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quo. (The "double blind" option recently adopted by *Physical Review* is a brave step, but it makes only a tiny correction in the imbalance of power.) Still, journals proliferate and physicists love to try new ideas, so one dares to hope for change. I call for establishment, under a provisional five-year charter, of *Physical Review E*: "*Let 100 Flowers Bloom Physics*," as a test bed for innovation in science journalism. Here's how it might work:

An article accepted for PR E gets, on a separate line after name(s) of author(s), "accepted by Personage." Here "Personage" is the name of one and only one (no committees!) member of the PR E editorial board. Addenda, errata and comments will cite this article as if it had appeared in a journal called "*Physical Review E* (*Personage*)." If an accepted article in PR E insults your intelligence, you might hassle the Personage associated with it; you'll be gentle, though, lest you get put on the board yourself. If, as has happened in some medical journals, an accepted article is later exposed as fraud, Personage will expect to feel real heat.

An article rejected by PR E and then abandoned by its author(s) disappears without a trace. Up to this point, one can fly "single" or "double blind." If not so abandoned, the title, author(s), abstract and "rejected by Personage" appear in a "rejected articles" section of PR E. This section is not indexed. Comments, errata and other references to rejected articles are forbidden. The board may adopt other measures to help fame and fortune elude authors in this section, though not by ridicule. A subscriber wanting to read a rejected article petitions the editor-in-chief (tearout form provided). This person, considering the weight and number of such requests, decides either to send out individual copies or to promote the article to a "subscriber's choice" section of PR E. Personage may append a rebuttal to an article so promoted or decline to be further associated with it. In the unlikely event that a promoted article is later cited by the Nobel committee, Personage may assume that somebody will search through the unindexed abstracts and expose him.

With bad judgment so difficult to hide, there will surely be articles for which neither a "reject" nor an "accept" vote can be found. These are what it's all about, and the only constraint on editorial policy is that they should be treated better than rejected and worse than accepted articles.

"How're we doing?" is an always-relevant question at PR E. Frequent reports describe how material is moving through the pipeline and how the various sections compare in the Science Citation Index. The Letters department of PHYSICS TODAY bulges with gripes and suggestions for PR E. Altogether, one foresees five indecorous, even raucous years. So delightful, in fact, that we'll wonder how life was possible before PR E.

Obviously I'm not serious about the name; something more sober than *Let 100 flowers bloom* must be found. I put it in only to remind us all that noble endeavors can just fail. But some of you need no reminder; you're itching to jump in and tell me PR E can't work. Feel free. To start the discussion, let's try to agree on the maximum credible disaster for the experiment.

Some will say it is this: An invidious author will vie for a "rejected article" just to get that name and assassinate Personage. If that's a serious possibility, then the scheme is dead. But I don't believe it. A guy who can afford a Mafia hit contract can afford a spy to penetrate secrets in the editor's office; referees are not now dropping like flies.

If you follow me on that, an alert editorial board can handle lesser abuses. An author floods "rejected articles" with abstracts on triondynamics? Limit how often a person may appear in that section. A subscriber stuffs the ballot box with phony petitions for a rejected article? The editor-in-chief was not born yesterday. And so on.

So the maximum credible (to me) disaster is that five years later we've all learned that the now-existing system for sharing power is the best one possible. Some persons already know that, of course. For them the maximum credible benefit of the experiment is the chance to crow "I told you so." But some of us don't know it yet; for us even a failure is worth the effort. But hey, physics fans! What if there *is* a better way?

R. W. HARTUNG

9/83 *East Lansing, Michigan*

Complaints from abroad

PHYSICS TODAY has, on a few occasions in the past, published letters from readers giving accounts of adverse circumstances encountered in accepting positions abroad. In so doing, you render a great service to the scientific community.

We very much regret to have to report certain experiences encountered at the Institute of Astronomy of the Swiss Federal Institute of Technology (ETH) in Zurich, Switzerland. We have both, independently, encountered very serious situations involving misrepresentation and professional discourtesy which would, no doubt, come as a shock to the many people who believe, as we once did, that Switzerland is a country with impeccable standards of honesty and integrity.

We emphasize that we are not concerned with sensationalism and, for this reason, we will not attempt to detail the long list of unpleasant incidents to which we were subjected; the following two examples should make the situation abundantly clear. Prior to our acceptance of positions, we were both given verbal assurances, from the head of the Radio Astronomy Group acting on behalf of the head of the Institute, that our stipends would be good by Swiss standards and commensurate with the senior positions that we already held. Stipends at ETH are based upon fixed salary scales with additions which, in principle, reflect scientific seniority, age, family size and so on. After we had been at ETH for a few weeks, we discovered that we were both receiving salaries comparable to that of a technician in the Institute who was still working on his doctorate. One of us (Spicer) went to ETH with the promise that he would receive support to initiate and to head a Plasma Astrophysics Group within the Institute of Astronomy. The other (Barrow) went to work on an experimental project with the understanding that equipment and support would be available. Neither of these assurances proved to be true.

We both endeavored to have these matters rectified, working our way through the appropriate administrative hierachy until we eventually reached the president of ETH. At each stage, we encountered no more than token interest and negative reaction. Only after the first threat of legal action did Spicer eventually receive a significant salary adjustment. Barrow did not take legal action and so never received any significant salary increase. Even though we were both able to establish, beyond all reasonable doubt, that the blame for these misrepresentations and other discourtesies lay with the head of the Institute of Astronomy and the head of the Radio Astronomy Group, no action was taken; not even an apology was forthcoming. We were left with the impression that we were dealing with an old-fashioned club in which all of the Swiss and a few privileged German-speaking foreigners were life members. Other foreigners were merely tolerated as intruders to be used as cheaply as possible, an attitude which is not uncommon in other fields of endeavor in Zurich.

To give some idea of the seriousness of these matters, Spicer went to ETH on a regular appointment but resigned after little more than one year. He took legal action against ETH, who eventually made an out-of-court settle-

ment. Barrow went on a temporary basis but encountered equally grave problems and agreed to act as a court witness on Spicer's behalf, should this be necessary.

Needless to say, we can document, or refer to witnesses, to substantiate all of the foregoing. Other senior members of the ETH Institute of Astronomy were shocked at the treatment that we received. We know of at least two other scientists who, within the past three or four years, have encountered similar problems working with one of the groups that now make up the Institute of Astronomy and who have since departed as a result of these.

We wish to emphasize that, generally, we found life in Switzerland to be interesting and pleasant. We encountered many very agreeable and helpful people both within ETH and outside. We would, however, strongly recommend that anyone considering a position should check the conditions very carefully and ensure that these are clearly stated in writing before making a final committment.

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6/83 *Greenbelt, Maryland*

RESPONSE FROM ETH: The case described by D. S. Spicer and C. H. Barrow in their letter is strongly distorted, as I will try to clarify.

For Spicer, who was employed as a regular staff scientist, I spelled out *in writing* the exact salary that he would receive in Swiss francs, a year in advance of his employment (letter of 27 June 1980, start of employment 15 June 1981). Later I arranged for Spicer to be moved up to a higher salary grade, which took effect on 1 January 1982. It was only several months after this salary raise had come through that Spicer expressed to me his strong dissatisfaction with his employment conditions, and started to take legal actions.

As a Visiting Scientist on a temporary position financed from several different sources, the salary of Barrow cannot be judged by the same criteria as those of the regular staff scientists of corresponding seniority.

In their letter, Spicer and Barrow compare their salaries with that of a "technician in the Institute who was still working on his doctorate." The person they refer to, who received his PhD in 1981, was, however, in a high salary grade because he had over a number of years played the key role of chief engineer within the Radio Astronomy Group, not because he was making a scientific career. The salary received by Spicer was considerably higher than what could be payed to Swiss postdocs on an academic track.

In spite of these inaccuracies, there is also some substance behind the dissatisfaction expressed by Spicer and Barrow, and their case revealed to us some personnel problems within the institute. We have therefore (before Spicer and Barrow wrote their letter) reorganized the Radio Astronomy Group to prevent such problems from occurring in the future.

I can assure you that many foreigners, most of them not German-speaking, have stayed or are still staying at our institute without having the feeling of being "used as cheaply as possible." Indeed, I am a foreigner myself (Swedish, with an American wife), who has been living in Switzerland for $3\frac{1}{2}$ years now.

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9/83 *Zurich, Switzerland*

Nuclear test ban again

In the debate on a comprehensive test ban published in PHYSICS TODAY (August, page 24), both parties—pro and con—focus on weapons reliability. "Pro" says we can have reliable nuclear weapons without continued testing; "con" says that continued testing of the weapons is required to assure their reliability. Both parties imply that reliability of nuclear weapons is a "good thing"—as physicists, we always strive for reliable tools. I wish to make the point that it is reliability of the entire system—the international security system—of which nuclear weapons are only a part, which is the "good thing"; that reliability of nuclear weapons is *not* to be desired and that, therefore, a comprehensive ban on the testing of nuclear weapons is a very desirable goal of the entire world community—scientific and otherwise.

Reliability is required of a weapon if one plans to use it; the planner needs assurance that his plans will be carried out precisely. The planned use of nuclear weapons implies the fighting of a nuclear war. In addition to large segments of the American and West European populations, there are many generals—the professionals who will have to fight any war—who have publically stated[1] that nuclear war cannot be fought, that nuclear weapons can have no role in war, that they are political and not military tools.

It follows that the only anticipated use for nuclear weapons is as a deterrent to an opponent's first use of such weapons.

Planned first use of nuclear weapons requires absolute reliability. The initiator of an exchange puts himself way out on a limb, which he must anticipate will be strong enough to hold him securely. The role of a nuclear first strike is to obliterate the opponent's capability to strike back or to reduce it to a "tolerable level." Failure to accomplish all of the goals of the first strike—nonreliability of the tools of the first strike—leaves the initiator open to fatal reply; he may succeed, partially, and thus murder his opponent but, in the process, commits suicide.

Deterrence does not require reliability—precise functioning according to plan. It just requires that a significant fraction of the weapons survive and work—more or less well. Deterrence does not necessarily imply the targeting and massive killing of civilians. Deterrent nuclear weapons can hold hostage the economic, political and military institutions of power and control of the opponent—most of them "soft targets." (Former Secretary of Defense Robert McNamara's original estimate, that 400 megatons-equivalent of nuclear explosive would be a sufficient deterrent, is very much an overestimate for holding hostage any modern society.) Given the many thousands of megatons-equivalent of nuclear explosive that are liable to survive even the most reliable first strike, the gamble taken by a would-be first-striker is very great. The gamble is even greater, and thus less likely to be taken, if his first-strike tools are of questionable reliability.[3]

Thus any procedure that can equitably reduce nuclear-weapon reliability reduces the inclination for a first-strike and hence adds to the deterrence of nuclear war. Such a procedure runs counter to the desire to use nuclear weapons for policy goals—for acquiring the ability to threaten an opponent into doing or not doing something. Such a procedure aids in preventing the opponent from doing just that to you.

We thus have another argument for a comprehensive nuclear-weapons test ban: such a ban doesn't make reliability impossible, but makes it much harder to attain. Continuous testing increases reliability, leads to faith in the efficacy of nuclear weapons, and thus increases the probability that they will be used—in some policy dispute or other. At that point we will learn, at great cost, whether or not nuclear escalation can be controlled.[2]

References

1. W. Schütze, "European Security Policy in the 1980's: Rethinking Western Strategy," Paper 3 in *Nuclear Disengagement in Europe*, S. Lodgaard, M. Thee, eds., Taylor and Francis, London (1983). See also: Union of Concerned Scientists, "No First Use, A Report by the Union of Concerned Scientists," Cambridge, Mass., 1983.