LETTERS TO THE EDITOR

Letters are selected for their expected interest for our readers. Some letters are sent to reviewers for advice; some are accepted or declined by the editor without review. Letters must be brief and may be edited, subject to the author's approval of significant changes. Although some comments on published articles and notes may be appropriate as letters, most such comments are reviewed according to a special procedure and appear, if accepted, in the Notes and Discussions section. (See the "Statement of Editorial Policy" in the January issue.) Running controversies among letter writers will not be published.

AJP EDITORIALS SHOULD STICK TO PHYSICS

I think physicists in influential positions and societies and organizations should stick to physics and avoid public politics. For example, I personally object to the following words from your editorial "The Joy of Physics," in the July 1988 issue of AJP: "As any rational person must do, I worry about the military uses of our subject and about whether some of my students may be driven into weapons work for lack of other job opportunities."

No doubt your opinion here is shared by many physicists. Yet, I am equally sure that many physicists endorse and participate in weapons work. They believe that such work has been and will continue to be necessary for our defense and security. There is certainly room for debate (not preaching by self-appointed peaceniks), but I do not think that AJP is the appropriate forum for this debate. And, if it were, both sides of the issue should be presented, which was not the case in this instance.

Richard Denham
1519 Stanford Street
Santa Monica, CA 90405
28 July 1988

THE JOY OF PHYSICS

This is not really a Letter to the Editor, although you should feel free to treat it as such. It is just a letter to the editor, Robert Romer, from a reader whose eyes are slightly moist after reading the July editorial.

It's wonderful to see my own feelings so articulately and poetically reflected!

Stefan Machlup
Department of Physics
Case Western Reserve University
Cleveland, OH 44106
29 July 1988

THE TERRELL EFFECT

Note by the Editor

While trying to create a title for the following letter, I was reminded of a lecture by Rudolf Mössbauer at a low temperature conference in 1960. Mössbauer spoke about the Mössbauer effect, an effect that he tortuously referred to as "The Recoilless-Resonant-Absorption-of-Gamma-Rays Effect." (*There* is a fine example of a "freight-train adjective!") Nicholas Kurti, who followed Mössbauer on the program, observed that Nernst used to refer to the third law of thermodynamics simply as "My Heat Theorem." With those thoughts in mind, I persuaded the author to accept my proposed title for his letter. Let us follow Professor Sheldon by calling Terrell's 1957 discovery simply the "Terrell effect," honoring the physicist who discovered this intriguing result when so many of us who had the opportunity had overlooked it. Are we not fortunate to have a subject in which such a simple yet interesting effect can lie undiscovered for 50 years, an effect that could have been discovered by any undergraduate physics major willing to engage in clear thinking about special relativity? In making that remark, I do not intend in any way to lessen my appreciation of Terrell's contribution; on the contrary, he deserves credit for noticing what had gone unnoticed for so long. The belated discovery of the Terrell effect gives me hope that other similarly interesting discoveries are there for the taking, in special relativity or elsewhere in the undergraduate curriculum, waiting to be found by our students or even by their teachers.

The recent letter by Erich Sheldon,[1] "The Twists and Turns of the Terrell Effect," indicates that this subject (which I called the invisibility of the Lorentz contraction) is still somewhat controversial. Although the literature on this effect has grown to considerable proportions, the effect is still not mentioned in a number of textbooks, and is sometimes inaccurately presented or overlooked [as in Jacob Bronowski's otherwise admirable television series, "The Ascent of Man" (1973)]. Some aspects of this discovery still need clarification, it appears.

Asher Peres should not have been criticized for stating correctly in his excellent review article[2] that "if a snapshot is taken of a moving object, the latter does *not* appear contracted, but rather *rotated*." This is essentially what I originally stated in 1959,[3,4] and what Weisskopf stated in 1960.[5] A snapshot is commonly understood to be a two-dimensional, nonstereoscopic photograph, but Sheldon's criticism involves the *three-dimensional* appearance of the moving object, not a snapshot. As Sheldon (correctly) points out,[1] for a relativistically moving object the stereoscopic appearance would involve shearing and other distortions, in addition to the apparent rotation. These more complicated aspects of stereoscopic vision were, in fact, pointed out by me in my original paper,[4] as well as by others.[6]

Some of the numerous papers[7] and some of the criticism[8] concerned my assumption of a small subtended visual solid angle, within which a conformal transformation preserves angles and shapes. If a finite solid angle is considered, some distortions appear, due to the varying angle of rotation (aberration angle), as I had also pointed out[4] in 1959. Thus some, like Sheldon,[1] have considered the idea of an apparent rotation to be misleading. An interesting effect that emphasizes the rotation aspect is that a highly relativistic object will be seen approaching the observer tail-end first. At one meeting[9] I showed a slide illustrating this—a relativistic spaceship coming toward the observer, with little but the rear end visible. (See Fig. 1.)

Another peculiar aspect of relativistic appearances—the compression of celestial star fields into the forward sector of vision for a fast-moving

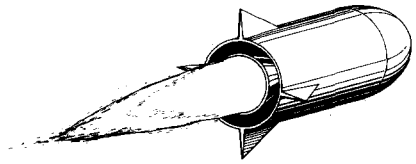

Fig. 1. Relativistic rocketship approaching observer; seen at 150° from flight direction. Here, $v/c = 0.98974$; $\gamma = 7$; $M = 1$ (no Doppler shift); $\theta = 150°$; and $\theta' = 30°$.

spaceship—has been discussed by many authors, such as Oliver,[10] Moskowitz,[11] and Scott and van Driel.[12] Surprisingly, none of these authors seem to have noticed that this effect was discussed earlier by me in a 1960 paper,[13] which was based on my widely circulated but unpublished paper of 1957 on "The Clock Paradox."[14]

This paper was written at the time of a running controversy concerning the clock paradox, or twin paradox, and the views of Herbert Dingle, largely in *Nature*. I sent or gave copies of my paper,[14] which included but perhaps did not sufficiently emphasize my discovery of the "invisibility," to a number of prominent relativists. Some (including Dingle) thought it wrong, and some thought it not too important. Some, such as George Gamow and Sir Charles Darwin, liked it; Darwin said that he also had tried to convince Dingle of the error of his ways, "but he lost himself in a cloud of algebra."[15] My paper was rejected, in any case, by several eminent journals (including *Nature*) in 1957 and later, which eventually led to some awkward questions of priority as to the invisibility of the Lorentz contraction.

The delay in achieving publication of my result[3,4] became a problem in 1959, when Penrose also published his somewhat more restricted result concerning circular outlines.[16] However, the whole matter might have been overlooked if Weisskopf had not published his *Physics Today* article[5] in 1960, in which he generously called attention to my 1959 paper.[4] Gamow also helped to publicize it.[17]

Since my paper still seems in danger of being overlooked, or at least not read, I appreciate Sheldon's calling attention to it in his recent letter.[1] I should also mention that my 1960 paper[13] was reprinted by the American Institute of Physics.[18]

James Terrell
Mail Stop D436
Los Alamos National Laboratory
Los Alamos, New Mexico 87545
16 July 1988

[1] E. Sheldon, Am. J. Phys. **56**, 199–200 (1988).
[2] A. Peres, Am. J. Phys. **55**, 516–519 (1987).
[3] J. Terrell, Bull. Am. Phys. Soc. **4**, 294 (1959).
[4] J. Terrell, Phys. Rev. **116**, 1041–1045 (1959).
[5] V. F. Weisskopf, Phys. Today **13**(9), 24–27 (1960).
[6] For example, N. C. McGill, Contemp. Phys. **9**, 33–48 (1968).
[7] For example, M. L. Boas, Am. J. Phys. **29**, 283–286 (1961).
[8] For example, P. M. Mathews and M. Lakshmanan, Nuovo Cimento B **12**, 168–181 (1972).
[9] J. Terrell, Bull. Am. Phys. Soc. **5**, 272 (1960).
[10] B. M. Oliver, IEEE Spectrum **1**, 86–92 (1964).
[11] S. Moskowitz, Sky Telescope **33**(5), 290–294 (1967).
[12] G. D. Scott and H. J. van Driel, Am. J. Phys. **38**, 971–977 (1970).
[13] J. Terrell, Nuovo Cimento **16**, 457–468 (1960).
[14] J. Terrell, "The Clock Paradox," Los Alamos Document LADC-2842 (1957).
[15] C. G. Darwin, private communication (January 1958).
[16] R. Penrose, Proc. Camb. Philos. Soc. **55**, 137–139 (1959).
[17] G. Gamow, *Biography of Physics* (Harper and Brothers, New York, 1961), pp. 180–181.
[18] *Special Relativity Theory: Selected Reprints* (American Institute of Physics, New York, 1963).

PHYSICS: A GRADUALIST SCIENCE?

I was surprised and pleased to encounter in the August issue [Am. J. Phys. **56**, 765 (1988)] a review of my book *Heretical Verities: Mathematical Themes in Physical Description*...surprised because reviews of the products of small, obscure presses are comparatively rare and pleased because of the exceptionally fair-minded and well-balanced judgments expressed by the reviewer, Jeff Nicoll. Although my book contains a number of rather jaundiced assertions about the current state of fundamental physics (and mathematics)—and by implication the intellectual standards that have produced it—Dr. Nicoll's review serves as a heartening reminder that the divorce of science and scholarship has not yet been legally finalized.

On only one point would I offer a rejoinder. This concerns the reviewer's reference to "...the metaphysical principles Phipps employs. He feels that physics should be a gradualist science, incrementally increasing its accuracy and understanding rather than looking for changes of perspective..." This shocked me when I first saw it. I had not spoken of either gradualism or incrementation...yet here was an obviously attentive reader's paraphrase of what he thought had been said. I looked again at what I had written and, sure enough, it could be read in that light. The metaphysical principles I stated were: (1) Science progresses by successive approximations (with the corollary that one has to get the lower-order approximations right before proceeding to the higher-order ones). (2) Seek invariant forms (as distinguished from covariant ones). (3) Seek (invariant) covering theories in order to correct any deficiencies of parametrization.

It is evidently from the first of these precepts that the reviewer primarily derived his conclusion that I favored gradualism in science. But I must hasten to point out a distinction between successive approximations as a method and gradualism as a truth-seeking strategy. I stated (p. 142) that "The method of successive approximations entails viewing each order of approximation as an 'independent physics,' answerable to its own obligations, e.g., of form invariance and adequacy of parametrization." About successive approximations I pointed out (p. 2) that "such a principle is of no active help to the scientist, but rather defines the limitations of science." In other words, gradualism is of no use for finding the all-important *first approximation* on which everything else rests.

The other two principles may prove to be of some help in that regard, but gradualism or incrementalism (e.g., the tilting of Ptolemaic epicycles out of the plane of the main circle, the piling-on of fields and "coupling constants," etc.) is not only the least imaginative but generally the least productive of strategies. The main thing needed, I feel, is an historically guided and disciplined intuition for "what works." It is to the guiding and disciplining that great teaching makes its contribution. But I did not discuss that in the book and would not presume to offer any strategy for discovery of what is generally attributed to the inspirations of "genius."

In elementary particle physics today, and perhaps even in less exotic areas such as superconductivity, the informed amateur or outsider (as distinguished from the committed expert) perceives at once that the right first approximation to the physics is missing. In such a fix, gradualism can accomplish nothing but prolonging the agony of a science. It appalls me