

load from the UNIVAC.

Back in those pioneer days, my own work was always influenced by the stark fact that memory was limited and precious and had to be reused as soon as it was no longer needed. My programs were always compact and quick-running, and I usually produced one or two pages of output, rather than the hundreds that now seem to be in vogue—at least in reactor work.

My time of innovation is long past, and I recognize that today's programmable hand-held calculators are more powerful than the large computers on which we worked so long ago.

BERNARD I. SPINRAD

(bspinrad@aol.com)

Lake Forest Park, Washington

Peer Instruction Not Peerless; Students Should Memorize More

As noted in Robert Hilborn's review in *PHYSICS TODAY* (April, page 68) of Eric Mazur's *Peer Instruction: A User's Manual*, Mazur was so dismayed by the poor performance of his students on conceptual physics questions that he developed a teaching strategy called peer instruction to redress the problem.

Mazur raises a good point. As teachers, should we put more effort into teaching concepts? If so, what fraction of our effort should be involved? On the other hand, what fraction of our effort should go into problem solving, math remediation or any number of other things?

It is true that most first-year physics courses do not emphasize conceptual issues. Many instructors teach and test almost exclusively using numerical problem sets. But peer instruction may be just one of any number of ways to teach concepts. Even old-fashioned lecturing might work.

I have some problems with peer instruction. My own classroom experience has been that students sometimes convince one another of erroneous points of view, and some students may be led to think that truth is something decided by majority vote. (I do describe postmodernism to my students, but only to explain that it is unsound.)

I want experiments to tell students what is true. And if students start getting crucial concepts wrong, the best response is to run an appropriate classroom demonstration, not a class discussion.

My students also need to memo-

rize more, not less. Some students entering my college physics class don't know the area of a circle or that volume is length times width times depth. I expect them to learn the formulas for Newton's second law, kinetic energy, momentum, etc.

ROBERT JONES

(jonesrob@esumail.emporia.edu)

Emporia State University
Emporia, Kansas

Did Landau Work—or Not Work—on the Soviet H-Bomb?

I read with interest the articles by German Goncharov in your special issue, "New Light on Early Soviet Bomb Secrets" (November 1996). However, I am left wondering at the extent to which the Soviet Union's leading theoretical physicist, Lev Davidovich Landau, was involved. He is mentioned only in passing by Goncharov, and others have also regarded him as a minor figure in the H-bomb program. Yet, in the three years leading up to the first Soviet detonation, in 1953 (the year in which he was also awarded the title Hero of Socialist Labor), he was at the height of his powers but published no papers.

If he did not work on the H-bomb program, why did this prolific physicist fall silent for three years, and how did he persuade the Kremlin to grant its best scientist exemption from doing vital work for the state? If he did work on it, why is this not more widely known, and what was the nature of his contribution?

ANTHONY GARRETT

(scitext@c2.org)

Granchester, Cambridgeshire, England

(EDITOR'S NOTE: See Gennady Gorelik's letter to the editor in *PHYSICS TODAY*, May 1995, page 11.)

Unorthodox Parallel Revealed between Sarov and Los Alamos

As the golden domes of the pre-revolutionary Sarov skyline suggest (*PHYSICS TODAY*, November 1996, page 27), the Soviet "Los Alamos" had an earlier incarnation as one of the chief centers of Russian Orthodoxy. In the 19th century, Sarov was associated particularly with the "hesychastic renewal," a religious movement vividly depicted in book 6 of Dostoyevsky's novel *The Brothers*

Karamazov. Sarov's most famous hesychast was the forest hermit St. Seraphim,¹ an apostle of nonviolence whose radiant personality, friendship with animals and mystical vision of a cosmos pervaded by love have led many to term him the "St. Francis of the East." The building of the Soviet Union's atomic bomb in the vicinity of St. Seraphim's wood thus has something of the same grotesquerie as the building of America's near the Sangre de Cristo Mountains.

References

1. *St. Seraphim of Sarov: Selected Works*, Little Russian Philokalia, vol. 1, Saint Herman of Alaska Brotherhood, Platina, Calif. (1988). N. A. Motovilov, *A Wonderful Revelation to the World*, Saint Nectarios Press, Seattle (1985).

NORMAN HUGH REDINGTON

KAREN RAE KECK

(redingt@mit.edu)

Massachusetts Institute of Technology
Cambridge, Massachusetts

Ranking the Physics Departments—and the Weighting Game

I would like to comment on the critical issue of how best to rate physics departments, which was last discussed in your pages in October 1996 ("Letters," page 15) when John Perdew and Frank Tipler raised some valid concerns about ranking such departments.

Their findings show that the average faculty quality (as they measure it) is high at departments that are not normally highly rated. I think that there is a very good explanation for this. With the job crunch in academia in the past two decades, most departments have been able to hire faculty of the quality that they would not have dreamed about hiring in the past. There are outstanding faculty in most physics departments, and that is a point that needs to be recognized by students, deans, granting agencies, etc.

I have two general reservations about Perdew and Tipler's suggested ranking scheme. First, they are substituting one arbitrary measure for another. It is true that citations per faculty member is at least a numerical measure (as opposed to general impressions about department reputations). However, some subfields of physics tend to have more papers than others (and therefore more citations), and there are reasons other than high quality for citing a paper—for example, to criticize it or because

it involves calibration techniques for a certain type of instrument. There are also questions of how to deal with multiple-author papers. Other numerical measures, such as funding per faculty, also may be biased. Any department could argue that the measure in which it scores the best is the right way to rank departments.

Second, it is not clear that we want to normalize the effects of size out of our evaluations. Sometimes bigger is better. For example, a bigger department may cover more areas, offer a greater variety of courses or have a lighter course load per faculty. On the other hand, a smaller department may provide more effective student-faculty interaction.

The important point is that any evaluation is a weighted average of many stated and implied measures. The weighting is subjective and may depend on the audience applying it. Prospective undergraduates, prospective graduate students and prospective grantors are likely to differ in their assessment of the same physics department.

Perhaps we have outlived the need for rankings. Maybe we should just have a database containing an alphabetical listing of departments with whatever data seems reasonable: number of faculty, loading per faculty, papers, citations, funding. There should also be some evaluation of the undergraduate and graduate programs by current and former students. The audience or audiences can then make the appropriate weighting based on their particular needs. We are sophisticated enough to adopt such an approach.

MARC L. KUTNER

(mkutner@tuc.nrao.edu)

National Radio Astronomy Observatory
Tucson, Arizona

Why Do Minicomets Bombard Earth but Not Moon, Other Planets?

In your brief story on the minicomet bombardment (July, page 18), you didn't mention the obvious flaw in Louis Frank and John Sigwarth's hypothesis. They claim that 10 to 20 kiloton-sized minicomets reach the vicinity of Earth every minute and burn up in the atmosphere. By extension, the minicomets also hit all other bodies in the Solar System, including the Moon, which has no atmosphere for them to burn up in and therefore must be continually pelted by these intruders. If, say, sixteen minicomets hit Earth every minute, two also hit

the Moon every minute, which means that one hits the Moon's near side every minute.

In fact, though, despite all the probes we've sent to the Moon, we've never witnessed any natural change in its surface. Nor did the Apollo astronauts report seeing any such icebergs continually crashing into the Moon. So what happens to the ice after it crashes into the Moon? And why haven't we seen any minicomet effect on any of the other bodies in the Solar System?

One other point: Lightning in Earth's upper atmosphere causes the same dissociation of water molecules that Frank and Sigwarth attribute to disintegrating minicomets.

JEFFREY WINKLER

(aristotle2@goplay.com)

Hanford, California

The 'Search for the New': Looking Back at 50 Years of Physics

As a graduate student in physics at Princeton University back in 1946–50, I overlapped with two good friends whose names appear in the March issue of *PHYSICS TODAY*: David Bohm, the subject of a biography by F. David Peat reviewed by James Cushing (page 77), and Silvan S. (Sam) Schweber, the author of an essay entitled "Reflections on the Sokal Affair" (page 73). Both the review and the essay, as well as all the discussions I have seen on Bohm's life and the Sokal affair, miss one crucial issue: What is science all about?

What is it that motivates bright young people to study science rather than make a fortune on Wall Street?

Everything that I have seen written by social scientists about science is clearly off the mark. If what they call science is really science, I would certainly never have been attracted to it, and I doubt that many of today's successful scientists would have been.

I entered Cornell University as a freshman in electrical engineering in 1938 because I found building and tinkering with radios interesting. But the EE professors told us that there were no jobs or future in electronics and that we should study power engineering and AC and DC machinery and circuits.

Fortunately, some of us heard that there were new arrivals from Nazi Europe, Bruno Rossi and Hans Bethe, who were giving excellent courses in the physics building. We went over, listened to them and learned exciting material like Maxwell's equations,

which were not being given in engineering at that time (the engineers were violently against such impractical stuff).

Immediately after graduation in 1942, I was recruited by the new supersecret Radiation Laboratory at MIT to work on microwave radar. It was all based on Maxwell's equations and electronics, and the leaders at the lab were all physicists, not engineers. So, after the war, I went to Princeton to study physics, not engineering. My motivation was clear. What is practical today is out of date tomorrow. To be prepared for the unpredictable tomorrow, a student must learn to attack problems at the frontier, to make new discoveries and learn new things. The first discovery and thesis problem itself is not all that important. Rather, what is crucial is learning how to learn and search for the new and unpredictable.

I took two courses in frontier quantum mechanics. Only one was required, but most students wanted to hear two different approaches. A Danish visitor presented us with the party line from the Niels Bohr Institute. The great revolution that had led to the understanding of atomic phenomena was over. To understand the smaller scale of the atomic nucleus, a new revolution was needed. It would lead to a new theory that would be as different from traditional quantum mechanics as quantum mechanics was from Newtonian mechanics. It could be seen by the state of the art at that time, when all attempts to use quantum mechanics on the nuclear scale either gave nonsense or disagreed with experiment.

The second course was given by Bohm, who tried to show us how quantum mechanics had succeeded in explaining atomic physics and to guide us in looking for the puzzles and paradoxes that would provide clues to the new theory.

In the half-century since I heard those quantum courses, the old Copenhagen quantum mechanics has remained alive and well and has proved to be adequate for the study of smaller and smaller distances. But I have been exposed to one fascinating development after another and have managed to get in near the ground floor many times. Parity violation and beta ray polarization. The collective model of the nucleus and the Bardeen-Cooper-Schrieffer description of pairing correlations and superconductivity. The Mössbauer effect. The use of group theory in nuclear and particle physics. The unitary symmetry now called flavor SU(3). The quark description of hadrons. The