they never reach criticality at all. (It's useful to recall that a nuclear reactor operates in steady state right at the condition of criticality, thereby maintaining a constant neutron flux for energy production without running away to explosion.)

A Jason panel's 1995 nuclear testing study that provided the technical basis for the US decision to seek a true zero-yield CTBT concluded that subcritical experiments "are useful for improving our understanding of the behavior of weapons materials under relevant physical conditions. They should be included among treaty-consistent activities. . . ." For example, one of the first two proposed subcritical experiments will use high-explosive-driven flyer plates to generate planar-shock waves incident on small samples of plutonium to obtain data over a range of high pressures as input for a more accurate equation of state determination.

Our above-stated conclusion in the Jason study (which I chaired) is directly contrary to what was misreported in your December 1996 story. We also concluded (as correctly reported in the March 1997 article) that a strong science-based stockpile stewardship and management program was required under a CTBT, but that "underground testing of nuclear weapons of any yield level below that required to initiate boosting is of limited value to the United States." By that, we meant all low-yield tests that exceed criticality.

The Jason panel's conclusion in support of a zero-yield CTBT was in fact adopted by the Clinton Administration and is the official US position. Physicists should have a clear and accurate understanding of both this position and the terms explained above so that they can contribute to enlightened public debate about ratification of the CTBT, which is a true zero-yield CTBT that marks a major step forward in the worldwide effort to reduce nuclear danger.

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Alan Sokal's Hoax and A. Lunn's Theory of Quantum Mechanics

In his excellent response—"Was Sokal's Hoax Justified?"—to Paul Forman and others who argue that the content of science is not so much determined by the nature of truth as by the social environment (PHYSICS TODAY, January, page 60), Kurt Gottfried points out that the crisis leading to the development of quantum mechanics was not confined to Teutons depressed about the outcome of the Great War. A little-known midwestern side story in the development of quantum mechanics dramatizes Gottfried's point.

The problems with early attempts at quantum formulations were very apparent to Arthur C. Lunn, a mathematical physicist at the University of Chicago. Lunn frequently described the Bohr quantum picture to his students as "an obscene theory—they pull down the curtain just when it gets to the good part."

Lunn developed his own theory, and he wrote a paper that he submitted to *Physical Review* in 1921. So far, we have been unable to unearth a copy of the paper, but one of us (SIW) did see the original in about 1930, when he was a student of Lunn's, and read the highly memorable beginning—but, alas, only skimmed the remainder. He clearly recollects that Lunn started by extending $E = hf$ to a complete relativistic four-vector with $p = h/\lambda$—that is, the theory of De Broglie waves. Given that Lunn had been pointing out since 1919 that "the origin of the Zeeman effect will be found in the Abelian property of the magnetic field group" (as SIW remembers Lunn quoting from his 1919 class notes), Lunn's theory obviously involved states that formed a vector space, with physical variables corresponding to linear operators. At any rate, as Lunn later told his students, Erwin Schrödinger informed him (on a visit in about 1927) that Lunn had done the same work that he had done.

The *Physical Review* referee, G. S. Fulcher, found Lunn's paper to be unphysical and impossibly abstract, and he rejected it. Fulcher replaced Lunn as a member of *Physical Review*'s editorial board early in 1922, and Lunn went on to withdraw bitterly from contact with most physicists.

Lunn's work disconfirms the idea that the specific content of quantum mechanics depends in any detailed way on what the social situation was in Weimar Germany. (Of course even without Lunn, perusal of the names Bohr, De Broglie, Einstein, Dirac, Bose, Fermi, Landau etc. might suggest the same conclusion.)

The Lunn story does confirm some social constructionist points, such as the claim that not all communities of scientists are equally prepared to accept some new idea, and that accep-

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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.

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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.

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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

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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 him-