altogether, as I almost was. That is a real shame: We could be missing out on the next Einstein. There is no guarantee that at age 20 he would've done well on the GREs.

I don't think that a few students with lousy scores can do much, but if departments were to speak out against the present form of the physics GRE, maybe it could be changed.

PETER SHELDON
University of Massachusetts at Amherst
2/91

A Telescope Overlooked

We appreciate the brief mention of the Spectroscopic Survey Telescope, which our institutions are building jointly, in the article entitled "The New Ground-Based Optical Telescopes," by Buddy Martin, John M. Hill and Roger Angel (March 1991, page 22). However, since the funding for the telescope is more than 75% complete, since construction is expected to begin this year, and since its effective area of 57 m^2 is larger than those of five of the telescopes listed in the table on page 24, we might have hoped for inclusion of the SST in that table and a few more words about its unusual features.

FRANK N. BASH
McDonald Observatory
University of Texas at Austin
FRANCE CÓRDOVA
Pennsylvania State University
University Park, Pennsylvania
3/91

MARTIN, HILL AND ANGEL REPLY: We agree that the Spectroscopic Survey Telescope should have been included in our list of major new ground-based optical telescopes, and we apologize to Frank Bash, France Córdova and their colleagues for its omission. Nearly all the planned large telescopes are designed for maximum versatility; they are intended to cover the whole sky and a broad range of wavelengths with high angular resolution and wide field of view. Our article concentrated on these telescopes and the enabling mirror technology. The SST represents a unique and exciting departure, aiming for dramatic simplification and cost reduction by restricting the goal to spectroscopy and by limiting sky access. Such specialized telescopes have an important scientific role.

BUDDY MARTIN
JOHN M. HILL
ROGER ANGEL
Steward Observatory
University of Arizona
Tucson, Arizona
5/91

A Question of Mind over Measurement

In his Reference Frame column "On the Nature of Physical Law" (December 1990, page 9) Philip Anderson undertakes to reassure us regarding the epistemological integrity of the "seamless web" of science and to dismiss categorically any anomalous observations that seriously threaten to "rip the fabric to shreds." In a rather pejorative tone poignantly reminiscent of the prequantum Maxwellian era, he disparages those "who call themselves physicists" yet are foolish enough to attempt systematic study of the interface between human consciousness and physical mechanics. As one of the primary, if unnamed, targets of Anderson's blunderbuss, I would simply like to correct a few errors of fact and inference on which his case is based. In so doing, it may not be irrelevant to note that although his office is only a few hundred yards from my own, he has not visited our laboratory, discussed any of his concerns with me directly or apparently even read with care any of our technical literature. Had he done so, he would not have made several misstatements in his representation of our work:

▷ The credibility of our results, like those of several other serious scholars of this topic, does not rest on "statistical deviations at the few-σ level." We have in hand several prodigious data bases, acquired over 12 years of continuous, intensive experimentation, that clearly establish the existence, scale and primary correlates of certain anomalous influences of human consciousness on a variety of physical systems and processes. In our Microelectronic Random Binary Generators experiment, 95 unselected human operators attempted to shift the output distribution means to either higher or lower values than the chance mean, in accordance with their prerecorded intentions. In 3 850 000 experimental sequences of 200 binary samples, the overall results were that means in high-intention runs exceeded means in low-intention runs by 4.38σ. (The probability of chance occurrence of this outcome is less than 6×10^{-6}.) In our Macroscopic Random Mechanical Cascade study, 26 operators attempted, in 4170 experiments, to influence the output distributions of 9000 $^3/_4$-inch spheres trickling downward through an array of 330 pegs. Right-intention means exceeded left-intention means by 4.43σ (probability of chance occurrence less than 5×10^{-6}).

And in our Remote Perception study, where 36 people attempted to acquire detailed information about remote physical targets by means other than known sensory ones in 336 experiments, 89% of the participants and 62% of the experiments scored above the chance mean. The total information acquisition exceeds the chance expectation by 6.36σ (probability of chance occurrence less than 10^{-10}).

The composite likelihood of these anomalous yields occurring by chance in three independent experiments is less than 10^{-18}, an 8.76σ deviation from chance. These values are further enhanced by a number of other experiments in our program. Yet more persuasive are the operator-specific character of the results and their insensitivity to various physical parameters, including distance.[1] A recent quantitative review concluded that these and several hundred similar experiments conducted at other laboratories constituted mutual replications at the 15σ level.[2]

▷ Despite their high statistical significance, the intrinsic effect size of these anomalies—of the order of a few bits per thousand—is far too small to threaten the house margins of gambling casinos or pari-mutuel tracks, or to bear much relevance to séances and spoon-bending, as Anderson worries. Perhaps more at issue is the ultimate fidelity of hypersensitive diagnostic and information processing equipment functioning in operator-intensive environments or, as Anderson properly notes, the microstructure of physical determinism.

▷ Most sophisticated scholars of this subject have respected skeptical perspectives to an exceptional degree. Our own laboratory regularly entertains skeptical visitors and maintains ongoing, constructive communication with several of them. Last year we presented an invited address to a plenary session of the annual convention of the Committee for Scientific Investigation of Claims of the Paranormal, and we have benefited from subsequent exchanges with a number of its members. We have actively facilitated establishment of other laboratories, in this country and abroad, that are directed and staffed by conservative, mainstream physicists, who are now performing an array of similar experiments "under maximum security conditions" (as Anderson explicitly requires), some of which have already achieved results similar to our own.

▷ More trivially, but indicative of the care with which Anderson researched his position, our Random Mechanical Cascade experiment does not use "ping-pong balls," but $^3/_4$-inch precision solid polystyrene spheres specifically tailored to a fully automated and instrumented apparatus that has undergone far more extensive calibration and qualification than most physical research equipment.

The need for extreme caution in acquiring and interpreting data on such intrinsically complex and potentially consequential phenomena as consciousness-related physical anomalies is obvious beyond question. But it is nonetheless regrettable that a scholar of Anderson's stature, in attempting to instruct us on the requisites of sound scientific research, should omit from his list, and from his own demeanor, other items that have unfailingly characterized the most important advances in understanding—namely, thorough familiarity with the total base of relevant knowledge, humility in the face of new empirical evidence, and openness of mind.

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ROBERT G. JAHN
Princeton University
Princeton, New Jersey

2/91

Boy, for a Nobel Prize winner, Philip Anderson doesn't know much about experiments. In his Reference Frame column "On the Nature of Physical Law" he pooh-poohs the notion that thought processes can interfere with physics. Any experimentalist could have told him that he is wrong. If you turn your back on an electronic counter, it will certainly start counting backwards. If you go to the bathroom, the temperature regulator is sure to fail. If you start a scan and then go to lunch, the stepper motor will invariably jam just after the door closes. And if you should dare to take a vacation, there is no limit to the disasters that can happen. All of these things occur because physical laws are only obeyed when a source of brain waves is nearby, and it is well known that these obey an inverse-square dependence on distance. Experimentalists all recognize this fundamental truth and are usually afraid to leave the lab because of it. This is why most experimentalists are humble, shy people who show kindness to

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children, small animals and delicate instruments. Most experimentalists are also deeply religious ("Jesus Christ, why doesn't this goddamned thing ever work?!"). So come on, Phil, ask your colleagues next time!

PAUL KOLODNER
AT&T Bell Laboratories
1/91 *Murray Hill, New Jersey*

ANDERSON REPLIES: Robert G. Jahn is correct that his work with his associates at Princeton was among the kinds of work I had in mind in writing the Reference Frame column he refers to. I am a theoretical physicist, not an expert on statistics or an experiment specialist, and while I have spoken to people of both those kinds who have talked with Jahn, I feel our differences would not be helped much by the pleasant collegial chat he suggests. My article was meant to explain why not.

What my piece actually said *was* within my competence as a theorist, which is to make logical connections, and the logical point I made is that physics as it is practiced, and specifically precise mensuration, is not compatible with Jahn's claims; one must choose one or the other, not both, as he also emphasizes. If the "observer effect," as he calls it—or "magic," as one might equally well characterize it—is correct, precise measurement is not possible. His ideas are as incompatible with the intellectual basis of physics as "creation science" is with that of cosmology and biology. It is for this reason that I feel measurements such as Jahn does must be tested with more rigor and more suspicion than their proponents, for some reason, are ever prepared to undergo.

I am told that people who *have* looked in detail at Jahn's protocols have found some familiar problems—discarded data, in particular. This was the substance of some of his interactions with PSICOP, in fact. Mathematical statisticians are also unhappy about some of the work. I might add as an additional point that one problem with this kind of measurement in general is that the appropriate statistical technique is not the conventional method that one uses to measure a known effect and with which most scientists are familiar. (This is the method one is usually referring to in mentioning a "so-and-so-many-σ error.") The technique Jahn uses should be closer to the modern ideas about Bayesian estimation, which is the appropriate statistical method for testing whether an extra physical parameter is needed. Physicists are, regrettably, quite unfamiliar with Bayesian methods. The Bayesian approach builds in Occam's razor—the fact that a simple theory such as physical determinism is better, in some true probabilistic sense, than a more complex one, in that the more complex theory has extra parameters to do the fitting. Bayesian statistics are the answer to the old saw that "with enough parameters, you can fit an elephant." Using Bayesian methods Jahn's numbers would be much less "favorable." (An excellent discussion of Bayesian methods by Anthony Garrett appears in *Physics World*, May 1991, page 41.)

I do not see why it is relevant whether one uses ping-pong balls or "precisely machined spheres" in these experiments. I can live with reproducible ping-pong balls.

Paul Kolodner's amusing letter does not seem to make any point that must be answered. He is not saying that well-made experimental equipment gives wrong answers, I hope, or he may have trouble with some of his colleagues who measure such quantities as e^2/h.

PHILIP W. ANDERSON
Princeton University
5/91 *Princeton, New Jersey*

Ease the Way to Hiring Foreigners

I too am worried about the decline of US science. I also worry about the decline of science worldwide.

It seems odd to me that we worry about the declining availability of good scientists when many of the best and brightest graduate students are unemployable in the United States because of their nationality. I wish I understood better how the US can be a "Mecca" for graduate research, literally attracting students from all nations, but be a "Death Valley" for employment to those same students? Cannot the large body of foreign graduate students help us (and also the world)?

The current political agreements the US has with foreign countries prevent US companies from hiring the best people for the job. I cast my vote in favor of making it easier to hire foreign scientists and engineers.

KELLY TAYLOR
Texas Instruments
3/91 *Dallas, Texas*

A Jump Shot at the Wigner Distribution

John Philpott (November 1990, page 123) objects to the definition of the Wigner distribution function used in the extremely well-written and informative article on squeezed and antibunched light by Malvin C. Teich and Bahaa E. A. Saleh (June 1990, page 26). In their reply to Philpott, Teich and Saleh agree to his minor criticism and concur with his modified definition. Surprisingly, neither definition is adequate.

We would like to present an intuitive, physical argument to motivate the standard definition of the Wigner phase space distribution. The central ingredient of our approach is the notion of a quantum jump.

Consider a quantum particle at position x moving in one dimension with momentum p. Here the uncertainty relation allows for a quasiprobability only. In the spirit of Heisenberg's matrix mechanics, we replace the single position x by a quantum jump from an initial position x' to a final position x''. It is reasonable to identify x with the geometric center of these two positions: $x = \frac{1}{2}(x' + x'')$. But how to incorporate velocity or momentum into this picture of a particle hopping by an increment $\xi \equiv x'' - x'$? The physics of de Broglie together with the mathematics of Fourier provides the immediate answer: transformation from ξ to $k = p/\hbar$. But what is the function we have to Fourier transform in this way? Heisenberg guides us in finding the answer: He represents an atomic Bohr transition—a quantum jump from an orbital of quantum number n' into one of quantum number n''—by a matrix element $A_{n''n'} = \langle n''|A|n'\rangle$. Here A stands for any dynamical variable, such as the dipole moment. Similarly we now consider the density operator $\hat{\rho} = |\psi\rangle\langle\psi|$ for a pure state $|\psi\rangle$ and its matrix element

$$\rho(x'', x') := \langle x''|\hat{\rho}|x'\rangle = \langle x''|\psi\rangle\langle\psi|x'\rangle = \psi(x'')\,\psi^*(x')$$

in position representation. This accounts for our quantum jump from x' to x''.

To bring out the structure of this jump we express the function ρ in terms of the mean position x and the increment ξ, which leads to

$$\sigma(x,\xi) \equiv \psi(x + \xi/2)\,\psi^*(x - \xi/2)$$

This is the quantity we want to Fourier transform with respect to the jump increment ξ. Thus we arrive at