If it does, he argues, it would appear to violate the relativity principle of both special and general relativity, which is often taken to be the assertion that no such frame exists. Unfortunately there is some confusion about the content of the principle of relativity, just as there is about the principle of general covariance, which underlies the general theory of relativity.

The resolution of this apparent dichotomy comes from the realization that the relativity principle is in fact a statement about the invariance properties of the *laws* governing the behavior of physical systems and not about the invariance properties of particular *states* of those systems. In general the invariance properties of such states are a subset of the invariance properties of the laws governing those systems. No one is bothered, for example, by the fact that the underlying invariance of the laws of motion with respect to arbitrary spatial rotations is violated on the surface of the Earth. One recognizes that the Earth itself must be considered a part of the system and that the laws that describe it and the systems on it together are rotationally invariant. Likewise, the totality of laws—general relativity, hydrodynamics and so on—that together govern the large-scale behavior of the universe are invariant under local Lorentz transformations, while a particular state of the universe allowed by those laws is in general not.

In point of fact, one does not need the cosmic microwave background to define a global reference frame. In principle one could have used the average distribution of matter in the universe to define such a frame and could have measured the velocity of the Earth with respect to this frame long before the cosmic microwave background was discovered.

JAMES ANDERSON
Stevens Institute of Technology
3/93 *Hoboken, New Jersey*

An important feature of the relativity principle can be illustrated in response to the concern expressed in Robert J. Yaes's recent letter. As Yaes notes, the principle states that "no experiment . . . can determine a preferred reference frame," and he wonders whether the cosmic microwave background's anisotropy in all but one reference frame does not violate this principle. It does not, because a preference of the type implied in the principle is not established by the shape of the microwave background. The reference frame in which the distribution is isotropic can be said to constitute a preferred frame, but only in respect to the particular way in which the expansion began. There must be some frame in which the general expansion is most symmetrically viewed, and in this frame the background radiation is, unsurprisingly, isotropic.

What the relativity principle rules out is a reference frame that is preferred on the basis of how the *laws* of physics work. Preferential status of this type would exist, for example, if electromagnetic radiation in empty space traveled at the speed predicted by Maxwell's equations only as measured in one particular frame.

ALLEN C. DOTSON
St. Andrews Presbyterian College
4/93 *Laurinburg, North Carolina*

Robert Yaes raises the question of whether the existence of the cosmic microwave background, now observed so precisely with the Cosmic Background Explorer, is compatible with the principle of relativity, which Einstein phrased as follows: "The laws by which the states of physical systems undergo change are not affected, whether these changes of state be referred to the one or the other of two systems of coordinates in uniform translatory motion."[1] Although I do not claim to provide an "expert" response to that question, as desired by Yaes, it is my understanding that the principle of relativity is not necessarily violated by the mere existence of a universal reference frame. The laws of physics can still be invariant under some transformation of coordinates. This transformation is not specified by the principle of relativity itself, although we have discovered so far that Nature respects Lorentz invariance (the principle of the constancy of the velocity of light). What is really at issue here is whether an observer can perform purely local measurements to reveal his state of motion with respect to a universal reference frame. If there existed some *interaction* that violated Lorentz invariance, then velocity-dependent effects could become locally apparent to a moving observer. It is an open question as to whether such an interaction exists and, if it does, what defines the true rest frame respected by this interaction. It has become commonplace in tests of special and general relativity to assume that this rest frame is defined by the cosmic microwave background, but that need not be the case.

Nevertheless, our knowledge of the existence of the microwave background (which was unknown to Einstein) compels us to perform new tests of relativity that could reveal a local dependence upon our apparent motion defined by the dipole anisotropy. Recently a group of us at the Jet Propulsion Laboratory have devised a new test of relativity involving atomic frequency standards and fiber optic instrumentation developed by the NASA Deep Space Network.[2] In the experiment, an atomic frequency standard is used to modulate a laser carrier signal that is propagated along an optical fiber to another atomic frequency standard several kilometers away. Unlike in the Michelson–Morley experiment, the signal is propagated directly from one point in space to another. A violation of relativity would be apparent as a variation in the phase delay of the signal as the orientation of the system varies from the rotation of the Earth. It is possible to achieve a high degree of precision with this test, provided that adequate funding can be obtained for necessary refinements.

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TIMOTHY P. KRISHER
Jet Propulsion Laboratory
3/93 *Pasadena, California*

SSC: Of Finances and Fundamentality

Five hundred years after Copernicus directed our view away from the Earth and toward the larger universe, it seems that certain physicists still feel that they are the center of the universe. The letter to members of Congress urging the approval of funds for the Superconducting Super Collider (see PHYSICS TODAY, August 1992, page 59) is hardly a testimony to the broad-mindedness of physicists.

The authors of that letter wrote, "The approval of the SSC project in 1990 was widely acclaimed as our nation's firm commitment to be a leader in this scientific age." As a physicist working in a biological science environment, I can assure you that not every scientist, nor for that matter every other citizen, believes that spending tens of billions of dol-

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lars on this project is the *sine qua non* of American scientific commitment. In essence, every man, woman and child in the US is being asked to donate $30 (for construction alone) to the SSC. I hope that every physicist supporting the SSC is equally generous with his or her own money when environmental, religious or lobbying organizations come to the door asking for donations.

Although what I have to say has been said before, I would like to indicate a few reasons why I, and perhaps others, have trouble supporting the SSC given the financial situation of the country, even though I find the physics exciting and fascinating:

▷ Other than scientists working on the space station, every scientist can look at the SSC and ask, "Why do they deserve so much money?" As a researcher in radiation physics, I see the NIH budget supporting research in radiation oncology (medicine and physics together) at $16 million in 1990, and I marvel at the hundreds of millions being spent for the SSC detectors alone.

▷ The SSC is not being supported for the right reasons. The physicists in the project fully appreciate why the SSC should be built, but society at large does not. The arguments in Congress in support of it included incredible claims of "a cure for cancer" and so on. As numerous reports have noted, many in Congress view it solely as a gigantic public works project. Aren't there other areas in society that could benefit from an enormous public works project?

▷ The costs are delusionary. Even if it is built at or near cost, the operating costs will eat up public spending (and maybe general scientific budgets as well) for decades. DOE continually fights to find money to operate machines that it had no trouble building. I have seen two accelerators closed at Lawrence Berkeley Laboratory because of financial expediency. Both were used for medical research, and I can provide hundreds of cases of people cured of potentially fatal conditions who would not have been treated without such Federally funded accelerators. Why does the SSC deserve its money and not those accelerators?

▷ Any such huge project inevitably wastes money. Even if there is perfect administration and accountability, large sums of money will go to very, very indirect purposes. During the design stage of the SSC at LBL, it was necessary to hire a full-time travel agent to make travel plans for the participants. What researcher can't imagine a more direct use of that salary, not to mention airfare?

▷ The letter states that the SSC has "galvanized many foreign countries to follow us and collaborate." It seems to me that this is just untrue. Besides India, what other country has so far felt it worthwhile to participate in the SSC as a full partner?

Without my livelihood at stake, I am not likely to organize a large protest against the SSC. I merely wish to inform those who do support it that there are many people who are pro-science who, for a wide range of good reasons, do not believe that now is the time to fund such a project. Perhaps in five or ten years, the time will be ripe for a truly international collaboration on what is surely the single most expensive scientific enterprise in history.

MARK PHILLIPS
University of Washington Medical Center
8/92 *Seattle, Washington*

I write in response to the many news stories in PHYSICS TODAY and other periodicals concerning the fate of the Superconducting Super Collider. Although the US government might feel that limiting funds for research and reallocating the money to domestic affairs will relieve economic hardships, this path will only bring temporary gratification instead of scientific and technological breakthroughs that bring long-term benefits to humankind.

Throughout the 20th century, during both booms and busts in the economic cycle, science and technology never came to a standstill. In the midst of the Great Depression, Ernest Lawrence devised the cyclotron. At the same time, in economically afflicted Great Britain, James Chadwick was researching the neutron. Continual scientific research should also be the practice in the 1990s. Though the House of Representatives did designate $517 million in the 1993 fiscal year to the SSC, that amount of money is substandard, given the importance of the project to the future of high-energy physics.

The US government should be proud that the SSC is located in the United States, providing both new jobs and international prestige. As the APS executive board said in its statement of 26 June 1992 (see PHYSICS TODAY, August 1992, page 58), "cancellation of such a highly visible project would send a message to the world that the United States is relinquishing its long-standing commitment to fundamental scientific research." I believe that the same logic applies to the full funding of the project. The government should not let such a significant enterprise dwindle in either construction or operation. The world harbors such a multitude of mysteries that delaying scientific progress because of lack of governmental funding disgraces humankind's quest to solve those mysteries.

LISA K. RAMOND
Duke University
10/92 *Durham, North Carolina*

I find PHYSICS TODAY's reporting on the funding of the Superconducting Super Collider disingenuous to say the least. In at least a half-dozen places, the news story in the August 1992 issue (page 58) equates or quotes various bodies equating the "commitment to fundamental scientific research" with commitment to funding the SSC.

Let us analyze this equation. It is on the face of it incorrect, since, first, fundamental scientific research is much, much broader, and one may well be committed to that goal and yet ignore or neglect the SSC. Next, can someone provide the criteria by which we decide which science is more fundamental than another? If particle physicists do not wish to abandon the English language, they must show how TeV particle physics provides the fundament—the base—for other fields. Applying the Alvin Weinberg criterion—what impact does the field have on neighboring fields?—puts TeV particle physics at the very bottom of the class among fundamental sciences. The basic parts of medicine, biology, Earth science, chemistry and materials science have not, cannot and will not be affected one iota by whatever comes out of the SSC and its relatives of the last few decades. Amazingly enough, nor will 95% of physics. How can we test this? Easy. Use the Institute for Scientific Information computers to list the papers coming from those sources (on Higgs bosons, various quarks and so on) and how often they are cited in any of the above fields. Even the most esoteric theoretical corner of condensed matter physics rarely encounters the particle physicist's TeV realm. Indeed, it is clear that chemistry, materials science and civil engineering will contribute enormously to TeV particle physics, since the machine essential to that corner of physics needs the spin-in from many other sciences to even get started.

The claim of special fundamentalness is also, of course, preposterous hubris. I submit that any objective analysis would reveal research areas in a dozen different disciplines that could much better serve as the test for a nation's "commitment to fundamental scientific research." Funding the SSC could result in abandoning fundamental scientific research in many fields and will certainly assure American decline in both science and technology in a dozen different fields of chemistry, biology and materials science. Indeed, it is obvious that funding the SSC is merely funding a public works project. There is absolutely no doubt that the SSC has survived only on its pork-barrel merit, and the scientists who use that route to advance their own tiny corner of science will no doubt rue the day, as the national technological capacity and wealth will slowly wither in their ability to support basic research at all. Only rich countries can afford esoteric research with no purpose connected to the public good.

The smaller, equally fundamental sciences are also quantifiably more meritorious in their value to society. The astonishing fact is that so few among the scientists in those fields have the intellectual fortitude to make their own case for being at least as fundamental as particle physics.

RUSTUM ROY
Pennsylvania State University
9/92 University Park, Pennsylvania

Inertial Fusion Qualifiers and Qualms

I read with interest the article on progress toward inertial confinement fusion by John D. Lindl, Robert L. McCrory and E. Michael Campbell (September 1992, page 32).

In the paragraph preceding their equation 2, rather than defining ignition, as they claim, the authors have defined a fuel break-even condition. By picking the right value of "burn temperature" they can get any value for the fuel areal density ρr (ρ *and* r are the compressed fuel density and radius, respectively) that they want, and it was necessary for them to choose the temperature of 20 keV to get their "ignition" ρr value of 0.21 grams per square centimeter—a value more accurate than the method actually used to assess it can provide. Almost all quotes of the ρr value necessary for D–T ignition are based on numerical simulations, and most people are willing to venture only that the smallest value for which D–T ignition can occur is about 0.3 g/cm^2. (See the article by C. Martin Stickley in PHYSICS TODAY, May 1978, page 50.) Lindl has previously supplied all the equations needed to derive a reasonable minimum value of ρr for the case of volumetric ignition.[1]

Familiar examples of thermochemical ignition are striking a match and the combustion that takes place in a diesel engine. Thermonuclear ignition is similar. Prior to ignition in ICF the plasma state is determined mainly by the hydrodynamics of the implosion, but following ignition self-sustaining thermonuclear burn occurs and continues despite the expansion cooling that ensues. While it may be too involved for an article in PHYSICS TODAY to present the analysis that shows why ignition depends parametrically on ρr, the reader should understand that all historical definitions of fusion ignition are based on the concept of a threshold that is a dividing line between strikingly different behaviors of the thermonuclear plasma. The derivation of ignition conditions has usually been cast in terms of the rate of temperature increase due to the energy gain from deposition by the reaction products less the loss by thermal conduction, bremsstrahlung and other processes. For volumetric ignition, the condition of zero work rate by the confining shell (or "pusher") is appropriate for assessing the smallest values of temperature and ρr that allow ignition. In fact, in the absence of external support by compression, ignition can occur for a range of temperature and ρr pairs, but there is a minimum pressure P times radius r (proportional to the product of temperature and r) for which ignition can occur. For a given mode of ignition the Pr value ultimately required for ignition dictates what *initial* value of Pr is needed to insure ignition. An excellent illustration of the ignition process was given by Lindl.[1] However, for the dynamic case of hot-spot ignition, one must consider the residual velocity field in the hot spot. The physics of both volumetric and hot-spot ignition has been studied numerically by various authors.[2] More recently, the ignition conditions required for magnetized target fusion have also been discussed.[3]

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RONALD C. KIRKPATRICK
Los Alamos National Laboratory
12/92 Los Alamos, New Mexico

I am very disturbed by the tenor of the two articles on inertial confinement fusion in the September 1992 issue (pages 32 and 42).

The authors of the first article, John D. Lindl, Robert L. McCrory and E. Michael Campbell, state that the attraction of the goal of controlled thermonuclear fusion is (among other things) the view of fusion as a safe, clean energy source. Immediately I started thinking about neutron activation and the danger of tritium leaks into the cooling fluid.

William J. Hogan, Roger Bangerter and Gerald L. Kulcinski start the second article with the statement, "Fusion is potentially a safe, clean energy source." Yet later they state, "All the studies cited here employ *low-activation structural materials and blankets*" (emphasis mine). They show graphs indicating the expected radioactivity of an inertial confinement fusion reactor as a function of time after shutdown. While considerably smaller than that of a fission reactor, it is still sizable. Also, one has to keep in mind that the graphs show calculated values for the ICF reactor but, presumably, hard experimental values for the fission reactor.

In reading both articles I had to constantly remind myself that many of the references were to calculations of expected results, not to hard experimental facts. The first article states that the newest version of the Nova laser delivers 40 kilojoules of light onto a target, while the Omega laser delivers 2–3 kJ. The estimates of the required energies for indirect- and direct-drive inertial fusion shown in figure 7 give requirements of 0.3–2 megajoules for the former and over 1 MJ for the latter. Thus an increase in laser power by a factor of about 7.5 to 50 is required for practical ICF. Remembering the time when lenses were badly damaged after three laser shots, I wonder whether the behavior of the optical focusing elements can be predicted safely.

The last paragraph of the second article states the goal of these articles: to gain support, if not for the continuation of the ICF work as a whole, then at least for the development of powerful enough lasers. While I recognize that enthusiastic writing is necessary for that purpose, I am afraid that the tenor of these articles may defeat the purpose and