- The CTBT "permits experiments . . . including those of the explosive nature, but under the condition that they are purely chemical (the so-called 'hydrodynamic experiments')." —Grigory Berdennikov, chief Russian negotiator, 7 December 2005.
- "My government's position [is] that the CTBT should not permit any nuclear weapon test explosion involving any release of nuclear energy, no matter how small." —John Weston, UK ambassador to the United Nations, 14 September 1995.
- "It maintains the possibility of testing called 'cold' tests and 'subcritical', no nuclear chain reaction." —Serge Vinçon, former vice president of the French Senate, 25 March 1998.

When the CTBT was submitted to Congress in 1997, the State Department included an article-by-article analysis with the following statement:

> The U.S. decided at the outset of negotiations that it was unnecessary, and probably would be problematic, to seek to include a definition in the Treaty text of a "nuclear weapon test explosion or any other nuclear explosion." . . . It is clearly understood by all negotiating parties, as a result of President Clinton's announcement on August 11, 1995, that the U.S. will continue to conduct a range of nuclear weapon-related activities to ensure the safety and reliability of its nuclear weapons stockpile, some of which . . . may result in the release of nuclear energy. Such activities . . . could include: . . . inertial confinement fusion . . . and hydrodynamic experiments, including subcritical experiments involving fissile material. None of these activities will constitute a nuclear explosion.

I don't believe that neutron-irradiated subcritical experiments were discussed in the negotiations. Since they involve nuclear chain reactions and are not purely chemical, they fall between cold, allowed subcritical tests and the forbidden, barely supercritical hydronuclear tests. They allow the US to accomplish, with its billion-dollar-plus setup, what Russia or China could accomplish much more cheaply with hydronuclear tests.

Recently, after the DIA accused Russia of cheating, the State Department offered the following explanation:

> Dating back to 1993, the United States has defined its own nuclear testing moratorium as a commitment not to conduct "nuclear explosive tests", and after August 1995 made clear that this means any test that produces a self-sustaining, supercritical chain reaction of any kind. This is what the United States refers to as the "zero-yield" standard. Beginning with President Clinton's announcement in August 1995, the United States led efforts to ensure the Comprehensive Test Ban Treaty (CTBT) was a "zero-yield" treaty, but these efforts did not produce a documented agreement among the nuclear weapons states on a definition of "nuclear explosion."[6]

Although the CTBT established the now-operational global nuclear-test-monitoring network, the treaty-mandated option for short-notice, on-site inspections of test sites is not yet available because the treaty has not formally entered into force.

To help clarify whether their ongoing experimental activities are fully compliant with the zero-yield ban on testing, nuclear-weapons-state signatories to the CTBT could pursue test-site transparency procedures. One approach would allow measurements of induced radioactivity or neutron transmutation products in the containment vessels to determine the fission yield after subcritical tests.

Unfortunately, Russia refuses access to its Novaya Zemlya test site until the US ratifies the CTBT, as Russia has. In the case of China, Los Alamos physicists visited the Chinese Lop Nor test site 10 times[7] between 1990 and 2001. The visits were to be reciprocated, but when the Chinese delegation arrived in the US, their permission to visit the US test site was vetoed in the Department of Energy, and they were given briefings at Livermore instead. Since then, the Chinese have not been interested in transparency unless the US goes first. As Kramer's report illustrates, the US weapons labs have been open in publishing photos and decriptions of their subcritical-experiment setups. However, the Trump administration is not encouraging them to discuss test-site transparency.

All 184 signatories to the CTBT would do well to support additional test-site transparency measures to reinforce the quarter-century moratorium on nuclear testing by the nuclear-weapons-state signatories.

References

1. Defense Intelligence Agency, "DIA statement on Lt. Gen. Ashley's remarks at Hudson Institute" (13 June 2019).
2. US State Department, *Adherence to and Compliance with Arms Control, Nonproliferation, and Disarmament Agreements and Commitments* (August 2019), p. 40.
3. P. Sonne, "U.S. military intelligence steps up accusation against Russia over nuclear testing," *Washington Post*, 13 June 2019.
4. D. J. Funk, "Enhanced Capabilities for Subcritical Experiments (ECSE): Portfolio Overview," rep. no. LA-UR-18-28253, Los Alamos National Laboratory, PowerPoint presentation (2019), slide 48.
5. US Department of State, "Key P-5 Public Statements on CTBT Scope," 26 January 2011.
6. Ref. 2, p. 39.
7. T. C. Reed, D. B. Stillman, *The Nuclear Express: A Political History of the Bomb and Its Proliferation*, Zenith Press (2009), chap. 14 and app. E.

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LETTERS

A nod to Philip Bevington

I read with appreciation Charles Day's important, lucid, and interesting editorial on reproducibility (Physics Today, December 2019, page 8). When he mentioned the need for better education in data analysis and named Philip Bevington's *Data Reduction and Error Analysis for the Physical Sciences* and its importance to him, I gasped audibly. For the first time, I appreciated how much that one small, clear tome had influenced not only my career but my whole approach to life and decision making in nontechnical areas.

For me, Bevington, as the book was affectionately known in my undergrad physics department, encapsulates the essence of the scientific method. I know such an ideal is infeasible on so many levels, but I believe that the world would be a much better place if everyone, not just every physicist, were to read the book as part of an undergraduate education.

We might want to revisit Bevington as an exemplar not just of scientific-method education but of meta-instruction in effective pedagogy in general. Phil Bevington seems to have struck the perfect balance of detail, rigor, practicality, and clarity.

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Neckties or not, and a quick fix

I appreciate Brian Kraus's review of my book *Real Scientists Don't Wear Ties: When Science Meets Culture* (PHYSICS TODAY, March 2020, page 52). The photo spread of diverse physicists mostly not wearing ties underlines a point I made in the book: More than ever, we physicists look different from each other and dress the way we want to. However, I must correct an error Kraus made when he wrote that I retired from academia in 1990. As I stated in my book's introduction (page xi), I continued academic research and teaching until 2011, when I retired as Charles Howard Candler Emeritus Professor of Physics after 42 years at Emory University.

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A physicist out of academia

The commentary from Elizabeth Frank (PHYSICS TODAY, October 2019, page 10) about her career move from academia to industry resonated with me in many ways . I largely agree with Frank's statement that "you don't have to justify your motivations to anyone but yourself," but I also point out that career changes can deeply affect one's family.

As Frank mentions, physics professors often have limited awareness of opportunities outside academia; therefore, input from someone with a career like mine is important. My path is significantly different from Frank's, and I speak from later in life. Furthermore, the variety of careers available to physicists is much greater outside academia than within.

My experiences in academic physics began when I was a precocious elementary school student and grew through wonderful experiences at Harvard, Princeton, and Stanford Universities to a tenure-track assistant professor position at the University of Massachusetts Amherst. At Harvard, I had strong physics courses and valuable interaction with renowned physicists, including my adviser Norman Ramsey. At Princeton, I completed a PhD in experimental high-energy physics in less than four years.

During my postdoc at Stanford in the early 1970s, I had the good fortune to work at SLAC on two experiments that led to Nobel Prizes—one for Burton Richter and one for Martin Perl. I then took a tenure-track assistant professorship at UMass, where I continued the work at SLAC while helping a local UMass team start an experiment at Brookhaven National Laboratory. All was going well.

Nonetheless, at age 30 I had a midlife crisis and decided to move from academia to industry. To Frank's point, the switch was deeply personal. At least three factors contributed to my malaise. First, I felt locked into my high-energy-physics specialty and was concerned about its future. The significant projects were getting much bigger, taking much longer, based in more distant laboratories, and producing increasingly arcane results. It seemed harder all the time for me to continue to derive personal satisfaction from the field.

A second factor was that as an experimenter, I felt that I should understand the theories pertaining to my experiments, but they had reached a level of abstraction beyond my comprehension. Third, I was concerned about the 1970s energy crisis associated with the Arab oil embargo, and I wanted to help address it. I landed a job at the GE Research and Development Center in Schenectady, New York. My wife was shocked but supportive: Our two children were young and portable, and her career actually benefited from the move.

My experiences in industry during 37 years with GE were also wonderful, but in different ways from my time in academic high-energy physics. My work at GE was mostly in lighting technology. After a decade I moved to the headquarters campus of GE Lighting in Cleveland, Ohio. I have done research that has been published in refereed journals. It has always been on problems with near-term, real-world significance. I have also worked on developing and producing new energy-saving lighting products.

Every few years my role morphed as the business changed and new needs arose. Those changes were invigorating as I gained new insights. I often started new assignments with little of the requisite technical know-how, but I was a quick learner with a background in basic physics and wonderful, talented, and technically diverse coworkers. The business funded the work, with no external grant proposals needed. I have had plenty of opportunity to teach, and I have enjoyed it. Toward the end of my career at GE, I was involved in the LED technology revolution. Since retiring, I have developed a successful consulting business that draws heavily on technical knowledge of light sources.

I look back to my decision at age 30 as the most important one of my career. On the one hand, academia *might* have provided better opportunity for long-term career focus and development of deep expertise. On the other hand, industry *did* provide stimulating work with near-term, beneficial, real-world significance. It also offered invigorating career and assignment changes. I have never regretted my decision, but I will never know where the path not taken might have led.

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