman's concern.

Charles E. Phelps, ProvostUniversity of RochesterRochester, NY

Stevan Harnad, University of Southampton, July 22, 1999

On Thu, 22 Jul 1999, ALPSP wrote:

> even if Stevan's estimate is correct that the costs of organising peer> review amount to no more than 1/3 of current journal costs > then the costs would be as follows (I have assumed that> authors of accepted papers pay twice as much as those whose papers are> rejected - the figures still stand, however, even if they all pay the same)> > Cost to existing subscriber = x> Total cost = 500x> > Cost for peer review only = 500x/3 = 167x> > Cost per rejected paper = 167x/180 = .93x> > Cost per accepted paper = 1.86x> > This suggests that the institution of an author whose paper was published> would pay more than at present, and that of a rejected author would pay only> slightly less.> > Have I missed something? Sally Morris

Yes, you've done the arithmetic wrong. Here's the way to do it:

The TOTAL institutional S/L/P income for all refereed journals is

$S

That is the current total cost. That is what will go down to $S/3 but let us do the arithmetic as if it will remain unchanged at 100%.

Any particular institution i will buy only a proportion of all those journals; let us say it spends

$si

The correct way to do the arithmetic is simply to note that institution i will continue to pay $si, but not for a subscrition to the journal, but to pay for the quality control and certification of its own author's papers refereed and accepted by that journal.

End of story. The only relevant details to work out (and they have been discussed before in this Forum, but no one has supplied hard data) concern what is to become of any possible discrepancy between "net provider" institutions and "net consumer" institutions.

Some institutions may provide more of the papers (in the 14K refereed journals in Ulrich's). Others read much more than they publish; the latter will get a free ride. If the imbalance is significant, some system will have to be found to rechannel their former S/L/P budgets too. Until we see the data we won't know, but my hunch is that the institutions with the biggest S/L/P budgets are also the biggest net providers, and vice versa, so the imbalance will be minimal.

Now I don't for a minute believe that 100% is the correct figure. I believe it is below 1/3. If so, the net provider/consumer issue is less significant, because 2/3 is a big buffer for any imbalance between net providers and net consumers.

The reason you are misconceptualizing this is because you are continuing to think of the articles as the product to the reader-institution, whereas the product in the era of online self-archiving will be the service of quality control to the author-institution, whereas the articles will be free for all, just as the authors would always have preferred them to be.

Your figures don't make sense because you are trying to calculate on a Moebius strip, holding onto the article-as-product notion. Drop it and you will see that even at 100% the figures have to balance; it's just a question of which end they are paid at.

About the fees: I happen personally to believe that a small submission fee, creditable toward the full publication charge, should the paper be accepted, would be a useful deterrent to frivolous multiple submission, wasting journals' and referees' time. But that is controversial and untested. So assume that all the residual costs (whether 100% or <30%) will be wrapped into the accepted article fee only.

Stevan Harnad, University of Southampton, July 28, 1999

The Editor of *Science*, Dr. Floyd Bloom has written an editorial about NIH's E-biomed initiative. http://www.nih.gov/welcome/director/ebiomed/ebiomed.htm

Floyd E. Bloom [Editorial] "Just a Minute, Please" *Science* 285 (5425) p. 197, 9 Jul 1999. http://www.sciencemag.org/cgi/content/summary/285/5425/197

This is a reply to his editorial.

To summarize, Dr. Bloom is writing ex officio as the Editor of *Science*, published by the American Association for the Advancement of Science. *Science* is a hybrid journal. It contains articles by salaried staff writers and commissioned articles written for a fee. It is important to note that these articles are in no way at issue here.

But *Science* also contains refereed research reports, submitted by their authors for free, with the sole objective of making the research findings available as broadly as possible once they have met *Science's* rigorous standards of peer review. It is these refereed articles only that are at issue here, and the issue is a simple one: Should NIH/E-biomed provide a free public Archive, modeled on the NSF-supported Los Alamos Eprint Archive in Physics (LANL), in which the authors of these refereed research reports can self-archive them online publicly, free for everyone, everywhere, forever?

http://xxx.lanl.gov/

Dr. Bloom is arguing that they should not be, and we will shortly examine his reasons. But we can be confident that Dr. Bloom will revise his views when more fully informed of the objectives of E-biomed and the scientific potential of free public archiving of refereed research on the World Wide Web, for Dr. Bloom represents the American Association for the Advancement (not the secondary sale or suppression!) of Science.

At the moment, Dr. Bloom's reservations are motivated by two factors: Concern

about the quality of the scientific research literature (and this concern is commendable, his journal being the representative of research standards of the highest quality) and concern about the revenue stream of his journal, which is the financial resource that is currently supporting those high standards of quality. It is here that I am afraid that Dr. Bloom is being somewhat short-sighted and perhaps even a little partisan too, unconsciously placing the interests of the maintenance of that revenue stream above the interests of the science that AAAS is dedicated to advancing.

It is undeniable that in the present PostGutenberg Era a conflict of interest has arisen between researchers and the current means of production of their published refereed research reports. There is a way to resolve this conflict, however, although it at first appears counterintuitive; and as the resolution is clearly to the benefit of science, and at the same time provides the revenue stream to sustain the essential service provided by the publishers of science -- quality control and certification in the form of peer review and editing -- there is every reason to believe that AAAS will find it fully compatible with its mission.

The resolution is a two-stage one.

First, it is necessary to identify and acknowledge the conflict of interest:

For scientific researchers, the reports of their (usually publicly funded) research findings are GIVE-AWAYS: They seek neither royalties nor fees; they seek only the eyes and minds of their fellow-researchers worldwide, present and future, so as to maximize the impact of their findings on the future course of research (and thereby also on the course of their careers and their livelihoods).

Researchers are accordingly interested in having their findings first quality-controlled and certified (QC/C) through peer review, and then made freely accessible to everyone. In the Gutenberg Era, the only way they could come anywhere near that goal was to treat their work exactly the same way trade authors (who wrote for fees or royalty) treated theirs, namely, to assign copyright to a publisher, who would then charge for access to the work in order to cover the substantial expenses of paper publication and distribution and to make a fair profit, where possible, for both himself and his author (in the form of royalties or fees).

But the scientists reporting their research findings in refereed journals were never interested in fees or royalties, for those would represent access barriers, restricting their findings to only those individuals and institutions that could afford to pay for them (via Subscription, Site License, or Pay Per View, S/L/P). Nevertheless, scientists had to live with these S/L/P barriers, for all the world as if they were trade authors seeking royalties or fees for their work, because in the Gutenberg Era

there simply was no alternative way to reach even that privileged subset of the potential readership of their article (not a large populace even in the best of times).

In the PostGutenberg era of global digital networks, however, there is at last an alternative, and not only researchers, but research itself, and hence all of society, would be the losers if we failed to take full advantage of it. For now we no longer have to rely on the expensive, inefficient and access-limiting technology of print on paper to disseminate refereed research findings. They can be SELF-ARCHIVED by their authors in public online archives like E-biomed (and its spectacularly successful model, LANL) and thereby accessible to one and all without any financial firewalls.

http://www.arl.org/scomm/subversive/toc.htmlhttp://www.cogsci.soton.ac.uk/~harnad/nature.html

Free public self-archiving, however, is only the first of the two stages of resolving the conflict of interest between research and its current means of publication. As long as there continues to be a demand for the paper version, it (and its proprietary online counterpart) can continue to be sold via S/L/P, which can continue to fund (among other things) QC/C (peer review). But meanwhile the worldwide research community will also have the self-archived online version on its desktops for free. And there is every reason to believe that they will grow increasingly reliant on it.

http://xxx.lanl.gov/cgi-bin/show_weekly_graph

Eventually, this is likely to shrink S/L/P revenues, and here it may look as if we are approaching a catastrophe point, for part of that revenue is paying for the maintenance of the quality standards of that literature (QC/C). But a very simple solution is available, once we recognize that the S/L/P revenues are largely being paid for by their researchers' institutions. Let us call this "reader-institution end" funding. All that is needed to continue covering QC/C costs is to switch from reader-institution end funding to author-institution end funding, covered fully by the S/L/P savings. The big difference is that reader-institution-end S/L/P is access-blocking, holding the literature hostage to access tolls, whereas author-institution end funding makes access completely free.

This is the second stage of the resolution of the conflict of interest, and it has the further advantage (although this is more controversial, because no one has the exact figures yet) that it will save institutions a great deal of money. For the cost of QC/C alone -- once publishers have scaled down to providing this essential service alone, leaving the access providing and preservation entirely to public online archives like LANL and E-biomed -- is likely to be much lower than current S/L/P expenditure. Indeed it is likely to be less than 1/3 of it, by current

estimates. See the American Scientist Discussion Forum threads on this:

http://amsci-forum.amsci.org/archives/september-forum.html

Odlyzko, A.M. (1998) The economics of electronic journals. In: Ekman R. and Quandt, R. (Eds) Technology and Scholarly Communication Univ. Calif. Press, 1998. http://www.research.att.com/~amo/doc/economics.journals.txt

This means that researchers benefit (access to their findings is expanded, potentially infinitely), their institutions benefit (both from S/L/P savings and from their researchers' enhanced impact), and research itself benefits (in both productivity and pace). Refereed journal publishers will unfortunately need to downsize, but in exchange they will have a stable, permanent niche that is compatible with the new medium rather than at odds with it.

Now I proceed to reply to Dr. Bloom's editorial on a quote/comment basis:

> Proponents [of the E-biomed Archive] acknowledge that cooperating> journals could lose subscription income and suggest that journals> recover their costs through submission and acceptance fees charged to> authors. E-biomed may be free to users, but it will not be free to> taxpayers or authors submitting through peer review.

We can now understand that this passage is based on a misunderstanding. Tax payers are already sustaining our educational and research institutions, including their S/L/P budgets, which will be REDUCED rather than increased by the switch to up-front payment in the online-only era.

And the costs of providing public research archive facilities such as LANL and E-biomed will be minuscule compared to the size of the literature and the benefits conferred; moreover, most of the infrastructure is in place already, in this increasingly networked world, and pooling resources with the rest of the disciplines (after Physics and the Biomedical Sciences) will make the marginal costs even more minimal.

So there is nothing whatsoever in this passage to deter us from resolving this conflict of interest in the way just described.

> [E-biomed has] much support from quarters long known to advocate a more> open scientific literature that would banish the alleged cabals of> editors, biased reviewers, and expensive commercial presses with> generally irrelevant content.

There are as always extremists around who want to banish QC/C, but leveler heads are bent on preserving it, and indeed the entire scenario just described is

predicated on just that.

http://helix.nature.com/webmatters/invisible/invisible.html

So this objection too is invalid.

> Lurking behind the public discussions are some potentially troubling> elements:

> What if the major journals choose not to cooperate out of concern that> their ability to survive and maintain quality control and timeliness> are threatened by the diversion of authors and competent reviewers into> the NIH system?

There was a little confusion in the initial draft of the E-biomed proposal. The eventual goal is cooperation with the refereed journals, in the form of official "overlays" on the archive, authenticated by them. But in the first stage, author self-archiving of their refereed drafts will suffice to free the literature.

http://www.nih.gov/welcome/director/ebiomed/com0509.htm#harn45

Nor is there any "diversion of authors and competent reviewers into the NIH system." There is no "NIH system," merely a public archive in which authors can deposit their papers (both refereed reprints and, if they wish, unrefereed preprints).

There is only one respect in which the major journals need to "cooperate," and one certainly hopes they will do so, otherwise this will escalate the conflict of interest instead of resolving it to the benefit of science: Publishers must not attempt to use copyright restrictions as a weapon to continue to hold the literature hostage to access tolls by forbidding public self-archiving.

This is THE central issue, and at the heart of all of this. *Science* itself has published a collective call for the retention of such author rights

http://www.sciencemag.org/cgi/content/full/281/5382/1459

along with a dissenting editorial by Dr. Bloom.

http://www.sciencemag.org/cgi/content/summary/281/5382/1451

Some prior comments on that exchange in *Science* are appended at the end of this reply. Let it only be noted here that progressive publishers are already resolving this conflict in a fair and rational way, in the interests of the scientific community they serve, rather than their own S/L/P revenue streams. A model in this regard (and they will be duly recognized by historians for this) is the American Physical Society (APS), publisher of the journals with the highest QC/C standards and

impact factors in Physics. Dr. Bloom's homonomyous APS counterpart, Dr. Blume, is one of the cosignators of the above copyright reform proposal in *Science*. For APS copyright policy, see:

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/

> Will societies whose members' future careers rely on NIH funding be> willing to resist the cooptation of their journals' editorial and peer> review systems?

Nothing is being co-opted. The NSF-funded LANL Physics Archive stands as a model for the kind of cooperative solution that will prevail. Journals, editorial boards and peer review will continue to exist, independent and intact. The only issue is whether they should be allowed to continue to try to hold this give-away literature hostage to S/L/P access tolls, against the interests of research and researchers.

> What will the real costs be to authors, peer-reviewed journals, and> scientific societies?

Yes, what will they be indeed, once the obsolete Gutenberg "add-ons" are phased out and only the essential QC/C costs remain?

> Does a monopolistic archive under government control by the major> research funder enhance scientific progress better than the existing> journal hierarchy, which provides multiple alternatives to authors and> readers?

Multiple journals -- indeed the entire hierarchy that currently exists -- will continue to exist for authors and readers. Nor will it be government controlled. (As always, quality will be controlled by peer reviewers, who, like the authors, do their work for free! QC/C costs are for IMPLEMENTING peer review, not for actually performing it.)

NIH will fund E-biomed, just as NSF funds LANL. The cost will be minuscule, and still smaller as more disciplines join in the self-archiving initiative. And once S/L/P expenditures shrink, savings will prevail, including savings on government-supported institutional serial budgets.

Pluralism will be, if anything, enhanced by a firewall-free global research literature. The objective is to free the literature from market restrictions that are no longer justified or necessary, not to take over a market!

(The word "monopoly," so clearly out of place here, will recur later in this reply in the context of certain collaborative firewall practises on the part S/L/P providers...)

> What about research in disciplines outside what the National Library of> Medicine considers biomedical?

There are plans for vetting the unrefereed clinical preprint sector to safeguard public health, but no planned restrictions of any sort on the refereed sector, any more than there are any such restrictions on the LANL Archive. (One wonders what Dr. Bloom has in mind here?)

> What about research not sponsored by NIH or even U.S. federal funds?

The answer to this question is so obvious, one can only wonder why it was raised: What about research not sponsored by NSF in LANL? What about LANL's 14 mirror sites around the world? Why on earth would an archive dedicated to freeing access to the refereed research literature for the world scientific community through self-archiving have any interest in blocking access to any of it? (The only interests vested in blocking access to this corpus at the moment are certainly not governmental ones...)

> Without answers to these and other questions, it is hard to determine> the feasibility of the proposal.

(The answers are in each case so trivially obvious that one can only wonder what the real source of the reservations about feasibility might be!)

> Science and other journals are eager to identify the advantages of the> E-biomed proposal and are actively looking for changes that could> benefit scientific publishing.

The advantage of the E-biomed proposal is that it will free the refereed journal literature, to the benefit of science, scientists, and humankind. The only change required at the moment is a copyright policy that clearly recognizes the no-royalty/no-fee author's right to self-archive along the lines of the APS policy.

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0006.html

> For example, the E-biomed server would provide a venue for online> publication of negative results and thus allow others to avoid> experimental repetition.

Among the much more profound benefits of public online self-archiving of refereed reprints and unrefereed preprints there is also the more modest one of being able to self-archive negative results, both those that have been accepted by refereed journals, and those that were not.

> On the other hand, if NIH really wants to improve access to the> literature, they could digitize the peer-reviewed literature published> before 1995.

The retroactive peer-reviewed literature is most certainly welcome in the free public archives, and will most certainly be deposited there, both by individual authors and by digitization initiatives (neither LANL nor E-biomed is a digitization initiative: they are public self-archiving initiatives).

But exactly what is the benefit to science of restricting availability to the pre-1995 literature?

> In addition, all would benefit if NIH developed software for online> journal submittals and provided access to a common search engine that> could survey all peer-reviewed sciences across all journal lines.

The first benefit, though undeniable, is likewise not E-biomed's mandate. (Why should NIH develop submission software tools?) On the other hand, the practise of self-archiving will certainly help accelerate the development of such tools, and it will hasten and expand authors' using them. Moreover, once the second stage is reached, official journal overlays on E-biomed will allow automatic online submission to the journals via the archive, as is already being implemented on LANL in collaboration with the APS.

As to providing the capacity to "survey all peer-reviewed sciences across all journal lines," this will be trivially provided by E-biomed and any number of generic search engines as soon as the self-archiving initiative is well under way, and E-biomed is well stocked with papers searchably tagged as "U" (unrefereed preprint) or "R" (refereed reprint, together with journal name "Jx").

But the principal advantage of this free public archive will be that it will indeed be "across all journal lines" without any of the financial firewalls that criss-cross the proprietary online corpus as it now stands -- a state of affairs that some would like to see turned into a "click-through" monopoly governed by interpublisher S/P/L fee agreements!

http://www.cogsci.soton.ac.uk/~harnad/citation.html

> It may be instructive to recall an earlier congressional reaction, as> Albert Henderson, editor of Publishing Research Quarterly did in his> response to E-biomed on 6 May. In the Sputnik aftermath, an> E-biomed-like proposal was made that Congress accelerate U.S.> scientific research by establishing a unified information system> similar to what had been created in the Soviet Union. The Senate's> advisory panel responded: "The case for a Government-operated, highly> centralized type of center can be no better defended for scientific>

information services than it could be for automobile agencies,> delicatessens, or barber shops." Surely other creative solutions can be> found to what NIH considers problems. Are they prepared to listen, or> is this a done deal?

Both Dr. Henderson and Dr. Bloom might benefit from being reminded (if they will only listen!) that unlike the producers of cars, bagels and haircuts, the producers of refereed journal articles wish to give them away for free. And there is no earthly reason why any government should not wish to help them do so, to the eternal benefit of science and society worldwide.

This would have been as welcome in the Sputnik era as it is today, but we had not yet reached the PostGutenberg Galaxy at that time.

The only costs that remain to be paid are those for the SERVICE of implementing QC/C, costs that it will make incomparably more sense for the author-institution to pay up-front, out of S/L/P savings, thereby freeing the literature for one and all, along with a considerable institutional saving, rather than at the access-denying reader-institution end, for the reasons described above.

This is the end of my reply. I close with some unanswered prior comments on Dr. Bloom's earlier editorial on copyright. See:

http://www.cogsci.soton.ac.uk/~harnad/science.html

Bachrach S. et al. (1998) Intellectual Property: Who Should Own Scientific Papers? *Science* 281 (5382): 1459-1460. September 4 1998.
http://www.sciencemag.org/cgi/content/full/281/5382/1459

Bloom, F. (1998) EDITORIAL: The Rightness of Copyright. *Science* 281 (5382): 1451. September 4 1998.
http://www.sciencemag.org/cgi/content/summary/281/5382/1451

Some excerpts:

Intellectual Property: Who Should Own Scientific Papers?

Bachrach, S., Berry, S.R., Blume, M., von Foerster, T., Fowler, A., Ginsparg, P., Heller, S., Kestner, N., Odlyzko, A., Okerson, A., Wigington, R., & Moffat, A.

"...The goals and motivations of scientists writing up their research are very different from those of professional authors, although they may be the same people in different settings. The scientist is concerned with sharing new findings, advancing research inquiry, and influencing the thinking of others. The benefits the scientist receives from publication are indirect; rarely is there direct

remuneration for scientific articles. Indeed, scientists frequently pay page charges to publish their articles in journals. The world of the directly paid author is very different. There, the need for close protection of intellectual property follows directly from the need to protect income, making natural allies of the publisher and the professional author, whether a novelist or the author of a chemistry text..."

"...The suggested policy is this: Federal agencies that fund research should recommend (or even require) as a condition of funding that the copyrights of articles or other works describing research that has been supported by those agencies remain with the author. The author, in turn, can give prospective publishers a wide-ranging nonexclusive license to use the work in a value-added publication, either in traditional or electronic form. The author thus retains the right to distribute informally, such as through a Web server for direct interaction with peers..."

"...[Some publishers, such as] *Science*, the *New England Journal of Medicine*, and the *Journal of the American Chemical Society*, have adamantly opposed authors' posting of their own articles on Web pages or e-print servers, whereas others, such as the American Journal of Mathematics, the *Journal of Neuroscience*, *Nature Medicine*, and *Physical Review*, have considered such distribution consistent with, and even advertising for, their own journals..."

EDITORIAL: The Rightness of Copyright:

Floyd E. Bloom

"...[C]opyright transfer is critical to the process of communicating scientific information accurately. Neither the public nor the scientific community benefits from the potentially no-holds-barred electronic dissemination capability provided by today's Internet tools. Much information on the Internet may be free, but quality information worthy of appreciation requires more effort than most scientists could muster, even if able...."

Questions for Reflection [SH]:

(1) Is F. Bloom's a logical or even a practical argument for full copyright transfer to publishers by refereed-journal paper authors, ceding their right to archive those papers for free public access?

(2) Is it really true that the only options are either (a) free papers, with no quality control, or (b) quality-controlled papers, but only in exchange for copyright transfer and the ensuing blockage of free access by S/SL/PPV (Subscription/Site-

License/Pay-Per-View) fee barriers?

"...A paper submitted to *Science* will undergo extensive review and, upon acceptance, extensive revision for clarity, accuracy, and solidity. A paper published in *Science* will be seen throughout the world by our 160,000 paid subscribers and perhaps two or three times more readers as issues are shared. More than 30,000 readers will be alerted to the new reports within hours of the appearance each week of *Science Online*...."

(3) How many other journals reach 160K subscribers (or even 1/100 % of that)?

(4) Free posting on the Web can reach all 160K (and 100 times that).

(5) *Science* magazine is a hybrid trade/refereed journal. It publishes refereed articles, contributed for free, plus commissioned and paid articles by staff writers and others, for fee. Hence it is in most relevant respects not representative of the vast refereed literature of which it (and a few other journals like it, such as *Nature*) constitutes a minuscule portion.

"...This degree of investment in the scientific publication process requires the assignment of copyright. This allows the society publisher to provide a stewardship over the paper, to protect it from misuse by those who would otherwise be free to plagiarize or alter it, and to expand the distribution of information products for the benefit of the society.

(6) Do we need this degree of investment? Is it worth the consequences (S/SL/PPV, fire-walls)?

(7) What is "stewardship"?

(8) What do copyright ASSIGNMENT (to the publisher) and S/SL/PPV tolls have to do with protection from plagiarism or alteration? (Doesn't copyright simpliciter already provide that, without transfer to the publisher?)

"...Permissions are granted freely to the originating authors for their own uses. *Science* holds the copyright of its authors because of our belief that we materially improve and protect the product we create together...."

(9) What if the "own use" is the provision of one's work to others, through free public archiving on the Web?

(10) Would payment for the true cost of the necessary "improvements" not be sufficient, without the need for copyright assignment, S/SL/PPV and firewalls?

[Again, this should all be considered in conjunction with the fact that *Science* magazine is far from representative of refereed journals, for the reasons noted above.]

Stevan Harnad, University of Southampton, July 30, 1999

For those who are following the ongoing online self-archiving debates, there are public archives of the discussion:

E-biomed: http://www.nih.gov/welcome/director/ebiomed/comment.htm

Scholar's Forum: http://library.caltech.edu/publications/ScholarsForum/

American Scientist September-Forum [1998 & 1999: largest Archive] http://amsci-forum.amsci.org/archives/september-forum.html

Forthcoming meeting on the universal self-archiving initiative: http://vole.lanl.gov/ups/ups.htm

There are also many relevant links on the home page below.

Stevan Harnad, University of Southampton, July 31, 1999

On Fri, 30 Jul 1999, ransdell, joseph m. wrote:

> The responses to the E-biomed proposal are preponderantly affirmative> and strongly enough so that if Varmus has been testing the waters for> support he would seem to have no reason to hesitate in implementing a> revised version whenever he thinks enough time has passed to do so.> Hopefully, it will be modified in light of the flaws Stevan pointed out> in his critique of it, chiefly by a correction of its mistaken aim of> undertaking journal reform, as distinct from providing facilities> supporting journals in going on-line properly.

This is incorrect. The correction was not just to drop journal (peer review) reform and "instead provide facilities for journals to go on-line properly"! The former is correct, but the latter is almost as garbled as the (remediably) garbled portions of the first E-biomed draft.

http://www.nih.gov/welcome/director/ebiomed/com0509.htm#harn45

My recommended correction was to drop peer review reform AND to make it

explicit that a SELF-ARCHIVE was precisely what E-biomed was to be (in the first instance), exactly as LANL is a self-archive. AND, most important of all (and systematically not taken into account in any of Joseph's comments), a self-archive not only for the unrefereed preprint literature but for the REFEREED reprint literature (exactly as LANL is, and has been from its very inception, as Paul Ginsparg's recent posting has reminded us).

The effect of the latter is that the self-archive frees the refereed journal literature (i.e., makes it accessible to everyone online without they or their institutions having to pay Subscription/Site-License/Pay-Per-View [S/L/P/] access tolls to get it).

This effect is certainly not correctly described as "providing facilities supporting journals in going on-line properly"! It does, however, prepare the way for a later, collaborative stage of journal overlays, where the journals can officially authenticate the refereed drafts as such. I explicitly stressed, however, that this collaboration cannot be presupposed or even expected before the "subversive" effects of self-archiving have prepared the way (by bypassing S/L/P for this special literature, which is, and always has been, intended by its authors as a give-away literature, and not a fee- or royalty-bearing one).

http://www.arl.org/scomm/subversive/toc.html

Why does Joseph mis-state this? I think it is because of our disagreement about the catastrophic drop in quality standards that I believe would result if peer review were abandoned in favour of a self-archived "vanity press." This is not, and never has been, what I think self-archiving is all about. The simple proof is the self-archived REFEREED literature itself, which is simply a free give-away of the current S/L/P-based journal literature by its authors. This is NOT a vanity press! It is simply the journal literature, online, without a price tag.

Joseph's imagination is taken up with the OTHER side of self-archiving, the unrefereed preprints. These are a wonderful, indeed revolutionary supplement to the classical peer-reviewed canon -- and, as I have argued elsewhere, even THEY are not quite a vanity-press either, because of the "invisible hand" of peer review: it is in the expectation of being answerable to the peer review that virtually all of these unrefereed preprints have been drafted, and indeed most of them are formally submitted to journals simultaneously with being self-archived as preprints (and the refereed, accepted final drafts are swapped or added as soon as they are available in most cases).

http://www.cogsci.soton.ac.uk/~harnad/nature2.html

Nevertheless, a self-archive's unrefereed sector alone can be correctly described as

a kind of "interim vanity press" -- but with the knowledge and expectation that it is only an embryonic stage along a continuum which will eventually be clearly marked by the quality-controlled/certified milestone of the accepted, refereed draft. (The current average latency in LANL is about 11 months, between the preprint and the reprint, as Les Carr will soon be reporting in a paper.) Nor does the continuous, interactive, and self-corrective process of learned inquiry come to an end with the certified refereed version, for there is still the possibility of self-archiving updated/corrected revised drafts, as well as critical commentaries and responses, all linked to that certified version.

http://cogsci.soton.ac.uk/~harnad/Papers/Harnad/harnad90.skywriting.html

Not to mention subsequent self-archived refereed articles (by the author and others), citation-linked to the original one. All this is the world opened up by self-archiving (and not just the "vanity press" that Joseph mis-describes me as calling it!)

http://www.cogsci.soton.ac.uk/~harnad/citation.html

> But supposing that> happens, will the journals actually take advantage of such facilities?> Perhaps some will, but I doubt that this will happen to anything like> the extent wanted if nothing more is done than to provide archival> support for that as well as for self-archiving by authors.

As stated in the original critique, there will initially not be much incentive for journals (especially those published by commercial publishers) to collaborate with E-biomed (other than the need to plan ahead and face reality in the form of what is clearly the optimal and inevitable solution for science and scientists).

http://www.cogsci.soton.ac.uk/~harnad/Papers/Harnad/harnad97.learned.serials.html

But if/when the subversive effects of freeing the literature through self-archiving begin to make themselves felt as declining S/L/P demand, then publishers will certainly want to adapt in such a way as to retain a niche (which I have predicted will be through downsizing to provide only the service of quality-control/certification, QC/C, funded not through access-blocking reader-institution end S/L/P charges but through up-front author-institution end publication charges -- leaving the archiving to E-biomed, and merely providing an official overlay to authenticate the archive's refereed sector).

So Joseph has missed the point here.

> More generally, in spite of increasing evidence of popular support> across an

impressive range of interested parties for what I will call> "The Harnad Initiative" to free the professional literature, there seems> to me to be little reason to think that the attempts to implement it by> providing the archives for it will have the success hoped for.

Does the following evidence of the spectacular success of LANL not count as reason to expect success (if the desirability of a free online journal literature for science and scientists is not reason enough)?

http://xxx.lanl.gov/cgi-bin/show_monthly_submissionshttp://xxx.lanl.gov/cgi-bin/show_weekly_graph

> The> archives should be built, in any case, as they will have some use and> significant change will eventually come about, no doubt, but I wouldn't> bet on much immediate use of it that betokens a change in publication> practices. Why? Because the migration on-line is conceived thus far as> depending on self-archiving, and there is no reason to think that people> are presently motivated to do that, nor has anything been suggested or> planned that might provide some incentive.

If researchers are motivated to have their research read by all who want to read it, if they are motivated by the desire to make an impact with their research (on both subsequent research and on citations), and if their long-standing willingness to self-supply paper offprints to all who ask is still in vigour, then all disciplines will take to self-archiving, just as Physicists have done.

Of course, there's no second-guessing how quickly the rest of the scholarly/scientific thoroughbreds will stoop to drink from the waters of self-archiving: but that's no reason not to lead them there.

> The idea that if an archive is provided then it will be used has no> evidence in its favor, as far as I know, and if there really was some> general propensity for people to self-archive whenever the opportunity> presented itself that would surely have shown itself by now.

Vide supra. I hope you are wrong about the scholarly/scientific community's agnosia about what is optimal for it, but of course there is a logical possibility that, historically speaking, Physicists will be the only ones who ever twig on this! (I rather doubt this, though; they may be smarter than the rest of us, but not THAT much smarter...)

> There are> too many special considerations in connection with the LANL archive to> make the continuing increase in use there evidential for some general> trend toward going on-line across the board in academia, and I don't see> any real

indications of this happening elsewhere, with possibly some> spotty exceptions here and there.

This sounds like a classical hedge; let us solemnly hope that those subversive spots keep growing!

http://www.acm.org/repository/http://cogprints.soton.ac.uk/

> Of course, it doesn't help the cause of self-archiving for the chief> proponent of the practice to label it as resorting to the "vanity> press", but although Stevan keeps shooting himself in the foot with> that, I don't think that is at the root of the problem.

Not only is it not at the root of the problem, it isn't even true! It is Joseph who is here calling self-archiving "vanity press," whereas I call the self-archiving of refereed papers "freeing the refereed literature" -- which is the antithesis and antipode of vanity press! (Only the self-archiving of unrefereed papers is vanity press, and only if it stops there, rather than going on, as most papers do, to pass through peer review into the journal canon.)

> What is at the> root of it is, I think, a failure to understand the role of EDITORS in> the publication process, which has been obscured by the mistaken> conflation of the editorial function with the function of peer review.

It would be an odd circumstance indeed if I, who have been editing a major refereed journal for over two decades now, suddenly became agnosic to that fact (or confused refereeing with editing).

http://www.princeton.edu/~harnad/bbs/index.htmlhttp://www.princeton.edu/~harnad/psyc.html

> Editors tend to be self-effacing, and there seems to be a common> (mis)understanding that because editors only have a "service" function> as mediators they are not important. But it would be much closer to the> truth to say that editors are the true rulers of academic life because> they are found everywhere, at all of the gates of communication, opening> or closing them according to judgments which hardly anyone ever thinks> to question. Not important? Hey, think again! But why, in all of the> discussion of "decoupling" of functions are editors not discussed?

Because this discussion is not about peer review reform but about freeing the peer reviewed literature, such as it is!

Peer review and the role of editors is eminently ripe for empirical investigation,

but that is a different topic:

"Neither the editor nor the referees is infallible. Editors can err in the choice of specialists (indeed, it is well-known among editors that a deliberate bad choice of referees can always ensure that a paper is either accepted or rejected, as preferred); or editors can misinterpret or misapply referees' advice. The referees themselves can fail to be sufficiently expert, informed, conscientious or fair." [Harnad 1998h: see references at bottom of this comment]
Joseph's commentary goes on to a display of animus against both peer review and university administrators:

> ... pseudo-glorification of the peer reviewer... Validating and> certifying and putting stamps of approval on documents is the sort of> thing they used to do at the Vatican -- or maybe they still do, since> the Pope is still officially infallible -- and in Moscow, too, up to a> decade or so ago. But in the secular sciences of the free world?...

> ... the administrative view that the research universities are knowledge> factories, producing and selling knowledge, with the faculty regarded as> workers on the production line.

I won't comment on any of this. I do think I recognize (from 20 years' of editing) the core of Joseph's grievance. It is the single aggrieved author's viewpoint (analogous to the single aggrieved student's viewpoint, when he feels that a test has not been a proper measure of his proficiency or performance).

Such grievances are not to be taken lightly, because the system (both peer review and tests/exams/marking) are indeed fallible and imperfect. But the real question is one of scale. Every student hopes to have his every thought and action weighed in a blind, omniscient and infinitely fair absolute balance. In reality, all population-based measures are approximate and have a margin of error and even bias. The objective is to minimize that error and bias, within available resources, and to remedy detected cases of error where possible.

The rest is down to testing and designing systems that work at least as well as the current ones (for a population at least as large). It is certainly no solution to focus on known or perceived cases of misevaluation, and to simply propose scrapping the evaluative system on their basis!

> A peer reviewer in any field is a presumptive equal of the person> whose work is being reviewed, and that means that there is no> presumptive superiority in status that makes the peer reviewer's view> right and the reviewee's view wrong

when they disagree. If the> disagreement is a simple contradiction one is perhaps right and the> other wrong; but there is nothing in the conception of a peer or of a> reviewer that can justify regarding the peer reviewer as being in a> favored position when disagreement occurs or which would turn an> agreement in opinion of the two into a validation of the one by the> other. The conclusion of a peer review is just a second opinion, that's> all.

A competent editor knows all this, and is dealing with it in every case. It is only the aggrieved author who sometimes feels (and sometimes with justification) that he has been ill-used, and that an incompetent referee's judgment has blocked his submission.

Competent, conscientious editors (who cannot be specialists in all areas) are responsive to authors' rebuttals of referee reports. They may betoken a faulty choice of referees, or shortcomings in the referee reports. But they may also betoken defensiveness on the part of the author, or unwillingness to do the work required to make a paper ship-shape. I know of no way to automate or replace editorial judgment here, but we should certainly keep testing new ways of strengthening it. What is certain is that no concrete or practical alternative (let alone one that has been tested and shown to do at least as well as the present system) has been proposed by Joseph Ransdell here!

> why do we> keep talking as if formal peer review is the key to legitimization in> the sciences and elsewhere? Willingness of peers to criticize and> openness and responsiveness to peer criticism is what provides the> critical self-control of the process of inquiry through corrective> negative feedback -- it is this process itself, not individual persons> and judgments, that regulates inquiry overall and legitimates it as> science or scholarship -- but there are many different ways in which> corrective feedback loops can and do occur in the course of inquiry.> Formal peer review, set up for certain special purposes, including> journal publication, is one of them but should not be fixed upon so> exclusively as to blind us to the other ways critical self-control> functions in the professional communication of scientists, and should> not be allowed to mislead us into thinking that the sciences depend for> their validity on anybody wielding stamps of approval.

It is hard to extract the substantive point in all this: Peer review is certainly not the only self-corrective mechanism of Learned Inquiry. Informal peer feedback before publication, formal and informal peer feedback after publication, and the further march of Learned Inquiry itself, attempting to build upon published ideas and findings (especially in empirical science), are all parts of this collective, cumulative, self-corrective, and, one hopes (where appropriate), convergent system too.

But at the present scale of publication, if the classical peer review at its core were removed (at the point of formal -- i.e. refereed-journal -- "publication") and only the rest were left in place, I believe it would all soon devolve into anarchy, human nature being what it is (at the population level), when it is not directly answerable to quality standards -- until peer review was simply rediscovered or re-invented as the simple solution for the triage of all that growing body of unregulated and unnavigable human noise!

I could be wrong, but surely this all has to be tested first, locally, and in a controlled way! So, for the moment, as mentioned before, classical peer review should proceed apace, until further empirical notice, and we should focus instead on freeing the peer-reviewed literature, such as it is.

> If you re-read the Phelps document> which is ancestral to the Caltech proposal, for example, you will find> that Phelps' understanding of what is wanted in promoting on-line> publication practices supposes that the "certification" function of> publication (which he also regards as the "credentialing" of the author)> can be cleanly decoupled from the distribution function, so that one> need merely set up a pool of peer reviewers -- the first generation of> which are Unreviewed Reviewers whose superior quality is guaranteed by> The Self-chosen Chooser, apparently -- who can be called upon to perform> the operation of quality control on documents submitted to them,> assessing them as fit or not fit to print.

I don't wish to defend any untested armchair schemes for reforming peer review, whether Ransdell's or Phelps's (indeed I made the same recommendation for the Scholar's Forum archiving initiative as for the E-biomed archiving initiative: Dissociate them completely from peer review reform schemes). Joseph is, I think, just lapsing here into the conspiratorial view about both administrators and peer reviewers that he has repeatedly expressed in September-Forum. Again, he may have some legitimate grievances, but he has no realistic or relevant alternative to offer, let alone a tested one.

http://library.caltech.edu/publications/ScholarsForum/042399sharnad.htm

http://amsci-forum.amsci.org/archives/september-forum.html

> The editorial function was forgotten> just as it has been forgotten in the ongoing discussion here by being> reduced, in effect, to a stamp of authority wielded by the peer> reviewers, who have been assigned the bogus task of being the official> validators of the entire scientific process.

Nonsense. The function of a competent, conscientious, answerable editor is part of the very meaning of the classical peer review system! It never was a disembodied stable of "peers" to which papers were dispatched willy-nilly for their box-score

votes.

> I suggest, then, that the reason nothing is happening in academia as> regards the migration on-line...

The premise is false. A good deal is happening because of the LANL initiative, and, one hopes, a good deal more will be happening thanks to the E-biomed and Scholar's Forum initiatives.

> is that editors see no future for> themselves in it and therefore are not about to change the> communicational arrangements researchers live by; and nothing will be> happening until the editors do see something worth running the risk they> run in going on-line, which may very well result in a diminishment of> their importance if attention is not paid to it.

I am afraid that this too makes no sense. It is up to AUTHORS, not editors, to self-archive; not even the journal's copyright policy on self-archiving is in the editor's jurisdiction -- although they can take a position on it, and have: cf. Editors Blume and Bloom of APS and AAAS, respectively:

http://www.cogsci.soton.ac.uk/~harnad/science.html

To repeat, journals are almost all "going online" already. That is trivial and a foregone conclusion. What is at issue here is whether the only online version should be the one held hostage behind the access-denying financial firewalls of S/L/P, or there should be an alternative give-away, author self-archive:

http://www.cogsci.soton.ac.uk/~harnad/nature.html

> That their fear of> being diminished or even eliminated is a reasonable one is evident from> the following remark of Stevan's in his response of June 27th to the> immunologists:> > sh> Now there is no doubt whatsoever that this service> sh> will force the established journals to restructure themselves> sh> in certain ways. (My own prediction would be that it will> sh> make journals scale down to providing only the service of> sh> peer review and authentication, ...> > The editor has just disappeared, it seems.

Nothing of the sort. The Editor's role is PRECISELY the same as it always was in classical peer review, which is not changed by a single epsilon -- at least according to my own recommendations...

This ends my comment. The references promised above follow.

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Stevan Harnad, University of Southampton, August 9, 1999

On Fri, 6 Aug 1999, Arthur Smith (American Physical Society) wrote:

> If self-archiving succeeds as Harnad promotes, it is clear that> journals will have to change, and will be expected to.> They, and the editors who represent them, have every right to know > what to expect, and to have a say in how their journals respond.

Journals and their editors certainly have every right to know what to expect

(insofar as anyone can know for sure), and it is to be hoped that heads will be put together to make rational plans for how to respond if/when the effects of freeing the literature through self-archiving should have their likely effect on S/L/P demand. This Forum (among others) has strongly recommended concerted advance planning for this eventuality.

But meanwhile the self-archiving initiative should certainly not wait.

There is no reason whatsoever why AUTHORS should sit back and wait to self-archive until journals and their editors first see fit to plan a transition scenario in case self-archiving should one day cause S/L/P revenues to decline -- for in that case the wait could prove to be a long one indeed! LANL authors did not wait; it is time now for other authors in all the other disciplines to follow suit. The creation of further self-archiving facilities modeled on LANL, such as CogPrints, E-biomed, and Scholar's Forum should facilitate this.

There is at present only one contingency between author self-archiving and journal plans and policies, however, and that concerns copyright, in particular, the author's right to self-archive. Now that LANL has shown the way, not only is there no longer any justification at all for continuing to hold authors' refereed papers hostage to S/L/P access tolls, but there is no justification for holding them hostage to journals' failure to make contingency plans either.

The handwriting is on the wall (or in the sky, as it were): Self-archiving, is within reach of all, and it works, to the benefit of all, as LANL has resoundingly demonstrated. If authors see this and fail to take advantage of it -- if they are led to the water yet fail to drink -- that will be their own fault, and nolo contendere.

But if they are deterred from doing it by journal policies that attempt to forbid it, then I am afraid there may be unstable times ahead; for such restrictive copyright policies are no longer either ethically justifiable, technically enforceable, nor even logically coherent (for there is a slippery slope from the author's raw, unrefereed first draft, circulated [to how many people?] informally in paper before submission to the journal, to the public archiving of that draft, to the public archiving of successive revised drafts, all the way up to the final, accepted, refereed draft: where is there an ethically justifiable, technically enforceable, logically coherent boundary line along this give-away continuum?).

So if "how their journals respond" refers to how they respond to S/L/P decline as a result of self-archiving, by all means journals should be as informed and proactive as possible; but if it refers to how they respond to the THREAT posed by self-archiving -- i.e., what they can do to prevent it -- then I am afraid this would only escalate the conflict of interest rather than resolve it.

> Many have responded by not accepting papers that have> previously appeared on preprint servers, and/or by holding authors to> egregious copyright agreements that preclude subsequent self-archiving.

Yes, and fortunately Arthur Smith's Editor in Chief and many others have come out strongly against this self-serving policy, which is so contrary to the interests of research and researchers.

http://www.cogsci.soton.ac.uk/~harnad/science.html

> How do you get self-archiving started if nobody in a field does it,> and the journals are already online and accessible from most institutions> with researchers who might care?

Partly by creating reliable public self-archiving facilities modeled on LANL (such as CogPrints, E-Biomed, Scholar's Forum)

http://cogprints.soton.ac.uk/http://www.nih.gov/welcome/director/ebiomed/ebiomed.htmhttp://library.caltech.edu/publications/ScholarsForum/

and partly by tirelessly preaching to the auctorial thoroughbreds the benefits of partaking of these waters.

http://www.cogsci.soton.ac.uk/~harnad/nature.htmlhttp://www.cogsci.soton.ac.uk/~harnad/Papers/Harnad/harnad97.learned.serials.html

> I have some experience dealing with major scientific societies in > the U.S. and I can tell you that very few of them are comfortable with > the projected revenue loss they see coming from your predictions.

No doubt. But are they more comfortable with trying to hold the journal literature hostage from the optimal and inevitable?

> We who do not agree are definitely in the minority. In the U.S.> it is the physical society, the astronomical society, and with perhaps> some equivocation the mathematics society on one side, and everybody> else pretty much on the other.

Patience. Physicists and mathematicians may be smarter, but eventually the rest of us will catch up too.

> The British Medical Journal, which is partly sponsoring its own> author self-archive, is the only bio-medical publisher I have seen> that strongly advocates it.

There is no reason for publishers to ADVOCATE self-archiving; they need only refrain from trying to PREVENT it.

http://www.nih.gov/welcome/director/ebiomed/comment.htm

> And I have a hard time believing that Stevan's current arguments will> win more than a handful of converts from the opposition, given their> current entrenched positions.

That may be true, but I am not primarily preaching to publishers here, but to authors: They are the "self" in the self-archiving initiative.

> LANL has grown at a pretty much linear> rate, handling probably 25,000 new submissions this year, 20,000 last> year, 15,000 the year before, etc. Projecting this linear growth> forward it will take another 10 or so years to capture all articles> published in pure physics (currently something like 1/3 of papers we> receive also appear on the archive), roughly 50 years to capture> both pure and applied physics, and at least 200 years to capture most> of scientific publishing. A lot can happen in 10 years, let alone 200.

All true. Let us hope that once E-biomed is on-line the pace will quicken. A lot can happen in 10 months too, when it comes to self-archiving.

> Anyway, the point is there is absolutely no guarantee that self-archiving> will prevail among authors, and there are good reasons to think existing> journal publishers and their editors will work against it.

There certainly is no guarantee that self-archiving will prevail, but there are strong reasons to believe it would be optimal for research and researchers. Let us hope that if publishers work against it, it will only be by trying to offer something even better, rather than by trying to forbid it.

As to editors: the editors are US (just as the authors, referees and readers are us); let us only hope they/we do not forget it:

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0019.htmlhttp://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0021.html

> recent discussion has revolved around differences between> physicists and mathematicians and even between subfields of> mathematics - not that any of the arguments in opposition hold much> water, but they are there, and they are acting to prevent author> self-archiving from taking hold in that community.

Let us hope that arguments that don't hold much water don't keep us from the water for much longer...

> Journals and their editors need to support this, or it will> not happen. The time is past for being subversive.

I don't see this at all! On the contrary, the time is very much now.

Subverting a system means taking matters in one's own hands in order to bring about an alternative. I cannot legally archive YOUR articles, nor you mine, in order to bring us both to the optimal and the inevitable; but each of us self-archiving our own small portion of this give-away literature can -- and could do so almost overnight.

http://www.arl.org/scomm/subversive/toc.html

Stevan Harnad, University of Southampton, August 10, 1999

On Mon, 9 Aug 1999, Michael Jacobson wrote:

> Dear Dr. Harnad,> > I have been following the discussion on E-biomed with much interest. I> contributed a comment to the site, which was posted in June.> > You have replied in detail to many comments that address your views, but > have not responded to mine. I would be most interested to have your > thoughts on my posting, which is at:> > http://www.nih.gov/welcome/director/ebiomed/com0613.htm#jacob158> > Michael Jacobson, MD, MPH, FACP> http://www.journalclub.org/

My apologies to Dr. Jacobson for failing to respond to his apt and thoughtful commentary. There have been so many comments and responses that I missed that one in the first round. I hope the following response will make amends.

Before moving to quote/comment mode, let me only say that I largely agree with Dr. Jacobson's analysis. But there are a few seemingly minor points on which, once the binary flag is re-set, the overall picture looks quite different.

Let me hasten to add, though, that the setting of these binary flags is a matter of probability, not certainty, and it involves an element of trying to second-guess human nature, which is always risky.

I accordingly stand by my own scenario insofar as what is optimal and inevitable is concerned. (I don't think Dr. Jacobson contests the optimality, but perhaps he

thinks the inevitability is a longer way off than others of us hope!) Also not contestable, I think, is what is within authors' reach now, practically speaking; whether or not they actually reach for it is of course another question. For my own part, I shall continue to sing the virtues of self-archiving (and to help provide the facilities for it). The rest is all about what, once horses have been led to water, will lead them to drink...

> Dr. Harnad's basic premise is that although biomedical journals are> well-suited to perform peer-review, they no longer have the legitimacy> to usurp article authors' right to distribute their work.

I would not have put it quite that way (words like "legitimacy" and "usurp" are fighting words), but Dr. Jacobson has the facts right: I would have said it this:

"Biomedical journals continue to perform an essential service to biomedical science in implementing peer review (quality-control and certification, QC/C), but they are no longer the optimal distributors of the refereed research reports; and if they were to attempt to prevent optimal distribution via public self-archiving, then they would in fact be acting contrary to the interests of biomedical science and scientists."
I would add only that this is a new and unprecedented state of affairs, arising from the revolutionary possibilities opened up by global digital networking, and that it must be looked at afresh, rather than by simply trying to force it into the Procrustean paradigm of a bygone Papyrocentric era.

> The reason authors ceded copyright protection for their work to journal> publishers in the first place was because they had no other way to> distribute their research results. According to Harnad, scientific> authors were forced to strike a Faustian bargain.

This is correct, but it omits two essential points. One is that this literature (the refereed journal literature) is and always has been a GIVE-AWAY literature from the author's point of view; this makes it profoundly unlike any other literature.

The second point is that there is still an essential service that the publisher provides (apart from the now redundant distribution function) and that is QC/C. So no matter how much the author may wish to give away his refereed research reports via self-archiving, a way must be found to continue to fund the QC/C, otherwise there will be no REFEREED research reports to give away!

So the the Faustian (copyright) Bargain certainly has to be resolved in science's favour -- scientists MUST be allowed to give away their research reports -- but QC/C costs must be covered too. (Fortunately, that is easily done, out of institutional Subscription/Site-License/Pay-Per-View [S/L/P] savings.)

http://www.cogsci.soton.ac.uk/~harnad/nature.html

> Now, with the widespread availability of the Internet and the ease with> which one can distribute intellectual content online, authors can> distribute their work worldwide, without needing to use the mechanism> of paper-based journals, and should thus no longer be forced to give up> their property rights.

Not quite correct, for the QC/C, which is still a "mechanism of paper-based journals" must still be performed, and its costs must still be covered. Those costs, however, can eventually be covered up-front, out of a portion of the institutional S/L/P savings, once the distribution mechanism of paper-based (and online) journals becomes redundant in the face of the free, auto-archived (refereed) journal literature.

What is immediately true is that the author no longer needs to cede his give-away rights in exchange for QC/C-cum-distribution. The two are now dissociable, the dissociation IS the resolution of the Faustian Bargain, it is greatly to the benefit of science, and it saves everyone money -- except, alas, publishers, who will have to downsize into the new niche of being the QC/C provider only.

Having to downsize is always regrettable, but if one's former enlarged service is no longer necessary, and it is to everyone else's benefit that one scale down and phase it out, one must do so. I am sure that journal publishers will not try to hold give-away research reports hostage to S/L/P access barriers merely to protect their own revenue streams, now that it is clear that this would be contrary to the interests science and scientists.

The reader will find it instructive to weigh the rationales that Dr. Jacobson mentions below (not on his own account, but as rationales likely to be invoked by journal publishers in defense of holding the literature hostage to S/L/P tolls) in terms of their SUBSTANCE: Is there any SUBSTANTIVE advantage to SCIENCE inherent in these rationales? Or are they merely rationalizations for preserving the status quo at any cost, irrespective of what is optimal for biomedical science and scientists, and even when it is no longer either necessary or justifiable.

> Of course, according to Harnad, journal imprimatur will still be needed> and useful for vetting the quality of work. But authors should be free> to publish online copies of their work before it is accepted by> journals ("preprints") and after it has been accepted ("reprints").> This online publishing by authors is what Harnad calls> "auto-archiving", and already is the standard in the world of physics.> Why should biomedical journals allow this to happen? Because they have> lost their power over authors, since researchers are no longer> dependent on

journals to distribute their work.

Almost exactly correct, as stated (and although I think I coined both terms, and although I use "self-archiving" more often than "auto-archiving" because it is more self-explanatory, "auto-archiving" is the better descriptor, because it captures the "self" as well as the "automated" and "autonomous" aspects of the initiative).

But journal "vetting" (QC/C) is still essential, otherwise the only thing authors have to auto-archive is unrefereed preprints (and although technically the latter counts as "publishing" too, I think we do better to call it "vanity press," reserving the term "publishing" for the auto-archiving of the refereed reprints that have been accepted and certified by a journal).

Nor have biomedical journals lost their "power" over authors: on the contrary; it is no doubt this perceived/presumed power that is holding biomedical authors back from drinking from the waters of auto-archiving for the moment!

My argument is that there would be no ethical justification for journal publishers' trying to use journal submission policy or restrictive copyright agreements to prevent auto-archiving in the PostGutenberg era; this would simply be contrary to the interests of biomedical research and researchers in every respect. No benefit whatsoever to science would come from it.

Nor would it be practically enforceable (because there is an arbitrary, continuous, and slippery slope from a raw draft, mailed or emailed to a few fellow researchers through a bigger and bigger email list and eventually a web URL given to more and more researchers; and the same is true for successive revisions of the draft [in response to informal peer feedback as well as to formal peer review] all the way done to the auto-archived final refereed draft -- this is, by the way, virtually a recapitulation of the actual ontogeny of the LANL archive, the mother and model of all archives!).

So the only "power" here is a psychological one. But psychological powers have the disadvantage that they can be openly challenged. Let us now do so, examining what SUBSTANTIVE justification there might be to any efforts to deter research authors from doing the optimal and inevitable (and obvious) with their give-away research reports.

> The problem with this analysis is that it attributes the strangle-hold> of publishers over authors solely to the ability of publishers to> distribute scientific work. In fact, the reason scientific authors> desire publication in the most prestigious journals is the same> motivation that drives authors in other fields of endeavor: recognition> and career advancement, or just plain fame and fortune. Obtaining the> widest possible audience for their work is part of this, is both a>

prerequisite for and a consequence of recognition, but is not the> entire goal in itself; the goal is also recognition and career. Much> as I would like to believe that researchers want only "to reach the> eyes and minds of their fellow-researchers with the reports of their> research findings", I fear that the motivations of most of them are> somewhat more complex.

They are indeed. But those further fame/fortune goals are perfectly compatible with auto-archiving; indeed, auto-archiving can only ENHANCE the impact of their work (on eyes, minds, and thereby citations, further research, fame, and fortune).

What is REALLY at issue here (attention reader!) is the role of the journal BRAND-NAME in all this. But of course the brand-name is the second "C" in QC/C! Research quality is first evaluated and then raised to the journal's acceptance threshold (if that is possible) via peer review, revision, and if necessary re-refereeing, etc., and then the accepted final draft is certified with the journal's brand name, attesting to the quality level it has attained.

The value of this QC/C service is uncontested. But what is there about it -- logically, practically, ethically -- that implies that it can only be had at the cost of denying the author's right to auto-archive?

A journal's patina, after all, its quality, its impact factor, etc., are all a consequence of its QC/C. And that QC/C must continue to be implemented and paid for, if all the fame/fortune benefits are to continue to be had. But in what respect does the quality, QC/C and fame/fortune vouchsafed by a journal depend on blocking access to what would otherwise be a give-away literature? This is the question to which authors should be seeking an answer from journal publishers. (And no substantive answer will be forthcoming, because there isn't one.)

The only reply possible is that that is how we have done it so far, it works, it brings revenue, and everyone is happy. But one might have said the same of horse-drawn carriages and steam engines: We can now have a lot more (in fact, infinitely more, in terms of the Net's potential spatial and temporal reach), for a lot less.

Nor is the right reply that the journals will soon all be available online too. For "available" does not mean free for all, hence it does not mean available to all. Proprietary online journal archives will still be behind the financial firewalls of S/L/P, and THOSE are precisely the access barriers that are at issue and at stake here, for this peculiar literature, which, one must never tire to repeat, is and always has been a GIVE-AWAY literature from the author's point of view.

We are talking about access (and access-denial) to the research reports of scientists who have no interest in fees or royalties or their accompanying access-barriers;

their interest is (to repeat) solely in maximizing the impact of their ideas and findings on the eyes and minds of their fellow researchers, present and future (and of course the RESULTANT effects of that on their own fames and fortunes) -- once they have successfully passed through the dynamic filter of QC/C (peer review).

Dissociating QC/C from distribution does not mean LOSING the magical effects of the brand name; it just means calling a spade a spade!

Let us not accept, as an excuse for preserving the status quo, a mystification of the fame/fortune effects of a journal's imprimatur. Holding the journal literature hostage to S/L/P tolls plays NO essential causal role in these fame/fortune effects. [It plays only one incidental causal role in fortunes (and whose fortunes those are is left as an exercise to the reader), but that role is no longer essential, indeed it now stands squarely contrary to the best interests of science and scientists.

The virtue of auto-archiving is precisely that it is "subversive": It allows the author to have his cake and eat it too: He can continue to submit his give-away paper for QC/C to the refereed journal of his choice, but concurrently he can also give it away publicly through auto-archiving -- right up to and including the refereed final draft.

As long as S/L/P revenues cover the costs, this is a stable situation, but once user preference for the free, auto-archive literature erodes S/L/P revenue streams, dissociation from distribution and downsizing to QC/C alone will have to take place, and up-front revenues to cover QC/C costs will be fully recoverable from institutional S/L/P savings.

The system will accordingly have been subverted precisely in the direction of what is optimal and inevitable for science (and not just for physics, but for all of science, which does not differ one bit from discipline to discipline in this respect, apart from what happen to be the sizes of the current revenue streams of their respective journals).

http://www.arl.org/scomm/subversive/toc.html

> As long as the leading medical publishers can dole out career> advancement by rewarding authors with publication, they will be able to> do so on their own terms and can continue to demand ownership of> intellectual property rights.

They are doling out career advancement by implementing QC/C, and those papers that succeed in meeting their quality standards are the ones that are rewarded. This service is essential, and it is essential that the true costs of implementing it continue to be paid. It is NOT, however, essential that they continue to be paid by

restricting auto-archiving rights; nor is it essential that they continue to be held hostage to further inessential costs (distribution) recoverable only by sustaining S/L/P barriers.

Here is an interesting question: Will the scientific community continue to comply with demands to transfer all intellectual property rights for this special give-away literature even as it becomes transparent that there is no real basis for demanding compliance other than the preservation of the status quo against what is optimal for science? And all that, with nothing more to prop it up than a known and trusted BRAND-NAME (and one whose quality standards are guaranteed by OURSELVES -- for we, the research community, are not only the authors and the readers, but also the referees [who contribute our services for free] and the editors [although we may sometimes forget that])?

Once it becomes clear that QC/C is a dissociable module, could it be that, as the increasingly tenuous copyright glue strains against the optimal and the inevitable, the QC/C module might actually dissociate, and break free, in the interests of at last freeing this give-away research literature for once and for all, for one and all?

> Just as the author of a detective novel> will sell her copyright to a publishing house in return for> distribution and financial reward, the author of a scientific paper> will sell her copyright to the journal, in return for distribution and> career advancement.

I think this misses the profound difference between the for-fee and the for-free literature.

Let us see what happens in the auto-archiving era; for even in the PostGutenberg era the detective novelist neither has nor WANTS any further options, whereas the give-away scientist always wanted and now at last has an option that allows him to give his research reports away; and the true causal underpinnings of this new option can only become clearer with time and open discussions like this, not murkier than they are now.

http://amsci-forum.amsci.org/archives/september-forum.html

For the record, though, detective authors do indeed SELL their texts, whereas scientists have always GIVEN THEM away. The "Faustian" analogy is to the soul, not the sale: Faust signs away his soul in exchange for immediate earthly rewards. In the age of auto-archiving, it will become increasingly apparent that there is no longer any need for scientists to pay this price...

> The ability to self-publish and self-distribute, or auto-archive, on> the Internet in no way lessens the ability of biomedical publishers to> influence the careers of

researchers, and thus does very little to> lessen their overall power over scientists.

Indeed it does not; and my concern is only to ensure that biomedical publishers make no attempt to CURTAIL that ability with respect to researchers work!

The journal brand names, certifying the quality standards that have been met by a research report, will continue to exist and to bring their rewards. But the give-away research report itself will be publicly auto-archived, free at last to have its full impact on one and all, without restraint from S/L/P.

> Why has the power of publishers apparently not succeeded in resisting> the power of the Internet in the field of physics? Although I am not> intimately familiar with the situation in physics, I would assume that> the amount of money at stake for publishers of physics papers and their> power over career advancement are not sufficient to win the battle.

I don't doubt the difference in the amount of S/L/P revenue at stake, but I would strongly doubt that salaries, promotion, tenure, grants, impact and awards are determined one bit less by the brand-name of the journals in physics than they are in any other discipline.

So is the difference in S/L/P revenues alone going to be the decisive factor in whether or not the rest of science is to be denied access to the optimal?

> In medicine, vast sums of money are at stake: the health care sector> comprises some 15% of our economy.

Please! What proportion of that 15% is journal S/L/P? Let's keep things in proportion here and not mix apples and oranges...

> Pharmaceutical companies have fortunes to spend on advertising, which> goes straight into the pockets of journal publishers.

Maybe they will succeed in obtruding those ads into some online archives (as others have on the Web), although one rather hopes not. But in any case, that is neither here nor there: Are lost ad revenues then a substantive reason for continuing to hold give-away research reports hostage?

> And the careers of researchers rise and fall on their publication in> the most prestigious journals.

This point has already been answered. The journal quality/prestige hierarchy can and will remain intact, irrespective of whether QC/C and distribution costs are coupled or decoupled, and irrespective of whether they are recovered through

reader-institution end S/L/P or through author-institution end publication costs paid out of institutional S/L/P savings.

So, please, let us not conflate either the dissociability or the cost-recovery model with the prestige value of the journal brand-name.

> Furthermore, many of the most prominent researchers are [a] on the> editorial boards of journals and [b] have a vested interest in the> continuation of this system.

It is certainly true that [a], but there are good reasons to doubt [b], for, when fully informed about the causal contingencies and noncontingencies, those board members are still OURSELVES, and it is unlikely that they will forget that their primary allegiance is to biomedical science and not to biomedical journal revenue streams.

http://www.sciencemag.org/cgi/eletters/285/5425/197#EL12
http://www.cogsci.soton.ac.uk/~harnad/science.html

> Thus, unless the "unpublished researchers of the world unite", and> overthrow the industrial-editorial complex (a rather unlikely> scenario)

The auto-archiving initiative is not analogous to a communist or anarchist revolution by any stretch of the imagination. And I strongly doubt that there is an "editorial complex" dedicated to opposing what is so obviously best for science. In any case, apart from the question of whether auto-archiving rights are contested or uncontested by journals, auto-archiving can follow its own subversive agenda without any further ado. Nothing needs to be overthrown; the public reports of scientific research need merely be given away, as they were always meant to be.

> the current status will not change greatly, at least as far> as intellectual property rights are concerned. *The New England Journal of Medicine*> will be able to enforce its Ingelfinger rule, if it so> chooses, and will be able to interpret and enforce its requirements on> authors.

Let us see whether it will be as easy to do so when the true underlying causal contingencies and options are relentlessly unmasked for one and all. One cannot second-guess the biomedical cavalry when it comes to water and drinking, but one can at least assure that they clearly SEE the water.

As to whether Ingelfinger should continue to rule, see:

http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Author.Eprint.Archives/0019.html

> Of course, journals are adapting to online publication and will> continue to do so. They will surely collaborate with the NIH in order> to allow more widespread distribution of their content, perhaps at> reduced cost. But this will occur in a negotiated fashion, and is not> likely to entail eliminating the toll-gate system that Harnad so> deplores. The NIH would be well advised to consider the copyright> system currently in place and its evolution (or lack thereof) in its> laudable plans to make biomedical information more accessible to all.

NIH is not interested in merely becoming an online S/L/P provider for journals with E-biomed, nor should it be. The journals' proprietary online archives can do that perfectly well for themselves, and for NIH to collaborate, even for the sake of reducing S/L/P costs, would be self-defeating, for it would be to let the Trojan Horse of S/L/P itself inside the gates of a public archive that is meant to be free for one and all. In fact, at this point, there is no contingency whatsoever between NIH's E-biomed and the journals (and implying that there was or would be one was highly premature, as I indicated in my initial critique of the first draft of the E-biomed proposal). E-biomed is to be a free, public, auto-archive, just like LANL. Official journal overlays would only come at a later stage, following rather than preceding the success of E-biomed along the lines of the success of LANL.

http://www.nih.gov/welcome/director/ebiomed/com0509.htm#harn45

In other words, E-biomed is not eliminating the S/L/P toll-gate system; it is merely offering authors a reliable, credible means of bypassing it, so they can give their unrefereed preprints and refereed reprints away for free for all, just as they had always wished to do, to the eternal benefit of biomedical science and hence all mankind.
