was founded by Benjamin Franklin, modeled on the Royal Society and dedicated to promoting useful knowledge. He called becoming its president in 1797 his life's "most flattering incident," though he took office the next day as vice president of the US. And he stayed on as the society's president through both his terms as US president and for five years into his retirement.

Though wary of government expansion, Jefferson conceived of the Federal support of science. His report on distilling freshwater from saltwater may have been the first scientific paper published under government auspices. Historian Silvio Bedini calls him "the father of the Bureau of Standards," and attributes to his influence the establishment of early scientific agencies such as the weather bureau. Jefferson planned and organized the Lewis and Clark expedition to, in his words, "extend . . . the boundaries of science." Small wonder that Harvard University physicist and historian of science Gerald Holton has declared that more than any other high public official of any era, Jefferson dramatized and promoted the sciences for human progress.

Holton has compared the Lewis and Clark venture to "a research program by which science serves both the search for truth and the interest of society." Jefferson Lab serves that same dual purpose. With our users, we serve the purely scientific purpose of investigating the quark structure of nuclei. With industry and the Navy, we also serve the interest of society in developing versatile, high-average-power free-electron lasers based on our superconducting RF accelerator technology.

Historian John C. Greene has heralded Thomas Jefferson as "a symbol of American respect for science and faith in its power to promote human progress." The founding of CEBAF reflected the value the nation places on that respect and faith. Now, the nation has given our lab precisely the right name. We intend to live up to it.

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US Position on Test Ban Treaty Explained, Key Terms Clarified

The comprehensive test ban treaty (CTBT), signed at the United Nations in New York on 24 September 1996 by President Clinton for the US and subsequently acquiring more than 140 other signatories, bans all nuclear explosions—of any size, at any time and in any place. It is referred to as a true zero-yield CTBT.

During the coming months, leading up to the treaty ratification process, there will be extensive debate in this country as to what are the "permitted activities" under the CTBT that Los Alamos, Lawrence Livermore and Sandia National Laboratories will rely on to ensure the continuing safety and reliability of our enduring nuclear stockpile as it ages and shrinks in the years ahead.

I am writing this letter to correct serious inaccuracies and ambiguities on this subject that occurred in PHYSICS TODAY news stories in December 1996 (page 37) and March 1997 (page 63). The problems concern the meaning of the term "subcritical experiments" with reference to activities that the US plans to continue at the Nevada Test Site, and whether these activities would be consistent with the CTBT.

"Subcritical experiments" are very different from the low-yield underground nuclear tests with which they have been confused in the two PHYSICS TODAY stories. In a low-yield test explosion, the nuclear device is generally assembled with less fissile material and/or altered detailed features from a stockpile warhead to ensure that the fission chain reaction will terminate before the release of nuclear energy exceeds a deliberately preset limit. A device is said to have exceeded criticality when a fission chain reaction is initiated and produces exponential multiplication of neutrons. When the total nuclear energy release does not exceed the energy release from 4 pounds of TNT, the test is commonly referred to as a hydronuclear test; such explosions were carried out by the US in 1959–61 during the nuclear testing moratorium with the USSR and the UK.

In the US prior to the 1996 signing of the CTBT, a number of senior administration leaders in defense and national security affairs advocated that "permitted activities" include underground nuclear testing at yields of up to approximately 500 to 1000 tons of TNT. Such testing would allow studies to be undertaken of boost gas initiation and initial burn, which represent a critical step in achieving full primary design yield for igniting the secondary, or main stage, of a modern thermonuclear warhead.

In contrast to these activities, subcritical experiments involve such limited quantities of plutonium-239 that 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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