conference, Smith discussed the idea of twisting and referred to data his group had obtained. They seemed to bear out the validity of his decoupling scheme.

With a solution to the coupling problem that seemed very logical and technically possible, the people here at Brookhaven—myself from a magnetization standpoint, Morgan, Sampson and Dahl from a boil-off approach—started simultaneously to fabricate twisted and nontwisted material from a composite material furnished by a commercial supplier.

About the time I had data on shorter lengths and lower twist rates, Bruce Montgomery of the National Magnet Laboratory informed Sampson and me that Yuki Iwasa had obtained decoupling on long pieces of twisted materials in a flux-jump experiment.

The boil-off measurements required much greater amounts of material, and by the time Morgan twisted the material, I had the magnetization measurements finished. Then within a matter of weeks so were the boil-off measurements of Morgan, Sampson and Dahl.

The pioneering work and concept credit certainly belong without question to Smith and J. D. Lewin and their collaborators at Rutherford.

A. D. McINTURFF
Brookhaven National Laboratory

Lorentz, Riemann and relativity

In reading the article "Space, Time and Elementary Interactions in Relativity" by Mendel Sachs (PHYSICS TODAY, February, page 51) I was surprised to find no explicit mention of covariance or Lorentz transformations, per se, even though the basic principles are unmistakably there.

It might be a logical complement to the discussion to note that relativity is essentially a non-Euclidean dynamics based on Lorentz covariant laws, of which constant c is a special case, or, equivalently, covariance is a generalization of constant c, this generalization being the principle of relativity. Einstein was a great generalizer, this being one of the primary assets of any theoretical physicist.

At the same time, the use of Riemannian geometry in such a successful theory does not necessarily establish it as the true geometry of space–time, or prove Euclid wrong, any more than the use of Hilbert space in quantum theory establishes it as a reality in the laboratory (except perhaps by some stretch of the imagination).

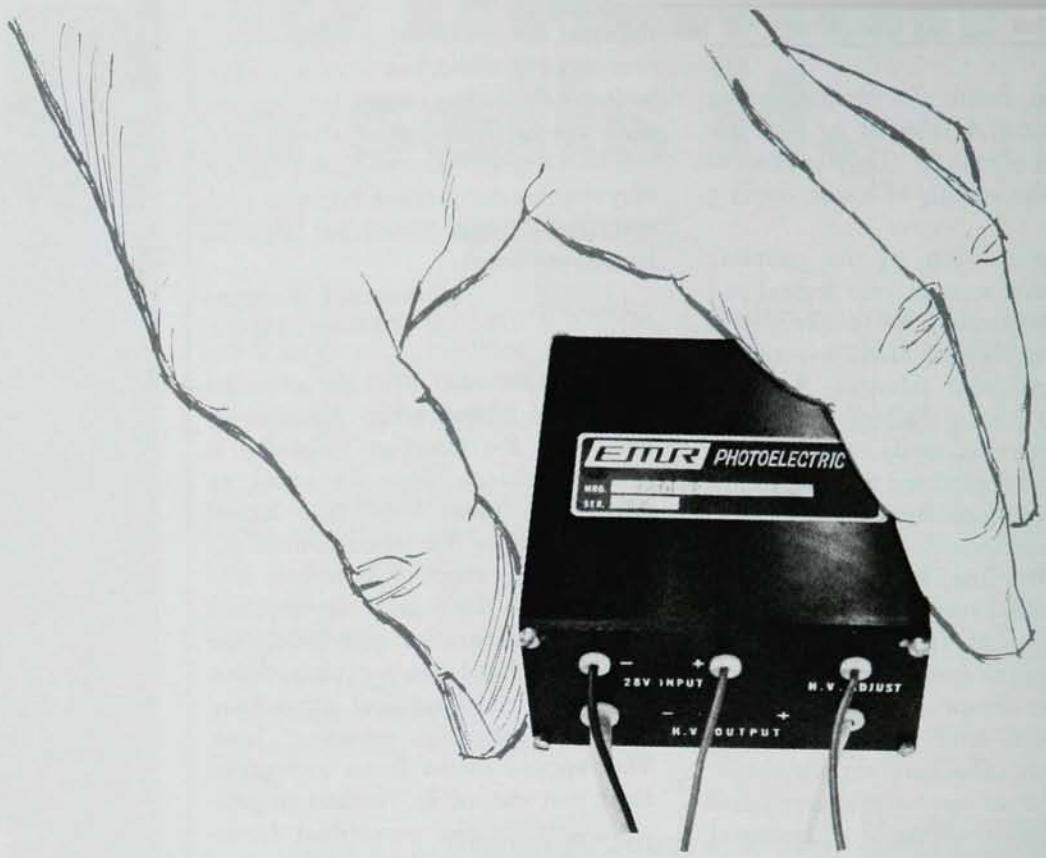
The simplest theory of the future may require compromise between past and present hypotheses (as well as brand-new ones).

KENNETH J. EPSTEIN
Chicago, Illinois

THE AUTHOR REPLIES: An essential point was missed when Epstein argued that the assertion "relativity is a non-Euclidean dynamics based on Lorentz covariant laws" is a logical complement to the discussion in my article. This assertion is in fact contradictory to the logical development in my argumentation that led to the conclusion that Lorentz-covariant laws are only a mathematical approximation for generally covariant laws. This approximation is an asymptotic limit that cannot be reached in principle (within the conceptual framework of relatively theory)—no matter how closely it can be approached!—so long as we can only talk about interacting matter.

The comment that "covariance is a generalization of constant c, this generalization being the principle of relativity" is not correct. The principle of relativity does not concern itself with the constancy of c as a primary concept but is rather a logical implication. The principle of relativity is concerned with the more general statement about the invariance in form of the laws of nature when they are compared among relatively moving observers.

I agree with Epstein's comment that the success of Riemannian geometry does not necessarily establish it as the true geometry of space–time. In fact I do not believe that within the framework of the conceptual structure of relativity theory there is such a concept as "the true geometry." I did try to stress the *utility* of Riemannian geometry in general-relativity theory (that is in the mathematical expression that we have thus far been using to exploit the ideas that underlie the theory). I also emphasized the lack of validity in the use of Euclidean geometry in any *exact* expression of a theory of coupled matter. In this regard, it is interesting to note the similarity between Einstein's break with Euclidean geometry, as a self-evident and "true" set of relations between space–time points, and the earlier



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break with the absolute notion of the geocentric model of the universe, which led to the Copernican revolution. In the latter development, the spatial positions of matter in the universe became relativized. In the revolution of relativity theory, space–time became relativized, with an underlying geometry that was taken to be a representation of the matter distribution of the universe.

MENDEL SACHS
State University of New York, Buffalo

Confidence in the council

As a concerned member of the American Institute of Physics I wish to place a vote of confidence in the American Physical Society Council. Contrary to Jay Orear (PHYSICS TODAY, May, page 9) I feel that the council does represent me as a member of the majority who voted against the ill-conceived effort to move the forthcoming annual meeting place of APS from Chicago.

I find the views expressed by those who are urging this move to be disturbingly subjective. Do Orear and "his colleagues" think that such a council is a kind of electoral college with each member an automaton responding directly to a certain number of constituents? Is it not, rather, a group that the voting majority entrusted to run the affairs of the society as best they can within the rules of the society?

And if they are not doing so to our satisfaction, can we not always "vote the rascals out"? Also does Orear think that the vote of the January business meeting should take precedence over the membership-wide poll taken by the council? If so, where is the representation of the many like me who did respond to the poll but who could not come to the meeting?

Physicists should remind themselves that they are endowed by their professional abilities with absolutely no special competence in sociology or politics. How much more, then, must APS as a group avoid the arrogance of trading on public status and professional reputation of its group membership in areas far removed from the physical sciences!

How can the members from Cornell presume to teach the mayor of Chicago about how to handle public violence? Did Cornell do any better? How can APS, with membership from Cornell, Columbia, Howard, Berkeley and many other institutions of "higher" learning presume to do so? What proven competence do we have? Without such competence our "lesson" degenerates to presumptious effrontery. Perhaps, even, the lesson should come from the other side, Mr Orear. After all, no one was able to bring guns or cleavers to the Democratic Convention floor.

WILLIAM R. DRAKE
Alexandria, Virginia

Anti-Chicago pledge

With respect to the January 1970 annual meeting of the American Association of Physics Teachers and the American Physical Society in Chicago, some of us felt that irrespective of the official policy of APS many physicists would welcome the opportunity to state that they personally will not attend that meeting. Pledges to this effect have now been signed by 450 physicists at colleges, industrial and government laboratories all over the United States. Among the signers are: Joseph C. Callaway, Bellur S. Chandrasekhar, Leslie L. Foldy, Carl A. Levinson, Henry P. Primakoff, Fritz R. Rohrlich, Silvan S. Schweber and Malcolm H. Skolnick.

The pledge states:

> "Along with our fellow countrymen in every occupation and profession, we physicists were horrified to witness the police action in the city of Chicago during the 1968 Democratic National Convention.
>
> "Previous to the Democratic Convention, the American Physical Society arranged to hold its 1970 annual meeting in that city. We fear that our attendance at a meeting in Chicago might be interpreted as indicating either approval or indifference to the events that occurred there.
>
> "As witness to our dismay at the behavior of the Chicago police during the Democratic Convention, we physicists individually sign this statement that we will not attend the 1970 annual meeting of the American Physical Society in Chicago."

Our pledge was circulated among only a limited number of people. There must be many more who would like to state their affirmation. We so-