be done for the human rights of physicists all over the world, including Palestinian physicists. They do not need to be superstars like Sakharov to get the attention they deserve. We should be continually on the lookout for new, effective channels for action. But spreading disinformation about other human rights cases does not help them.

HARRY J. LIPKIN
Weizmann Institute of Science
8/89 *Rehovot, Israel*

DYSON REPLIES: I am glad to be corrected by Harry J. Lipkin if it is true that Andrei Sakharov violated no Soviet law. This does not at all affect the point of my argument. I am saying that both Sakharov and Tayseer Aruri were punished for political activities that the respective governments—Soviet and Israeli—considered dangerous. Neither was formally charged with any crime. Both were peacefully resisting the policies of an oppressive government. Whether or not either of them technically violated the law, the comparison between their moral courage and their tragic fates remains valid.

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Recent PhDs and the Politics of Productivity

At a time when we are busy congratulating ourselves for Nobel Prizes recently won for work that was actually done decades ago, it is disconcerting to learn from Paula Stephan and Sharon Levin's letter (October, page 151) that "the latest PhD cohorts were not the most productive in any of the subfields that we studied." This is unexpected. Since the late 1960s, physicists have been facing a very tight job market. One would think that only the very *best* would have been able to find academic positions. This group should be more productive than their predecessors, who graduated when anybody with a PhD could get a job. Maybe it wasn't the most productive physicists who landed the jobs after all, but rather those who were most adept at playing the game of academic politics.

ROBERT J. YAES
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STEPHAN AND LEVIN REPLY: Robert Yaes's explanation is, of course, plausible. As we point out in our forthcoming book (Oxford U. P., New York), other plausible explanations exist. One, for example, focuses on the fact that many of the academic jobs that were available in the late 1960s and 1970s (the period when the latest cohorts in our study got jobs) were not in the very top research departments but in the "expansion" departments that emerged in the 1960s. This may have had both a direct and an indirect effect on the output of the "latest" cohorts in academe. The direct effect is that those who got jobs in expansion departments often found themselves to have less time for research and fewer resources to support research than colleagues at the very top institutions (where jobs were extremely scarce). The indirect effect is that some very able physicists might have decided that a job in industry was preferable to a job at an expansion university that placed a somewhat low priority on research.

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Emptying the Physics Waste Paper Basket

I have spent almost two-and-a-half years working in solid-state physics in North America. Coming from a very different system as far as the organization of science is concerned, I was very impressed by many aspects of American research. I was equally surprised, however, by some negative phenomena.

Funding agencies and employers in the US frequently take the number of papers a scientist has published, or sometimes the total number of pages in those papers, to be a measure of the quality of that person's work. Therefore there is tremendous pressure to produce "garbage" papers, to artificially multiply the number of publications and so on. I have met scientists who were first authors on 15–20 papers a year and some who were proud to have their names on about 80 papers a year. It should be clear to anybody that it is not possible to write 20 decent papers a year or to participate in 80 different projects.

Because the refereeing system is inefficient, even at prestigious journals, bad papers often get published. It is very difficult to publish a paper criticizing such work: Both the authors and the referees of the garbage paper will defend their product, and the journal does not want to get involved in lengthy disputes. One of my American friends told me: "Writing papers is like sowing grain. The bad grain simply becomes forgotten, and the good grain brings you crops. The more you sow, the more you get."

This kind of thinking is a byproduct of the system. It does not take into account that garbage papers have a negative value—it is as if one has sown weeds. First of all, research is expensive, so bad papers waste the taxpayers' money. They can mislead other scientists: It is often difficult, for example, for an experimentalist to recognize a bad theoretical paper. And reading garbage papers is a terrible waste of time. At present the only barrier against producing garbage can be the conscience of the scientist. I know from other experience that people in general adapt to the system, however weird it may be. Only very exceptional individuals will act against it.

What could be done to improve the situation? I only have a few naive suggestions. First of all, whenever the evaluation of scientific work is necessary, each paper should be multiplied by its "quality factor," which could be negative. The funding agencies should not count the "number of pages per dollar" and should not expect ridiculous numbers of publications. The refereeing system should be improved: Maybe the job should be paid for; journals might increase page charges to cover the cost. There should be more space for papers criticizing other work; it should be recognized that a critical paper is as important as any other scientific contribution.

Science should be a search for the truth, not a paper factory.

WITOLD TRZECIAKOWSKI
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Fusion in a Solid: A Pump Primer

The claims for cold fusion are probably a mistake, although a few experimenters are still reporting anomalous behavior. However, the experiments have directed attention to nuclear fusion in a solid instead of a gas. A solid can provide the necessary concentration of nuclei without the high pressure needed in plasma fusion. The way out is to keep the idea of fusion in a solid and increase the energy of the bombarding ions. This could be done easily with an orb ion pump.

This ingenious pump was invented by Raymond G. Herb.[1] It consists of a stainless steel housing, which is grounded, and two electrodes. The cathode is a tungsten filament, which is kept at about +30 V above ground to prevent electrons from reaching the housing. In the axis of the cylinder is the anode, a thin tungsten wire at about 10 kV or more. Between the cathode and the anode is a shield at cathode potential, which prevents electrons from falling directly into the anode. The electrons, emitted tangentially, orbit in loops around the anode. They stay in orbit a very long time and reach the anode only after they have lost most of their energy in many gas collisions. Thus a very small emission current can produce a very large circulating current, which is used to ionize the gas. The ions are accelerated toward the housing, on which they impinge with energies of many kilovolts. The electrons released in the ionization process, together with the scattered primary electrons, fall on the anode, which is heated by electron impact. In the center of the anode is the supply of "getter" material, titanium or palladium, which evaporates and is deposited on the inner wall of the housing.

I tested this pump thoroughly and was impressed by its good performance and the large quantity of gas absorbed by the getter.[2] Unfortunately, at the time I had no need to test the pump with deuterium, and I don't know whether a fusion reaction would occur. Although the likelihood of fusion is small, I think it would be worthwhile to do this very simple experiment because of its potential great importance if successful. The experiment should also be tried with a mixture of deuterium and tritium. Hydrogen should be used as a control; a significant difference in the heat of the cylinder would be an indication that fusion has occurred. I don't have the facilities anymore to perform the experiments myself, but I hope this letter will encourage somebody else to try it.

References

1. R. G. Herb, Rev. Sci. Instrum. **35**, 569, 573 (1964).
2. R. F. Herzog, final report on NASA contract no. NASW-1314, 1971.

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Correction

April, page 77—The price of *Physics of Medical Imaging* is \$140.00 in hardcover (\$112.00 for AIP members) and \$39.00 in paperback (\$31.20 for AIP members). ■