LETTERS

Preventing Nuclear War

In his editorial "A run worth making" (23 Dec., p. 1281), William D. Carey suggests that the long-term global environmental and biological consequences of nuclear war provide a new basis for dialogue between Soviet scientists and scientists in this country. He is right. The key addresses of the Conference on the World after Nuclear War, Washington, D.C., 31 October and 1 November, were transmitted via satellite to Moscow. The Soviet scientific community was represented at the preparatory meeting for the conference in Cambridge, Massachusetts, 25 and 26 April 1983. Soviet scientists (V. Aleksandrov, G. Golitsyn, and N. Moiseev) were active participants in the conference. At the conclusion of the conference, a "Moscow link" was established enabling participants in Washington to carry on a real-time audio-visual exchange of views with their counterparts in Moscow. This exchange was witnessed by audiences in Washington and Moscow. The moderator of the Soviet panel was E. Velikhov, Vice President of the Soviet Academy of Sciences. I performed that function in Washington.

To sum up 90 minutes of "live" conversations via satellite (restricted to scientific issues), there was unanimity that first-order effects are so large, and the implications so serious, that the scientific issues need to be "vigorously and critically examined," as urged by R. P. Turco *et al.* (23 Dec., p. 1290).

This examination is under way by the world scientific community in the Scientific Committee on Problems of the Environment of the International Council of Scientific Unions. The chairman is Sir Frederick Warner of the University of Essex. The dialogue between the Soviet and U.S. scientists is thoughtful, lively, and constructive. It is enriched by the participation of scientists from other countries. This is appropriate, since one of the implications in the concern over the prospects of a nuclear winter is that the survival of individuals in noncombatant countries may be in jeopardy. The nongovernmental framework facilitates frank discussion.

Carey wisely uses the word "probabilities" to convey the limitations of the scientific method in specifying the consequences of an exchange of nuclear weapons on the biosphere. The full power of the scientific method cannot be brought to bear on this issue because of understandable constraints on validating calculations with experimental results. This places heavy responsibility on modeling the relevant physical processes.

I am persuaded that a substantial research effort will be required to confirm or modify the tentative conclusions published in the 23 December issue of *Science*. This research provides "a new basis for dialogue" that can "make a difference in the one matter that transcends all others," in Carey's words.

If cooperation in helping to prevent a nuclear holocaust achieves a modicum of success, perhaps the groundwork will have been laid for addressing in a positive fashion those global issues that must be resolved to achieve a more harmonious world.

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Science and Mathematics Education

Former Representative Mike McCormack, in discussing our current problems in mathematics and science education, states (Letters, 25 Nov., p. 874) that "this country cannot wait for a generation or more for quality education." Accurately noting that most proposals for improving the quality of science and mathematics education "would be long-range in their impact," he reasonably advocates support for legislation like the Glenn-McCurdy bills, which would have some short-term impact on this problem.

But as useful and desirable as various proposals that would quickly improve science and mathematics education may be, there is no significant hope that we can "catch up with the rest of the industrialized world" in less than a generation. Tax savings to businesses who release their employees to spend some time teaching in local schools and other comparable mechanisms will at best scratch the surface of the problem we face. Indeed, this is a case where the standard American solution of throwing money at a problem may be necessary, but is far from sufficient. Not until societal attitudes toward education itself and toward teachers change substantially and, perhaps, not until the organization of education in the United States is changed considerably are we going to be able to rebuild our science and mathematics teaching to where we would like it. In the meantime it is likely that there will be a substantial decline in the scientific prowess of the United States relative to that of our main industrial competitors.

What this country badly needs now is a commission that will assess the probable effects on our scientific establishment over the next, say, two decades of the current perilous state of scientific and mathematics education and will make recommendations on how to ameliorate these effects in the period before any solutions can become effective. Central to any such commission's task would be the recommendation of ways to try to change the societal attitudes that have brought us to our present predicament.

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Spin Correlation

Fritz Rohrlich's article on "quantum reality" (23 Sept., p. 1251) leaves out an important fact. In considering the significance of the spin correlation experiment, it is essential to keep in mind one simple prediction of the quantum theory: If the two spin measurements are along the same direction a, then whatever the value of $s_1(a)$ yielded by a measurement on particle 1, a subsequent measurement on particle 2 of $s_2(a)$ will invariably produce the opposite value.

In interpreting this fact, the quantum theory quite reasonably maintains that after $s_1(a)$ has been found to have a given value from a measurement on particle 1 it is meaningful and correct to attribute to particle 2 the opposite value, even before its direct confirmation by a measurement on particle 2. However, the theory also maintains that before the measurement on particle 1 it is meaningless to speak of a definite value of $s_2(a)$ that will be revealed by a subsequent measurement on particle 2.

Einstein objected to the idea that a measurement made on particle 1 could change in this way the status of particle 2, even when the two particles were very far apart. He preferred to believe that particle 2 must have carried the value subsequently confirmed by its direct measurement, even before the measurement on particle 1, whether or not the quantum theory was capable of assigning meaning to such a value. The "hidden variable" in this case need be nothing more than that value—not some obscure set of parameters required to satisfy deterministic prejudices. Einstein's position was based, not on his commitment to determinism, but on a strong distaste for the "spooky actions at a distance" suggested by the quantum doctrine that $s_2(a)$ acquires its value only as a result of the faraway measurement made on particle 1.

What is remarkable about J. S. Bell's analysis, and what the experiments of A. Aspect *et al.* confirm, is that Einstein's view not only is at odds with the ontological precepts of the Copenhagen interpretation but is also numerically incompatible with the results of other spin correlation experiments. It is worse than a breach of quantum metaphysics to assign a value to $s_2(a)$ before the faraway measurement on particle 1—it is demonstrably inconsistent with the observed facts.

It is tempting to say that the only property of particle 2 changed by the measurement of particle 1 is what we know about particle 2, as in Rohrlich's example of the tossed coin. But the suggestion that the major difference between the classical and quantum examples lies in the presence or absence of a detailed dynamics insufficiently emphasizes what is most peculiar about the quantum case. The state of the coin is heads or tails, whether or not we know it. Particle 2, on the other hand, does not possess a value of $s_2(a)$ until we carry out a distant measurement of $s_1(a)$. After that it does. It is this state of affairs that has given rise to some of the bizarre philosophical positions Rohrlich mentions. I share his distaste, but it should be stressed that many of those positions are hardly more peculiar than the unadorned facts.

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I am grateful to Mermin for pointing out that I have not been sufficiently explicit about at least one point in my article. He refers to the case when the directions a and b are the same, that is, when the two spinning particles have their spins measured along the same direction.

In this case one must distinguish two separate matters. One is the law of conservation of angular momentum, which guarantees that the spins of the two particles are always in opposite directions (no matter what direction is chosen), because the total spin of the system has been zero before the breakup into two particles. It means that no matter what the outcome of the spin measurement of the particle that is measured first, $s_1(a)$, the spin measurement of the other particle will give the opposite result, $s_2(a) = -s_1(a)$. No action-at-a-distance scenario need be invoked here. This is simply a matter of satisfying a conservation law.

The other matter deals with the prediction of the outcome of the spin measurement $s_1(a)$. It can be one of two values, and therefore $s_2(a)$ will be one of two values. Which one it will be is just as probabilistic as $s_1(a)$. But because of the conservation law, $s_2(a)$ is determined uniquely once $s_1(a)$ is known, whether $s_2(a)$ is measured later or at the same time. In that respect the situation is the same as the toss of a coin.

The difference between the coin toss (classical mechanics) and the breakup into two spinning particles (quantum mechanics) is (i) that the coin toss has a detailed dynamics which in principle can be known and then permits one to predict the outcome from the initial conditions, while the breakup does not have such a dynamics (no hidden variables that make the outcome deterministic); and (ii) that the quantum mechanical prediction involves probability *amplitudes* while the classical prediction (when the detailed dynamics is not known) involves probabilities. It is the latter difference that is responsible for the difference between quantum mechanical and classical correlations.

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Digit Counting

I congratulate *Science* on printing 23-digit and 29-digit numbers without typographical error in the item "What does it mean to factor?" by Gina Kolata (Research News, 2 Dec., p. 1000). I infrequently encounter an error of any sort in *Science*; however, I find it difficult to reconcile the claim that 2 to the 193rd minus 1 is a 58-digit number with its decimal representation 12,554,203,470, 773,361,527,671,578,846,415,332,832, 204,710,888,928,069,025,791. Perhaps 12 is considered indistinguishable from a single digit in this transcendent realm?

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Erratum: The review of *Temperature* in the issue of 6 January (p. 44) was written by Robert J. Soulen, Jr.