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physical reality and consequently we find that reality is mathematical in nature. Arthur Eddington stated that "the mind has but regained from nature that which the mind has put into nature."[4] And also: "We have found a strange foot-print on the shores of the unknown. We have devised profound theories, one after another to account for its origin. At last we have succeeded in reconstructing the creature that made the foot-print. And Lo! it is our own."

Pythagoras understood the importance of a string's length, Archimedes formed in his mind the idea of the weight of a displaced liquid and Newton activated the concept of the amount of matter of bodies. In these cases, and in many others in the history of science, physicists have looked for measurable quantities as a basis to support natural laws. The use of quantities in theoretical explanations and predictions, and in the analysis of experimental data, is innately connected to mathematics. Physics characteristically looks for natural laws that have a mathematical structure. This search is essential to physics and is one of the marks that distinguish it from other natural sciences.

In conclusion, mathematics is inherent in the construction of both physical reality and physics. We cannot imagine either a physical reality or a physics that is deprived of mathematics.

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Pioneer Recalls How Computers Replaced Handwork at Oak Ridge

This letter is prompted by your October 1996 issue that featured 50 years of computers and physicists. Since I was involved with computing in the late 1940s and early 1950s, I may have something to add to the story.

In 1948, I started working with Gerald (Jerry) Goertzel on automatic computing. The idea was to introduce such computing to Oak Ridge National Laboratory, which was then doing a lot of hand computations. We considered a number of different problems, but finally narrowed our work to the computation of internal conversion coefficients of gamma rays, which had just been formulated by Maury Rose. Jerry took Maury's formulation, on which he had also worked, and he and I reduced it to a scheme for computing on a von Neumann machine. Then we verified that our formulation worked by simulating a von Neumann machine with pencil and paper—for a single energy and a single nuclear charge. That took us about a month.

Armed with those results, we proceeded to look for a machine to do the computations in bulk. IBM had a suitable machine for our purposes, the Selective Sequence Electronic Calculator (SSEC) in New York City, so Jerry and I went there to work with IBM programmers on the problem. Unfortunately, IBM decided to charge us a pretty penny to use the SSEC, and we were forced to look elsewhere. Eventually, we made contact with Howard Aiken's group at Harvard University, and we were quoted a reasonable price.

The Mark I machine on which we worked was a relay calculator and externally programmed with paper tape. In other respects, though, it functioned very much like a von Neumann machine, except that recursive calculations had to be performed using a circular tape, which continued until convergence was reached, at which point it signaled us to change the tape.

I was the principal middleman with the Harvard group and worked with John Harr and Peter Strong both on programming and running the problem. As I recall, it took about two days to run the whole set of calculations. Then I took the results back to Oak Ridge, where Maury very quickly found errors in the results. Back, then, to Harvard, where the program glitch was found and corrected, and from which I took back results that were acceptable to Maury. The work was announced in *Physical Review* (volume 76, page 1983, December 1949) and finally published in the same journal (volume 83, page 79, July 1951).

By that time, I had moved to Argonne National Laboratory, which began building an early von Neumann machine of its own soon after I arrived. I did some consulting on programming there, but soon became involved as a consultant to the Naval Reactors Group, which was designing the first nuclear submarine, *Nautilus*. I was then an acknowledged expert on nuclear reactor computations, and quite soon was tapped to be the reactor representative on the Atomic Energy Commission's Computer Use Committee, which was charged with dividing up the "use pie" for a UNIVAC that AEC had bought and installed at New York University. As it turned out, the committee was completely dominated by Edward Teller, and we did little until Teller's requests were settled. Then the rest of us split the rest of the pie pretty quickly. Ultimately, the committee was disbanded as other machines started to take more of the computing 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.

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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 memorize 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.

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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?

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(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 prerevolutionary 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.

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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