addresses issues raised by the reviewer but, in so doing, also lays down an added justification for publication of the manuscript, thereby adding a new dimension to the original contribution. If however, authors were to issue a rebuttal to defend an already untenable scientific position, as indicated by a "thorough and objective" referee report, publication of the rebuttal (in addition to the manuscript) would seriously undermine their professional standing.

In conclusion, the system proposed by Gordon certainly deserves a chance, but only if the identities of the reviewers are disclosed and only if the authors are given the opportunity to defend their work publicly in writing.

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THE AUTHOR REPLIES: The system of optional published refereeing I originally proposed has the advantage that it could be readily implemented by most journals using existing refereeing procedures. Journals would, of course, be free to adopt any of a number of different variations of optional published refereeing that might better suit their specific needs and those of their readers. Thus, journals desiring a more open an evenly balanced dialogue between referees and authors could adopt the proposal described in detail in the above letter by Kaplesh Kumar. Other journals might wish to encourage a more flexible exchange between authors and referees by letting referees themselves decide whether or not their reports should be published. (Should referees exercise this option, the manuscript could then simply be published without the referee's report).[1] Still other journals might prefer to place additional emphasis on the seriousness of the published refereeing option by requiring a mandatory rethinking period of six months for both referees and authors before the author would be allowed to publish a criticized manuscript.[2]

In practice, the variation of optional published refereeing best suited to a given journal will, of course, depend not only on the special advantages of each variation but also on such practical considerations as the availability of referees and the amount of additional editorial or administrative work that might be required. The essential point, however, which all of the variations of optional published refereeing share in common, is that the author will always have the option and responsibility for publishing the manuscript in question (preferably with the referee's or editor's comments) whenever it became clear that the dialogue between the authors and referees was not likely to resolve an important point of disagreement. It is precisely in such cases, where the ultimate validity of a given manuscript can be determined only by the long-term attention and participation of the interested physics public, that optional published refereeing is most advantageous, since it enables the discussion to be brought into the open in a form that specifically encourages an objective evaluation of the essential points in question. It would also, as noted before, lead to improved refereeing and maunscripts in general. The desirability of such a result for authors, referees, journals and the interested physics public is sufficiently great that it can only be repeated that a system of optional published refereeing, adapted to the specific needs of individual journals, deserves at least a trial period by a number of physics journals.

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1. B. K. Forscher, Science **150,** 319 (1965).
2. C. McCutchen, New Scientist **70,** (no. 998), 225 (1976).

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Call for papers

A special issue of the IEEE Transactions of Electron Devices scheduled for January 1980 will be devoted to *Infrared: Materials, Devices, and Applications.* The purpose of this special issue is to cover the broad state-of-art in the infrared field, which has dramatically changed under the impact of the development of new concepts for large-scale, integrated arrays. Papers are solicitied that cover infrared materials, detectors and related devices, and various systems and applications.

The paper deadline is 1 May 1979. All manuscripts or requests for more information should be directed to the guest editor:

Professor Andrew J. Steckl
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Tokamak instabilities

Your news story on the successful containment of high-temperature ions in the Princeton Large Torus (November, page 17) rang a bell when I read the statement that, at the high temperature achieved in the Princeton tokamak, the collisionless trapped-particle instabilities did not show up. I recall Kadomtsev and Pogutse[1] having predicted that instabilities driven by trapped particles in a tokamak cannot be completely eliminated. But I also recall that Richard Briggs and I, in a cursory analysis[2,3] had concluded that the high dielectric background formed by the untrapped particles could reduce the danger

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letters

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of trapped-particle instabilities considerably. As I have not been actively working in plasma physics for some time, it is very pleasing to read encouraging reports popping up every now and then (for instance, the Alcator at MIT, Mirror at Livermore, and the current PLT experiments at Princeton) in PHYSICS TODAY.

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2. R. J. Briggs, Y. Y. Lau, Phys. Rev. Lett. **28,** 1248 (1972).
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Herzfeld 50 years ahead

It will be appropriate, I hope, to present a brief addendum to the obituary of Karl Herzfeld (January, page 99). The subject of a minimum metallic conductivity has become a very fashionable subject in recent years, and a number of eminent scientists, such as Nevill Mott, have argued about the existence of such a minimum value, and have computed it in various ways.

Just recently (see, for example, the Proceedings of the 19th Scottish Universities Summer School in Physics (Metal Non-Metal Transition in Disordered Systems)—in particular, the chapter by H. Fritzsche—to be published in early 1979), it became better known that Herzfeld had carried out such a calculation in his own way a long time ago (Phys. Rev. **29,** 701 (1927)) and had arrived at an answer quite close to the more recently computed values.

I believe that Herzfeld appreciated the significance of what he had done, and I would venture to guess that not too many others did; a reasonable conclusion might just be that Herzfeld was simply fifty years ahead of his time.

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Immortality of deterrence

In the debate on the nuclear arms race, the central point still is the actual human meaning of nuclear deterrence. Nuclear deterrence certainly has not helped to bring trust and confidence to our international relations, which are the preconditions for a durable peace. The fact that such deterrence might, so far, have prevented some attacks or some wars from spreading does not guarantee that its function will be limited to *mere* deterrence in the future. What could be the consequences of the policy of deterrence if there occurs a political accident, in which an opponent imagines he is attacked, becomes desperate or insane or when he simply attacks as the Nazis? Who of us would then be able to inflict, with his *own* hands, the atrocities that the inevitable consequences of remote-control nuclear war actually imply to innocent victims? Is the issue any different if we make the nuclear weapons and relegate them to computers to drop?

Bernard Feld voiced the consensus of the Pugwash conference at Kyoto in 1975: "Deterrence is basically an immoral concept in that it holds civilian populations hostage to the actions—rational, irrational, or even accidental—of their leaders or, even more frightening, of leaders of nations over which they have no control. Deterrence, whatever its successes in the past, is inherently unstable and unworkable in this increasingly multipolar world."

HERBERT JEHLE
1/12/79 *Charlottesville, Virginia*

Corrections

September, page 17—In the story on the diamagnetic anomaly in copper chloride the Cleveland State University group that collaborated with A. P. Rusakov in 1977 consisted of C. W. Chu and Steinar Huang. S. Z. Huang of the University of Houston, who was mentioned in the story, was not a member of the group.

September, page 26—The names of O. W. Richardson and P. Langevin were transposed in the caption of the photograph identifying the participants of the Seventh Solvay Congress. □