problems, will we ever have confidence in the system? The answer is obvious.

What are the consequences? We and the Soviets will build such systems. Because of their complexity, and luck, it is quite possible that only one will work, even if they are identical; indeed by chance a poor system may work while a good one does not. And there is no way to predict what will happen. Can we take the risk that theirs will work and not ours? Clearly we will not only be in a race for a better and larger defensive system, but simultaneously we will tremendously increase our offensive systems as well to overwhelm their defensive system in case it works and ours does not. And they will do the same. Thus there will be two arms races, offensive and defensive, mutually interacting, speeding up both.

It is this lack of confidence, which has no solution, technical or otherwise, that will prevent what Gerold Yonas suggests in his article: both superpowers making "significant reductions in offensive missile forces." The final phase, when "offensive missiles are at a negotiated low point," is very unlikely as SDI will so greatly increase the need for offensive forces. (If we can negotiate such reductions, why can we not do it without SDI; why is it necessary to spend hundreds of billions of dollars to encourage the negotiators?) What happens if theirs works but ours doesn't? And why are we subjecting our nation to this tremendous peril by building SDI, thus encouraging the Soviets to do the same?

Even the wildest hawk realizes there are finite resources that can be given to the military. With a spiraling double arms race taking huge amounts of money, cuts will have to come from somewhere. One source is obvious: Withdraw our forces from Europe, and perhaps surrender it to the Soviets. Are the hawks really willing to give up Europe and our commitments elsewhere for SDI?

Does not surrendering our influence in large parts of the world threaten our own way of life and perhaps mean its destruction? Cannot SDI (the suicidal defense initiative) become the means by which we destroy ourselves? Why spend several hundred billion dollars to do nothing but damage our national security and way of life?

RONALD MIRMAN

9/85 *New York, New York*

Reality and quantum theory

We enjoyed reading David Mermin's article (April 1985, page 38). We agree with him that the so-called Einstein–Podolsky–Rosen paradox is brushed aside by many physicists without good reasons, most frequently because of lack of adequate consideration and of will to listen. This superficial attitude is typical of many "reviewers" (who enjoy freedom from proving their points), with the result of a serious obstacle to experimental progress.

The meeting in Joensuu, Finland, hosted by Pekka Lahti and Kauko Mansikka on the occasion of the 50th anniversary of the EPR letter (proceedings published by World Scientific in 1985) and the symposium at Urbino hosted by Franco Selleri and Gino Tarozzi showed that the EPR issue is very much alive.

We wish to describe the EPR issue in a way that is perhaps more direct than Mermin's and that does not give the *gedanken* experiment the appearance of a mathematical game. In fact the most important point raised by the Einstein–Podolsky–Rosen paper is a very physical one: whether it is possible for two particles, after interacting with each other and after reaching positions very distant from each other, to maintain a connection with each other (as distinguished from carrying information) as if they were still "nonseparated." Quite generally, Mermin's type-2a physicists say yes, and cite examples of extended quantum-mechanical states, as Susan J. Feingold and Asher Peres do in their letter (November, page 15). They certainly point to the heart of the problem in saying, "It is only because we force upon the photon pair the description of separate particles that we get the paradox of Einstein, Podolsky and Rosen." However, it is not by force, but by nature, that the particles are inevitably separated. Moreover, particles that are separate cannot have their polarizations "inseparably entangled" as Feingold and Peres state (though a formula with such a meaning can be written on paper), because one cannot observe a polarization of a particle where its wavefunction is zero. Let us consider the recent experiment[1] of Alain Aspect and his colleagues where atoms of calcium are excited by two laser beams in opposite vertical directions and emit two photons that are detected if they travel in opposite horizontal directions. The lasers could be pulsed, and because the beams are focused by lenses, all the dimensions of the photon wave packets could be less than a few feet, compared with the 40-foot distance between the detectors. By no method could we observe one photon outside its wave packet. As a consequence of the Schrödinger equation the polarization of a photon could not be influenced by an action at a place where its wavefunction has zero amplitude. Thus the photons are separated beyond doubt. Yet a distinguished speaker at the Joensuu meeting wrote that "we must either abandon space–time localization or quantum mechanics." The *only* explanation offered was a badly modified verse of Dante condemning to the inferno those who disagreed. At any rate, the episode is truly symbolic of the fact that a justification—or even a discussion—of the nonseparation hypothesis cannot be found in the literature.

Once we accept that the two wave packets are separated, the typical EPR experiment is easily analyzed.[2] We have a source of pairs of photons (the beam of Ca atoms), symbolized in Mermin's figure 7 by the ornate console with two horns producing strange sounds, and two detectors on opposite sides of the source (the recipient funnels in the same figure). The source might produce, say, a thousand pairs per second. The *gedanken* experiment would require that if a photon of one pair goes into one detector, its partner will certainly go into the other one. In the actual experiment, that happens once in a thousand times, which fact is very important (and may rescue us from the inferno). Neither the photon going to the left nor the one going to the right is polarized in the ordinary sense. However, some quantum-mechanical considerations encourage (but do not compel) one to assume that a peculiar polarization exists, which is expressed by a formula similar to Mermin's singlet-state formula 1. That assumption contains the EPR paradox. In fact, let us indicate a plane of polarization, which must contain the (horizontal) line of propagation of the photons, with a letter, say, P or L, and the plane orthogonal to it with $\not{P}$ or $\not{L}$. The quantum-mechanical state of a pair can then be described, for example, as (P, L) or $(L, \not{P})$, where the first letter indicates the polarization of the photon at the right and the second letter the polarization of the photon at the left. With these notations the formula for the pair of photons from calcium is

$$(P, P) + (\not{P}, \not{P}) \qquad (1')$$

In appearance, the formula expresses the classical property that in any one pair both photons are polarized either in a plane P or in the orthogonal plane $\not{P}$. However, the quantum-mechanical meaning of the state 1′ is much more intriguing. First, the plane P is not specified, so P and $\not{P}$ can be any pair of orthogonal planes containing the line of propagation. Second, because P is arbitrary, any property formulated in terms of planes P and $\not{P}$ must remain true for whichever planes P and $\not{P}$ we choose. It is easy to see that in quantum mechanics this property is not admissible for separated particles.

Suppose we operate an analyzer of polarized light on the left and one on the right. Each analyzer is characterized by two orthogonal planes M and N and has two detectors indicating whether a photon was detected as polarized in M or N. For a photon polarized in a different plane P, the probability of detection in M (or N) is $\cos^2 A$, where A is the angle between P and M (or N). Thus if the angle between P and M is 0°, 90° or 45°, the probability of detection in M is 100%, zero or 50%. We choose M vertical and N horizontal for both analyzers. If 1′ properly describes the state, the analyzers will show that both photons, to the left and to the right, when analyzed separately, are unpolarized. However, if we study the left–right correlation, we should find that when the analyzer on the left registers a photon in the vertical plane M, the one on the right does too. That happens for 50% of the pairs, at random. If we repeat the observation a large number of times, we will notice no exceptions. A similar result holds for the horizontal plane N. Quantum mechanics then establishes that the source produces two distinguishable types of pairs, one with photons polarized in the eigenstate M, the other with photons polarized in N. Moreover, if we hold the analyzer on the left with its M, N planes fixed and rotate the one on the right to scan all planes around the direction of the photon, we fully confirm the finding that the source produces two types of pairs of photons as described. Now the point is that photons in an eigenstate of polarization are *all perfectly identical* to each other. No substructure of an eigenstate is even conceivable in quantum mechanics, as has always been taught. Thus, just the assumption that the state 1′ properly describes the situation for M vertical and N horizontal dictates an experimental result that *by itself* establishes that we have obtained *all the possible knowledge* about the source. We can now predict[3] the result of any other experiment done with the described setup. One inevitable prediction is that if we turn both analyzers so that their M, N planes are at 45° with respect to the vertical we must observe no correlation at all between left and right, because the photons are prepared in eigenstates of vertical or horizontal polarization. In turn, that conclusion rests on the facts that, given their separability, nothing acts on the photons after they leave the source and only the source can be responsible for their preparation in a given polarization state. The source consists of macroscopic machinery (pumps, magnets) so that it cannot change just because we rotate analyzers that are far away from it. However, our prediction entirely contradicts the property of the state 1′ that a perfect right–left correlation should *still* be observed if any rotation is applied

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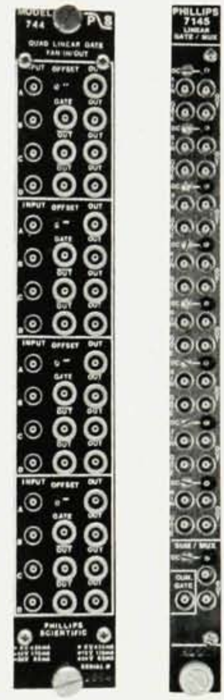

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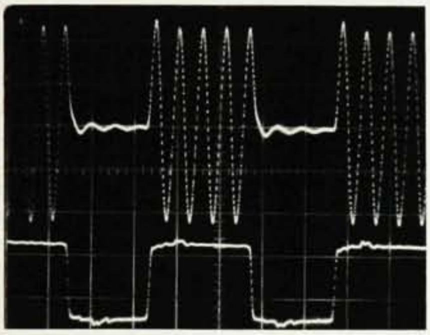

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equally to both analyzers. This is the paradox, in its physical and simple meaning.

We emphasize that our short description does not cover classical "hidden variables."[4] However, no theory of that kind exists today that could reproduce the successes of quantum mechanics.

The reader should notice that neither the pioneer work of Bell[2] nor philosophical discussions of reality have been needed in our reasoning.

We conclude that the EPR state cannot exist if quantum mechanics is correct as we teach it today. We emphasize that the proof of nonexistence should preempt any other argument. Clearly our proof would be invalidated by the existence of an action at a distance that would always make particles act as nonseparated even if their wave packets were separated by miles. We would be only too happy to see the proof of such a beautiful phenomenon. At the moment we are not attracted by the assumption of a miracle that allows us to ignore distances to explain a puzzle that originates only from distance. Unfortunately, while we concentrated on the question of existence we had to ignore the other side of the coin, namely the considerations, based on the quantum-mechanical formalism, that appear to compel the creation of the EPR state. Fritz Rohrlich (November, page 13) emphasizes the power of the superposition principle, which follows simply from the linearity of the Schrödinger equation. However, that principle is not to be interpreted as giving a sufficient condition for establishing the existence of a state. The superposition of a proton and a neutron as an isolated particle does not exist, while a superposition of two different neutral kaons does, and has[5] very peculiar properties. Also, while we surely expect the conservation laws to be respected, we must acknowledge our ignorance of what the final state of the photons is, so no conclusion is drawn on that account. The experiments are not *gedanken* experiments and thus have not yet given "the answer to Einstein." If Aspect's results had shown a correlation different from that of John Clauser and Fry then the existence of some unknown action at a distance would possibly have been demonstrated. As it is, the results seem to reject such a notion, unless we add to our inventory the miracle of velocities larger than that of light.

In conclusion, EPR remains a problem, and more work is needed. We make no progress attributing the problem to the inability of children of a classical world to understand quantum-mechanical amplitudes. The neutral-kaon system is much more challenging than is the comprehension of the simple dictum "forget about the distance," yet the undergraduate "children" understand it.

It is also important to note that classical mechanics, if given the same privilege of actions at a distance, is a more powerful tool for contriving fancy correlations because it has no inevitable uncertainties. Pictures of two brothers establish their resemblance better if taken with a sharp lens rather than with a hole camera.

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Oreste Piccioni
Werner Mehlhop
University of California at San Diego
1/86 *La Jolla, California*

Star Wars petition

I regard the "Star Wars" petition that is being circulated at many universities as misguided. The reason is not that I am in favor of SDI; I agree that the program is ill conceived and most unlikely to yield results anywhere near those advertised as its aim by the Reagan Administration. It is also, in my view, an enormous waste of money. I have, in sum, no quarrel with the first paragraph of the petition.

On the other hand, research on the various aspects of this effort is, *per se*, neither useless nor immoral. Indeed, it would be irresponsible of the American government *not* to support it at some level. I therefore find it quite inappropriate for those who oppose SDI to mount a self-righteous campaign to pressure other physicists not to participate in it (and that is surely what the drive does). The argument concerning the use of an institution's name is weak and irrelevant; universities can successfully protest such political misuses and have already done so.

However, the most distasteful aspect of the campaign, to my mind, is the implication that were it not for the stated objections, those who sign the petition might engage in SDI research. (Otherwise what meaning does a pledge "neither to solicit nor accept SDI funds" have?) Many of its signatories work in fields and have interests that would make their doing SDI research extremely unlikely. To use the pretense of self-restraint as an argument to tell others what not to do is surely hypocritical.

Roger G. Newton
Indiana University
11/85 *Bloomington, Indiana*

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The impropriety of anti-SDI petitions can perhaps be best appreciated by considering the following *gedanken* experiment. Imagine a group of biologists and physicians circulating a petition against accepting funds for AIDS research. They might argue that the involvement of prestigious institutions in such research would give legitimacy to homosexuality and drug abuse. They could make a case that AIDS treatments may not be effective against advanced cases, and that the virus might mutate anyway, defeating any therapeutic scheme. At the same time a less-than-effective treatment might give people a false sense of security and encourage them to indulge in aberrant behavior. This hypothetical group of scientists might further argue that AIDS is an effective deterrent against aberrant lifestyles, and that medical research against it might draw attention away from other programs such as preventive medicine. The thought that a treatment for AIDS might save at least some lives would never cross their minds.

Nicholas Zumbulyadis
12/85 *Rochester, New York*

Michael Weissman and John Kogut reply: Roger Newton raises some issues worth clarifying. The key paragraphs of the anti-SDI pledge are the ones concerning the overall dangers of the program (with which Newton agrees) and the pledge not to participate (to which Newton strongly objects). The paragraphs concerning the relations between universities and SDI are less important and were used on only some campuses, not including ours. It does remain true, however, that the SDI organization continues to cite university work and applications as evidence of the value of the program.

Many of us who started or signed the pledge are very much eligible to apply for SDI money. For example, one of us (Kogut) is currently trying to obtain support for constructing a compact supercomputer; the other has done consulting for Rockwell on infrared detectors. No one has claimed, nor has the press inferred, that most of the signers would have been likely to get SDI money soon—despite the prospect of an increasingly SDI-dominated research environment. However, nearly all the senior signers have risked irritating their granting agencies, their administrators and often some of their colleagues. Nearly all the junior signers have drastically limited their options in the job market. Thus the pledge has succeeded in making an