tance of an idea is in part a function of how effectively it is pushed by its originator. Nineteen twenty-one was not a good year to be a quantum theorist in America, and Lunn was not willing to fight enough to get his idea out. Of course, the history of Aristarchus and Copernicus made such points clear long before the new schools of science criticism latched on to them.

Meanwhile, we would like to hear from any PHYSICS TODAY readers who know of or have access to any confirming documents or recollections concerning Lunn's contribution.

SAMUEL I. WEISSMAN
Washington University
St. Louis, Missouri
MICHAEL WEISSMAN
(mbw@uiuc.edu)
University of Illinois at Urbana-Champaign

Exception Taken by Quotee in Piece about Sokal Affair

In his interesting "Reflections on the Sokal Affair: What Is at Stake?" in your March issue (page 73), Sam Schweber quotes a 1995 talk in which I said that "the product of our work [as scientists] is a worldview that has led to the end of burning witches . . . or at least to an understanding that we are not living in a world with a nymph in every brook and a dryad in every tree." And he remarks that "This statement, of course, belies the extreme dichotomy [between science and culture] that he [Weinberg] expounded in his *New York Review of Books* essay."

But I haven't been inconsistent. In that essay, I said that "I think that, with two large exceptions, the results of research in physics (as opposed, say, to psychology) have no legitimate implications whatever for culture or politics or philosophy" (*New York Review of Books,* 8 August 1996). Then I went on to explain that one of the two exceptions was "the profound cultural effect of the discovery, going back to the work of Newton, that nature is strictly governed by impersonal mathematical laws." This is precisely what I was talking about in the passage quoted by Schweber.

STEVEN WEINBERG
(weinberg@physics.utexas.ed)
University of Texas at Austin

Schwinger Credited with Finding Anomaly, Exploring Cold Fusion

I have read with interest Stephen Adler's letter on the history of his discovery of the axial vector anomaly (PHYSICS TODAY, March, page 106), as well as his earlier account in *Current Contents*.[1] I have also heard and read many of Roman Jackiw's accounts of the history of this anomaly (for example, see PHYSICS TODAY, February 1996, page 28). Although these gentlemen know the history perfectly well, it seems to me that their brief summaries may mislead younger readers as to the true discoverer of the axial vector anomaly in its original context, the decay of the neutral pion into two photons.

It was Julian Schwinger who, very explicitly in his classic paper "On Gauge Invariance and Vacuum Polarization,"[2] derived the anomaly by showing that pseudoscalar and pseudovector couplings are equivalent. Of course, the language used was somewhat different in those days.

This result apparently had been completely forgotten by the time Adler and Bell and Jackiw did their work, but very shortly thereafter, Jackiw and Johnson recognized that "the first derivation of [the anomaly equation] for external electromagnetic fields was given by Schwinger."[3] (Indeed, in a "Note Added in Proof" to his 1969 paper, Adler acknowledged Jackiw and Johnson's rediscovery of Schwinger's work.)

These remarks are not at all meant to disparage the significant contributions made by many people in 1968 and subsequently, but merely to remind us all in physics of what a great debt we owe to Julian Schwinger.

References

1. S. Adler, Current Contents **22** (31), 18 (1982).
2. J. Schwinger, Phys. Rev. **82**, 664 (1951).
3. R. Jackiw, K. Johnson, Phys. Rev. **182**, 1459 (1969).

KIMBALL A. MILTON
(milton@mail.nhn.ou.edu)
University of Oklahoma
Norman, Oklahoma

I would like to endorse Scott Chubb's tribute—published in your "Letters" last September (page 15)—to the outstanding achievements of Julian Schwinger that led to a Nobel Prize.

However, there is considerable interest in the last years of Schwinger's life, when, much to the surprise of his colleagues, he became a true believer in cold fusion. Chubb believes that Schwinger was correct. Having followed the subject closely,[1] and attended all six of the International Conferences on Cold Fusion (ICCF), I feel that some balancing comments may be useful.

Schwinger made two major contributions to cold fusion. First, he wrote[2] that "this cold fusion process [of Martin Fleischman and Stanley Pons's] is not powered by a DD reaction, rather it is an HD reaction, which feeds on the small contamination of D_2O by H_2O"—using the well-known fact that the HD reaction rate is several orders of magnitude greater than the DD rate, which is much more frequent than the HH rate. He explained that since the HD reaction is $p + d \rightarrow {}^3He + \gamma$, there are no neutrons, and that would explain their experimental paucity in cold fusion experiments.

At ICCF-4, I suggested[3] that cold fusion experimenters should believe Schwinger and test his ideas and find optimum conditions by varying the ratio of D_2O to H_2O from 1 to 99%, 25 to 75%, 50 to 50%, 75 to 25% and 99 to 1%. But surprisingly, no one has followed Schwinger's advice even though it is based on well-known rates. On the contrary, several experiments claim to have observed cold fusion with the HH reaction—which was used by Fleischman and Pons as a control giving no fusion. Thus, the experimental claims for cold fusion are in contradiction to the hierarchy of rates of HD being very much higher than DD, which is very much higher than HH.

Schwinger's second major contribution[4] was to explain at ICCF-1 that the mega-electron-volt gamma ray produced would not be observed because its energy would be shared by some 10^7 phonons, each of about 0.1 eV. He assumed that the lattice of the cathode (for example, palladium) would move coherently and thus absorb the energy. The basic problem with this idea is the differing times for the process to occur—the fusion reaction takes place in less than 10^{-20} seconds while the time for the energy to spread among 10^7 nuclei of the lattice is greater than 10^{-15} seconds. Thus, Schwinger's hypothesis of the gamma ray being dispersed widely over the lattice is unworkable by many orders of magnitude. Detailed theoretical criticisms were made by Mario Rabinowitz *et al.* at ICCF-4, where they demolished theoretical models of cold fusion even though they said the task was "like shooting at a moving target."

A physicist should try to prove himself wrong and should consider criticism. That was a problem with Schwinger after 1989. At the end of his talk at ICCF-1, several of us tried to discuss his predictions with him, but his answers were so contradictory that one doubted if baryon number conservation was respected.

Subsequently, during visits to the University of California, Los Angeles, I tried to contact him, but was told that he was virtually unseen on campus. When he was phoned at home, a charming lady explained it was not possible just then, and what can you do when such a person says, "He had a special glint in his eye this morning and I am sure he has a new idea, so I could not possibly disturb him"? Letters remained unanswered.

Even so, for his main work, outside of cold fusion, Julian Schwinger will remain a historical figure of science forever.

References

1. D. R. O. Morrison, Nature **382**, 572 (1996).
2. J. Schwinger, Z. Naturforsch. A **54**, 756 (1990).
3. D. R. O. Morrison, Trans. Fusion Technol. **26**, 48 (1994) (proceedings of ICCF-4).
4. J. Schwinger, in *Proc. of the First Cold Fusion Conf.*, National Cold Fusion Institute, Salt Lake City (1990), p. 130.
5. M. Rabinowitz, Y. E. Kim, V. A. Chechin, V. A. Tsarev, Trans. Fusion Technol. **26**, 3 (1994) (proceedings of ICCF-4).

Douglas R. O. Morrison
(drom@vxcern.cern.ch)
Coppet, Switzerland

South American Site of Auger Project Is Still Up in the Air

Although informative, the news note on the Pierre Auger Project in the "Search and Discovery" section of your February issue contains an error. It states that "The Southern Hemisphere site [is] in Argentina's Mendoza province." Although those of us involved in the project in our center's Group for Exact Sciences, including two astronomers, one physicist and one statistician, would naturally prefer that the site be located in our province, at the moment there are also two other candidate sites under consideration. One is located farther north, on the border between the provinces of La Rioja and Catamarca, and the other lies farther south, in the province of Rio Negro.

Only after a detailed astronomical and meteorological study has been made of all three sites will the final decision be taken as to where to locate the detectors.

Richard Branham
(cricyt@planet.losandes.com.ar)
Regional Center for Scientific and Technological Research
Mendoza, Argentina

Book Defended, Artful Solution Proposed as Way Out of Color Bind

In reviewing my book *Empire of Light* (PHYSICS TODAY, March, page 84), Pierre Meystre dwells negatively on the fact that it does not show the works of art I introduce as I discuss scientific and artistic views of light. He should direct his comments to the publisher, Henry Holt. As Meystre must surely know, color reproductions drive costs up, and financial decisions beyond the control of the author often determine the look of a book.

I do not understand why Meystre finds the lack of reproductions so damning, given that other reviewers have been untroubled by this issue and have praised the book's accessibility and graceful weaving together of science and aesthetics. Still, I welcome his suggestion that a future edition could benefit from the inclusion of reproductions. I invite him to join me in persuading my publisher to produce an especially handsome, as well as a highly readable, second edition (now that the first printing is already virtually sold out).

Sidney Perkowitz
(physp@emory.edu)
Emory University
Atlanta, Georgia

Corrections

April, page 96, and March, page 107—Stephen L. Adler's e-mail address should have been given as adler@ias.edu.

April, page 90—The contact e-mail address for the 7th International Conference on Ion Sources (ICIS97), set for 7–13 September 1997, should have been given as icis97@lns.infn.it.

February, page 47—The professional affiliations given for David J. Nesbitt, corecipient of the Earl K. Plyler Prize, should have read as follows: JILA, the National Institute of Standards and Technology and the University of Colorado at Boulder. ■