measures that Landau was kept alive. Internationally known experts were called in for consultation, among whom were Wilder Penfield, the great Canadian neurosurgeon, and Alexandr Romanovich Luria, the great Russian neuropsychologist. A full account of this and the subsequent part of Landau's life is given by Alexandre Dorozynski.[1]

The "old brain" (the diencephalon) is a fairly rigid structure. The cerebrum (the telencephalon) is protected by the cranium. The white matter between the two is filled with neurons connecting the old brain, the intermediate bodies and the cerebrum, and has the consistency of Jell-O. During a closed head trauma, this white matter quivers, breaking some neuronal connections, thus causing diffuse damage. The myth that a blow to the head causes loss of memory is often untrue. The patients frequently retain excellent memory. This can be a severe hindrance to rehabilitation, for they remember what they could once do but cannot any longer, and so they may become very frustrated and develop severe behavioral problems. What they lose, mainly, is not memory, but function: They cannot think as fast or as effectively. There is always dementia of greater or lesser severity.

Because there had been so few survivors of closed head traumas at the time of Landau's accident, much of this was not yet known. Luria's expertise came from the study of patients wounded in World War II, most of whom had focal lesions. He developed an extraordinary skill at diagnosis of the affected brain function by the application of very simple tests. It was he, and not a psychiatrist, as Khalatnikov writes, who was testing Landau, who apparently did not appreciate what Luria was attempting. These tests may have been inappropriate, for they were designed to interpret local damage. In Landau's case, there must have been diffuse damage. As Khalatnikov reports, Landau complained constantly about a pain in his leg. No injury to the leg could be found. It therefore seems likely that some of the damage in his brain was in the upper area of the sensory portion of the Rollandic strip, a region located above the forward part of the ear in the cerebrum, or in the connections to that area.

Reference

1. A. Dorozynski, *The Man They Wouldn't Let Die*, Macmillan, New York (1965).

WILLIAM PRIMAK
Hinsdale, Illinois

6/89

Sakharov and Aruri: An Unapt Comparison

I was surprised and disappointed to see a letter by Freeman J. Dyson (August 1989, page 87) stating that "Andrei Sakharov violated Soviet law by his political activities." This is simply not true, and the Sakharov family has been very sensitive to this issue.

Sakharov was never accused of committing any crime. His exile to Gorki and the harassment and abuse of his human rights by the KGB were adequately documented and were never disputed by the Soviet authorities. It was clear that the reason for this treatment was his speaking out openly in criticism of his government and that there was no alleged breaking of any Soviet law. The Soviet authorities never gave any acceptable reason for the treatment of Sakharov, and there was not a single member of the APS who would have written letters supporting the Soviets like those that appeared in the August PHYSICS TODAY supporting Israel.

The debate in the May and August issues on the case of the Palestinian physicist Tayseer Aruri muddies the waters by mixing in not only Sakharov but also other previous human rights activities by physicists. Different situations require different tactics, and it is important to focus on the special aspects of each.

The case for supporting Soviet Jewish refuseniks who were not allowed to emigrate for various reasons is more complicated than that of Sakharov, but clearly legitimate and not controversial. The Soviet Union had signed a treaty with the United States, the Helsinki Accords, that guaranteed the right of free emigration. The US gave the Soviets a *quid pro quo* for this concession, and the APS had a legitimate right to demand that the Soviets fulfill this agreement in the cases of Soviet physicists who wished to emigrate.

In my discussions with Soviet physicists, including members of the Academy of Sciences close to the establishment, I never heard a single justification for the treatment by their own government of Sakharov or of the refuseniks who were denied permission to emigrate for so-called security reasons. They all said that the actions of their own bureaucracy were ridiculous and encouraged us to exert pressure from outside (without quoting their names, of course). Most of the individuals whose cases we brought up last year have now been given permission to emigrate.

There is unfortunately still much to

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
Rehovot, Israel
8/89

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.

FREEMAN J. DYSON
Institute for Advanced Study
Princeton, New Jersey
3/90

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
University of Kentucky
Lexington, Kentucky
10/89

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.

PAULA E. STEPHAN
Georgia State University
Atlanta, Georgia
SHARON G. LEVIN
University of Missouri, St. Louis
3/90

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
Polish Academy of Sciences
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3/89

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