THE ROCKY ROAD TO 'COMPUTOPIA'

In his Reference Frame column "Publishing in Computopia" (May 1991, page 91) N. David Mermin proposes doing away with journals and peer review and replacing them with an electronic bulletin board to which research reports would be posted. Perhaps because he is a theorist, or perhaps simply due to a lack of space, he seems not to have considered some of the experimental difficulties with such a scheme.

Hardware. Mermin argues that peer review is ineffectual and that the reader should decide whether a given paper is "rubbish." Referees, however, frequently point out minor difficulties with papers that are nonetheless large enough to justify significant revision. So to the time spent deciding whether papers are "rubbish" we have to add the time spent asking and answering questions such as "What did you mean by this?" and "Why did you not try that?" The net result is that authors will start submitting revisions and revised revisions to the bulletin board, increasing the time everyone will have to spend making sure he or she has the most up-to-date version.

Mermin does suggest that a review panel be maintained for those submissions that require "official assessment . . . for private purposes," and he concedes that the decision of whether to send a paper to the panel should be left to the author. While some less competent scientists may avoid sending in their papers, I suspect most of us would welcome the opportunity for a review and find an excuse to consider every paper in need of evaluation. Thus the review panel will find itself just as overworked as referees are currently.

A third hardware problem is the question of who will be responsible for the upkeep of the archiving system. Assuming that one central facility is maintained, how are its managers going to handle such a (nearly) unwieldy computer system? If I want to see a paper from ten years ago, will I have to call APS in New York and ask someone to mount the 1981 disk or tape? Queuing problems are almost certainly going to be imposed by the physical limitations of any real computer system, which would probably be overloaded in a matter of weeks or months rather than years. And how are the managers going to store all of the disks and tapes and still cope with retrieval? How are they going to prevent the possibility that some disaster (such as fire or flood) will wipe out the entire repository? What happens while the computer is being backed up, or if it crashes or the phone lines go down? Am I to be expected to maintain my schedule around the people who maintain the computer system? Should an electrical storm in Iowa be able to affect my ability to access the literature?

Software. If all of the literature is going to be put on a bulletin board, then someone is going to have to write the programs to run it. How are figures and diagrams going to be included in electronic submissions? How will I make hard copies of figures? Is everyone going to have to have the same graphics software? And why should I have to invest in a particular computer system because that is what the powers that be have decided is the system of choice?

Besides the general problems of software implementation, there is another, more frightening difficulty. I predict that Mermin's network would be an irresistible target for hackers, who would do everything from planting viruses to crash the individual computers on the system to causing the deletion of the whole archive. On the other hand, the network would provide an easy way of dealing with rivals: Simply place a communication on the bulletin board with a hidden virus that will delete the data of anyone who reads it!

Money. Under the current system, I only pay for those journals of direct interest and only need to make occasional trips to the library to find out if I am missing anything in other journals. If every scientist is going to

have access to the whole network, how is each individual's payment to be calculated? If access will be limited to certain "journals," then to the costs in time and money already mentioned we have to add the costs of going to the library—a library that will almost certainly not have enough terminals to go around. How will payment be calculated for libraries? Will libraries still be able to choose to subscribe to a limited number of journals? Many scientists in third world countries cannot afford computers and their affiliated maintenance costs. Are they to be cut off from the literature?

I realize that it is possible that Mermin was kidding, or half-kidding, in his column (in which case he should have included a sideways smiley face, :-), like a true computer jock). But taking his column at face value, I have to conclude that even if he is correct that "journals are obsolete except as archival repositories, and even in this apparently benign role they waste such colossal amounts of shelving that plans are afoot to move them to compact disks," his proposal does not solve the problem. It either simply moves the problem somewhere else (as in the case of archiving and storage) or causes an entirely new set of problems. Perhaps Mermin should have submitted the column for further peer review (:-)).

I think there are other, less radical solutions. For example, if libraries were to begin canceling their subscriptions to Journals of Marginal Worthiness—and some are, for purely economic rather than scientific motives—then these JMWs just might have to go out of business. Mermin believes that helping to decide which journals to drop is a waste of time; I prefer to think of it as applying higher-order corrections.

As an admittedly utopian solution, Mermin's proposal may have some merit. But in the real world? I regret to say that I just can't buy it.

RICHARD SCHULTZ
5/91 University of California, Berkeley

I agree enthusiastically with David Mermin's thesis that the practices being followed in publishing research results are increasingly outmoded in the electronic era. I do not agree, however, that journals are the problem or that they should be eliminated. The problem is that we are continuing to communicate in print when that is increasingly unnecessary as well as enormously costly, wasteful and inconvenient.

Journals are not obsolete. They are essential for three reasons: validation, editing and distribution. Mermin dismisses validation as outmoded and ineffective. I do not agree. Indeed, as research continues to grow and as specializations continue to proliferate, the value of journal validation increases. It winnows out the chaff, sorts the wheat and sends it to market properly authenticated.

We are all aware of the degree of confidence that we can place in different journals. Do we really want to give this up in exchange for combing, ourselves, through a mass of contributions, including what would surely be an increasing number of hastily developed, poorly written pieces that would not be accepted by any current journal? I don't think so. The time invested by editors and reviewers saves a great deal of time for every reader.

Mermin recognizes the archival value of journals but regrets that this comes at the cost of an enormous waste of space and consequent loss of laboratories and classrooms—waste that is replicated in all of our universities as they continue to invest enormous sums in acquiring, organizing and maintaining highly redundant collections. I agree. The heart of the matter, then, is, Can we preserve the virtues of journal publication—validation, sorting, editing, organizing, preserving—and eliminate the vices—cost, redundancy, waste, delay, inconvenience?

Yes, we can—and the solution has many similarities to Mermin's proposal. However, there are several key differences. First, deposit of each article in the central electronic data store would not be made by the author. Rather, it would be done by a journal editor, to whom the author (as currently) would submit his or her work. The editor would make this deposit after the article had been reviewed, accepted and edited. Thus the article would be published electronically under the imprimatur of the journal that had accepted it. Each journal would continue to maintain its standards, preserving the sorting process.

Second, the data store would not be simply a bulletin board. It would be a central repository, organized and administered by the research–higher education community. It would be designed and maintained at a state-of-the-art level. It would be generally accessible to all members of the research–higher education community. Its collection would be comprehensive.

There is no fundamental barrier standing in the way of such a system.

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LETTERS

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It will, however, require that most difficult of all human accomplishments: institutional change.

ELDRED SMITH
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What David Mermin has forgotten is that progress in science is made by proof, not by edict. In preprints the authors can state anything they wish, and very few scientists have the time to study and point out the numerous errors in the flood of preprints being distributed. The facts that an average of 80% of the papers published in the physical sciences are revised before publication and that 10% or more are never published show the unreliability of preprints. Preprints are interesting to read, but scientists remain skeptical of the conclusions until they can be evaluated by experts—and not just by friends down the hall who may be reluctant to be honest with you or to study your preprints with care. Referees and good journals will not accept quotations from preprints for crucial arguments in journal papers. I realize that publication of papers is too slow by contemporary standards and will soon probably be replaced by on-line publication. But the review of papers to eliminate errors and unproven claims will remain an important part of the scientific method.

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In a recent Reference Frame piece, the establishment figure and provocateur N. David Mermin advocates abolishing refereed journals and replacing them by unrefereed postings on electronic bulletin boards. We would like to make some comments from our perspective as editors of *Physical Review D.*

In the last few years we have made progress in computer communication and publishing. We encourage referees to respond by e-mail, and about 50% do so. We encourage authors to prepare their manuscripts in the Latex or Revtex format and submit them by e-mail to our editorial office. (For information about procedures, see the General Information for Contributors in the January and July issues or inquire at the Internet address tex@aps.org or the Bitnet address tex@apsedoff.) The Revtex macro package enables the author to produce a version of the paper as it will appear when it is printed in the journal. This package and information on its use can be obtained from tex@aps.org or tex@apsedoff.bitnet. When accepted, the author's computer file may be used to produce the published paper. With regard to future developments in computer-aided publishing, we note that a thorough study on the future of electronic information systems has just been completed by a task force of The American Physical Society, and its report is now published.[1]

We believe that the refereeing process is an essential (albeit sometimes painful) ingredient in any form of publishing. Editors, with the help of referees and editorial board members, help set the standards in their field. About 40% of the manuscripts submitted to *Physical Review D* are rejected. If these high standards were not imposed, we expect that we would receive many more papers of at best marginal quality. About one-third of the remaining 60% of the manuscripts that are published are accepted immediately after a referee report is received. The other published papers are often substantially improved by the refereeing process, both in clarity of presentation and by the correction of minor, and sometimes even significant, errors.

We take this opportunity to thank Mermin for the careful reviewing he has done for *Physical Review D.*

Reference

1. Bull. Am. Phys. Soc. **36**, 1119 (1991).

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[*Editor's note:* The above letter was received by e-mail.]

N. David Mermin describes what is probably the inevitable future in suggesting that we abolish printed technical journals in favor of electronic bulletin boards. But the answer to his closing question of "What are we waiting for?" lies in conquering a problem also faced by the US Patent and Trade Office and the desktop publishing industry: We need faster, more compact and cheaper ways to store, retrieve and display images.

That images are important to technical journals can hardly be disputed. Equations might also be stored most easily in graphic form. In many journals, advertisements offer an irreplaceable way to circulate product information. Given a choice, who

would want to advertise in a text-only format?

The bottom line is drawn in terms of the almighty dollar. Suppose I want to "visit" an electronic library and read articles containing images. I will need a high-speed network connection and a high-resolution graphics monitor. Since I will probably download a copy of the journal for local consumption, my computer will need a very fast storage device plus a very fast processor to view the pages with reasonable speed. And this presupposes that the library uses a universal storage format that can be read by many different types of computers, or keeps multiple copies of the journal in different formats. Alternatively I could view the journal while on line, but this would require a very fast network connection indeed! In short, the entire chain of technologies needs to be faster and cheaper by an order of magnitude or more before electronic journals can become a viable alternative to paper ones.

But although we lack the technology to dive headlong into the world of electronic journals, perhaps we can begin testing the waters. Most patents rely heavily on illustrations, and so we must store them on paper as our great-grandparents did. On the other hand, chemical patents contain few images and are even now stored electronically. Can we not draw an analogy to technical journals? Some journals use almost no graphic images; let them become the pioneers of Mermin's vision. They will have to do without images for now, but at least they can address the many nontechnological issues such as deciding which articles get reviewed or posted.

CHRIS RUCKMAN
Virginia Polytechnic Institute and State University
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6/91

David Mermin's column "Publishing in Computopia" is right on the mark. Since it may seem radical to eliminate all journals, I suggest that we start by eliminating *Physical Review Letters*. Once we are convinced that we can live without a high-quality journal such as *PRL*, the rest may follow more easily.

PRL is clearly a journal that has outlived its purpose, namely, to provide rapid publication of important discoveries in all branches of physics. Nowadays, almost no one reads *PRL* for information outside his or her own specialty. The exciting developments in other fields are best learned about by reading the science news sections of PHYSICS TODAY, *Science* or *Nature*. Mermin eloquently stated the many reasons why submitting papers to *PRL* wastes the time and effort of authors, referees and everyone else involved. Moreover, promotions, raises and dispensing of grants are far too often influenced by one's publication success in *PRL*. This is a fallacious criterion given the uneven, subjective nature of the referee process. Rapid publication of research is now well handled by the Rapid Communications section of *Physical Review* and will eventually be best handled by the electronic publishing urged by Mermin.

RICHARD L. GREENE
University of Maryland
College Park, Maryland

6/91

THE EDITORS OF *PHYSICAL REVIEW LETTERS* REPLY: We thank Richard L. Greene for his praise for our first-class journal and for Rapid Communications. We agree with him concerning the inadvisability of basing raises, promotions or funding simply on the printing of a letter. Publication choices inevitably are based in part upon variable and noisy data.

Our agreement does not extend to Greene's assertion that almost no one reads *PRL*. There is published evidence to the contrary: See the report of the 1989–90 review panel for *Physical Review Letters*,[1] announced in the 2 December 1991 issue. Perhaps Greene is also a reader of *PRL*. He was sufficiently familiar with the journal that he could find a statement of our mission to quote. Further, one of us recently received a telephone call from him to discuss a letter for which he was neither author nor referee.

Reference

1. An executive summary of the report appears in Bull. Am. Phys. Soc., November 1991, p. 2553. Copies of the full report are available free of charge from The American Physical Society, 335 East 45th Street, New York NY 10017-3483, Attention: Evelyn Bernstein.

JACK SANDWEISS
Yale University
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GEORGE BASBAS
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GENE WELLS
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12/91

N. David Mermin argues for abolishing journals and for replacing them by various electronic-communication-related gimmicky procedures. I could not disagree more. Looking at the situation from the point of view of an author, as Mermin does, I can sympathize somewhat with his viewpoint.

But couldn't it simply be that the purpose of journals is not so much to meet our needs as authors but rather those we have as readers? And couldn't it be that the purpose of journals might be more than just to inform us in the most efficient way about what's going on in our own fields of interest? As a reader I am not at all dissatisfied with the present situation.

Just join me on a typical visit to the library. Somebody, maybe one of my students or a friend on the phone, just made mention to me of that one important paper I have to see right away. So I leave all the bureaucracy, the administrative problems and, most importantly, the telephone and all other electronic communication facilities behind and rush down to the library. That decision is bolstered by my Pavlovian expectation that the library is one of the very few places left at work where I cannot be reached at all. At the library, the mentioned paper more often than not turns out to be disappointing. Yet having the journal in hand, I cannot withstand browsing through the other articles.

And there the fun starts. Never mind that I can barely understand any significant number of those papers; there is always at least one that catches my fancy and I try to read it. Since the topic is not in my field of expertise I start to scan the references for an early review paper or a book that might afford me entry to the field. Soon I find myself in the section of the library carrying the older stuff. Needless to say, opening one of the older volumes of a journal is like opening a volume of the *Encyclopedia Britannica*: After turning a few pages I forget why I opened the book in the first place and dig into some other paper. Walking by the shelves in the library, I might also grab a recent issue of some other journal, maybe one not even covering physics, and start browsing through it. In any case, I usually end up reading a paper totally unrelated to the reason why I came to the library in the first place. Finally, after some time I realize that it is already half an hour into that all-important committee meeting, and with a smile on my face, happy to have been granted that extra free time, I join my colleagues in the job of making decisions on whatever important issues might move the university.

Does Mermin really want to take all that away from me? Does he know what interesting stuff I learned in that scandalously inefficient way about volcanic eruptions in Oceania or about the hibernation of ladybugs, just to mention two recent joys? And does he know of my excitement upon discovering in *Physical Review Letters* or somewhere else the first paper in a new field? (Scanning tunneling microscopy is a case in point.)

In contrast, the few times I absolutely could not avoid browsing through some electronic information retrieval system (that name is already a giveaway because "retrieval" has the connotation that something has been lost) I was happy when I got it over with. There is no way to compare the me who sits in front of a computer terminal with the me who reclines in a comfortable chair, scanning a journal.

Mermin at the end of his column asks two questions: "Why do we still live this way? What are we waiting for?" I hope I have given some answer to the first question. Concerning the second, maybe I just need somebody who is able to convince me to become better "computopiarized." By the way, I am always happy to see Mermin's prose in print despite having the privilege of being on one of his preprint mailing lists. Maybe I am just a bit old-fashioned.

ANTON ZEILINGER
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David Mermin's thoughts on "computopia" are no doubt inspired by the "colossal amounts of shelving" required to house the burgeoning Cornell physics library. But I wonder whether this is what really bothers Mermin. In previous columns, he has bemoaned the growth in the number and size of journals, and this problem would not be relieved by collecting what is now scattered in different journals into one place.

Another response might be to look more closely at what is driving the rapid growth in publications. Part of the answer lies in the growth of the number of physicists. This is probably for the good; in any case, it is not a trend that can be controlled easily by physicists themselves. But while the number of physicists has increased, funding has clearly lagged. (Many of Mermin's other columns in PHYSICS TODAY have discussed the difficulties that even good people have in getting grants.) Grant applications typically require one to submit a publication list. It is no secret that people are often judged as much on the length of their publication lists as on the quality of the papers therein. Thus it is no wonder that people will do anything they can to churn out more papers.

What can be done? If instead of a

publication list, applicants were instructed to submit their five best papers from the last three years (the numbers are arbitrary), the incentives would actually be reversed. Little would be gained from writing more papers, other than personal satisfaction, as the people evaluating the research would not see those extra papers. In particular, there would be no incentive to write two nearly identical papers or to break up a subject into many small papers. And since referees might *read* the papers, there would also be little incentive to do the opposite and write indigestible "kitchen sink" papers with many different projects stuffed in.

Such a system would be easy to implement. Only the relatively small number of granting agencies would have to agree to adopt a common set of rules for applications. They could enforce those rules by automatically deleting from applications any publication list that might "accidentally" find its way in. One might wonder whether grant reviewers would not be swamped by a rising tide of papers to read. To some extent, their problems could be eased by adopting another rule that would limit the text of an application to, say, four pages. Again, the rule could be enforced by mechanically deleting extra pages before sending out proposals for review. Far from being utopian, such a system has been partially implemented here in Canada by NSERC (the rough equivalent of NSF). NSERC does everything suggested above except that it still allows a publication list in addition to copies of one's best papers.

The above suggestions might seem to stray from the topic of computerizing libraries, but I believe that they go to the root of the problem that so worries Mermin and many others. Although growth is inevitable, we can control the worrisome trend toward repetitious and serial publication that has been responsible for a good part of the continuing explosion in the number and size of journals.

JOHN BECHHOEFER
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Your readers may be interested in a partial solution to the problems described by N. David Mermin. He points out that few people "have access to preprints, the current avenue of serious communication." One way to address at least *that* problem successfully has been demonstrated by *High*-T_c *Update*, an internationally distributed newsletter devoted to high-temperature superconductivity preprints.

High-T_c *Update* was begun in May 1987 at a time when it seemed that superconductivity breakthroughs were occurring faster than they could be faxed. In response to the urgent need to ensure rapid, reliable communication of research and to expand the network of researchers communicating with one another, the Department of Energy asked the Ames Laboratory to set up an information center and publish a newsletter.

The core of the newsletter is a list of preprints received during the previous two weeks. Each entry includes the preprint title, the address of the first author (including phone, fax and e-mail, if available), information about where the paper was submitted or presented, and key words and PACS numbers (if available). We do not referee the papers. The newsletter begins with "Nota Bene," a section highlighting some of the research described in the 100-odd preprints in each issue. Other sections include news, current events, resources and "FYI" (For Your Information).

High-T_c *Update* is published twice a month and is distributed both as hard copy (by first-class mail or airmail) and as electronic mail (sent over e-mail networks or downloaded from a dial-up account). It is available without charge.

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Detailed Breakdown of Broken Symmetry

The following is my contribution to the broken symmetry debate (May 1990, page 117; February 1991, page 13).

In general, symmetry with respect to a transformation group G acting on a physical system means that time translations and group transformations commute. For continuous transformations this may be expressed infinitesimally by a vanishing of the commutator $[H,G]$ of the group G with the generator of time translations, the Hamiltonian H. Thus symmetry is a property of the algebra of observables.

According to the principles of quantum theory a state of a physical system is described by a positive linear functional ω on the algebra of observables, and one naturally arrives at the question of how the concept of symmetry is realized on states.

One usually starts by looking at the ground state of a system, that is, the vacuum—the state in which the expectation value of the Hamiltonian is minimal. Sometimes it happens that there is a multiplicity of such states, naturally transformed into one another by symmetry transformations $g \in G$. In addition, it may happen that each of these states is invariant under the transformations belonging to a subgroup $K \subset G$. Thus the "manifold of vacua" is isomorphic to the homogeneous space G/K.

To get a unique vacuum one picks "spontaneously" one point of the vacuum manifold. In the language of algebras of observables we may say that a symmetry transformation (considered as an automorphism of the algebra of observables) changes the representation. This vacuum-representation-noninvariant realization of a symmetry may be called the Nambu–Goldstone realization and is often dubbed "spontaneously broken symmetry."

In this case, the spectrum of excitations does not form a multiplet spanning a linear representation space of G. Rather, due to results obtained by Heisenberg, Yoichiro Nambu and Jeffrey Goldstone, we encounter massless (or long wave) excitations, which are naturally associated with a nonlinear realization of the group G as a transformation group on the curved space G/K. In fact the Nambu–Goldstone modes may be viewed as excitations in the tangent space of this manifold.

The archetype of this phenomenon is the Heisenberg ferromagnet, for which $G = \mathrm{SO}(3)$ and $K = \mathrm{SO}(2)$. The manifold of vacua in this case is the 2-sphere $\mathrm{S}^2 = \mathrm{SO}(3)/\mathrm{SO}(2)$, each point of which may be visualized as a classical spin. The picking of one point of the vacuum manifold corresponds physically to spontaneous magnetization in a certain direction. Another prominent example is the spontaneously broken chiral symmetry of the nonlinear sigma model. Here we have $G = \mathrm{SU}(2)_\mathrm{L} \times \mathrm{SU}(2)_\mathrm{R} \cong \mathrm{SO}(4)$ (where $\cong$ denotes locally isomorphic), $K = \mathrm{SO}(3)$ and $G/K = \mathrm{S}^3$. The massless bosons are the pions in this case. Evidently, in reality this Nambu–Goldstone symmetry is only approximate (that is, it is explicitly broken). The antiferromagnet may also be modeled in this fashion, but the breaking is from SO(3) down to the discrete subgroup Z_2.

Superconductivity (treated in a gauge-invariant way) does not provide an example of the Nambu–Goldstone mechanism, since the Nambu–Goldstone mode does not correspond to a