

Freedom and Tenure of AAUP. I would very much like the American Physical Society and the American Association of Physics Teachers to become endorsers of this document, and I am taking this opportunity to solicit action from the members of these organizations toward this goal. I have already written the executive secretary of APS and requested him to present the matter of endorsement for the consideration of its council and I urge fellow members of APS to do the same. Probably a similar approach is needed to secure the official endorsement from AAPT.

K. M. THOMAS
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Stable superconductors

In the summary of the third annual Applied Superconductivity Conference (PHYSICS TODAY, March, page 101) reference is made to the predictions of P. F. Smith and colleagues with regard to intrinsically stable superconductors, formed by twisting or transposing many fine superconducting filaments embedded in a matrix of normal resistivity.

The summary states that "the only formal report at the conference (in this connection) was A. D. McInturff's decoupling measurements . . ." We wish to draw attention to our paper entitled "Loss Measurements on Twisted Multifilamentary Superconducting Wires," presented as a post-deadline paper at the conference.

The conductors used in these experiments were twisted in our own laboratory. It is an "indication of the pace of modern technology" that similar wires needed to extend these measurements have been on order from commercial sources for six months.

P. F. DAHL, G. H. MORGAN,
W. B. SAMPSON
Brookhaven National Laboratory

Conference reports are not perfect. In retrospect I feel I should have noted the 150-kilogauss coil reported by Henry Schindler (RCA) as a significant achievement.

I apologize to Dahl, Morgan and Sampson for not mentioning their report separately. I felt it amplified McInturff's report, and I presumed

their wire was prepared by or in collaboration with him. It appears, though, that there is more individual competition than I had suspected at Brookhaven.

Regarding who got there first: Although Smith and his colleagues invariably acknowledge the contributions of Richard Hancox, Peter Chester, John Stekly and others, the major credit for work on intrinsically stable superconductors belongs to the Rutherford group. At the June 1968 Brookhaven Summer Study, Smith not only presented their theory and predictions but also some initial practical measurements including stability and ac losses in small coils. Sampson was chairman of this session and edited the proceedings. The Brookhaven group was certainly stimulated by this report. Any claim to priority could only be justified by subdividing the topic into rather restrictive categories.

Other recent reports on intrinsically stable conductors may be of interest: Y. Iwasa, *App. Phys. Lett.* 14, 200 (1969) and J. A. Good, P. A. Hudson *Cryogenics* 9, 164 (1969).

I can sympathize with the frustrations expressed by Dahl, Morgan and Sampson in their final paragraph. The delay can probably be attributed to their requiring a special conductor configuration, in which situation there often are unfortunate unexpected delays. The fact remains, however, that US industry eliminated a lead held by UK industry in a remarkably short time. Twisted fine-filament conductors were produced in the US shortly after the Brookhaven meeting and have already been delivered in quite large quantities.

Perhaps I should add that I comment as a nearly disinterested Canadian.

DAVID L. ATHERTON
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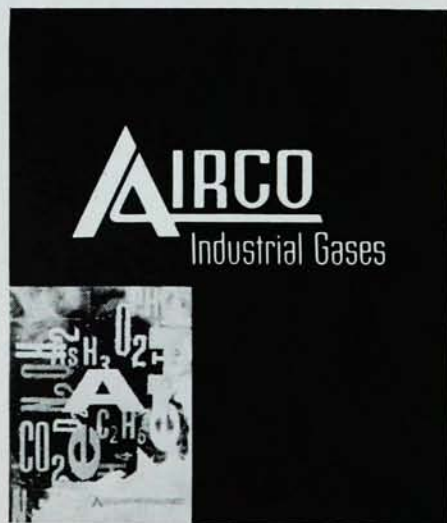
I shall try to reconstruct, as I know it, the chronological order of the work on twisted multifilament composites.

At the 1968 Brookhaven Summer Study, P. F. Smith from Rutherford, first in an evening bull session, discussed means by which one could decouple multifilament composites. Several persons, myself included, had obtained the experimental fact of coupling in untwisted composites. In a formal paper presented later in the



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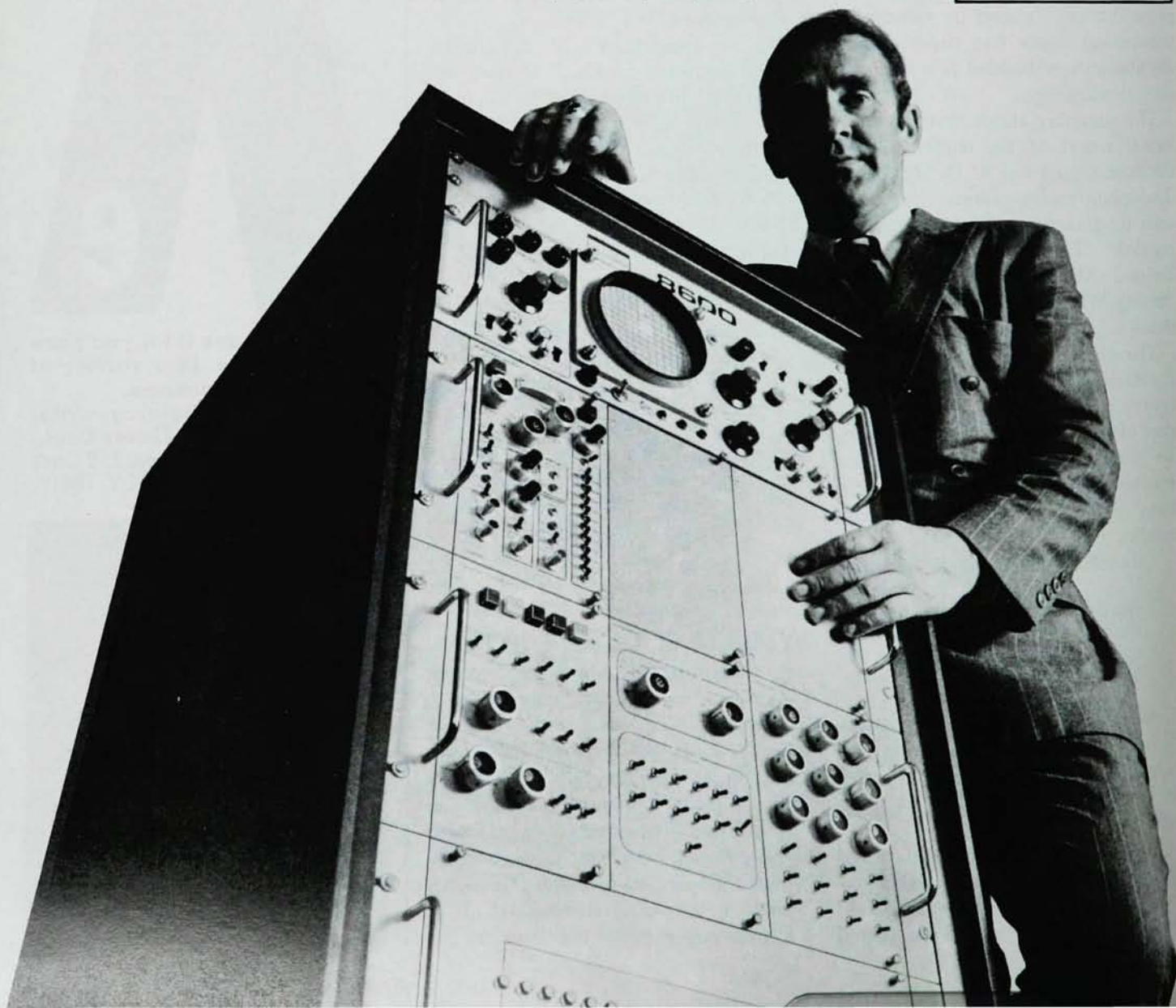
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conference, Smith discussed the idea of twisting and referred to data his group had obtained. They seemed to bear out the validity of his decoupling scheme.

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
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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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