

Edward Bullard and Patrick Blackett. Both went to the Cavendish Laboratory, but that is about all they have in common.

Harold Jeffreys and Maurice Ewing, two equally prominent geophysicists who were contemporaries of Bullard and Blackett and are also mentioned in Good's article, were not Cavendish people. The major problem they faced was how to reconstruct the structure and inner processes of Earth from surface data only. They may have recognized that the problem was ill-posed and that it could only be solved numerically. But their approaches were different: Bullard and Ewing used controlled explosive sources to constrain the solution, while Jeffreys used Bayesian statistics. Their views had much to do with their approaches. Jeffreys, in the fifth edition of his treatise *The Earth: Its Origin, History, and Physical Constitution* (Cambridge University Press, 1970), adopted my viscoelastic Earth model and thus unwittingly opened the door to plate tectonics.

Incidentally, Good claims that Jeffreys was "a fellow of Trinity College." He was not. He became a fellow of St. John's in 1914 and held successive fellowships thereafter under different titles, always at St. John's. I was his guest at St. John's College in the late 1950s.

Cinna Lomnitz

(cinna@prodigy.net.mx)

National Autonomous University of Mexico
Mexico City

Good replies: David Stevenson's letter raises an interesting issue that has more to do with the nature of history than with Earth's internal heat. As Stevenson notes, from our current vantage point we know that ignorance of radioactivity was not the main problem with Lord Kelvin's calculation of Earth's age. And the belief of Kelvin's peers that this invalidated his calculations "undoubtedly influenced the development of geophysics," as Stevenson says. What counts in history is what people thought at the time. Although "could haves" interest me, too, we historians usually struggle sufficiently just establishing what did happen.

I hope I did not suggest that Teddy Bullard was the first to think Earth's magnetic field might be due to electrical currents deep within. In fact, wonderful letters in which Bullard, Patrick Blackett, and Walter Elsasser debate details of such currents still exist in the Bullard archive. And even before Joseph Larmor's 1919 paper on stellar interiors, Arthur Schuster had also considered

those currents in several papers. Michael Rochester's comments are much appreciated, since he knew and worked with Bullard.

As Nick Rogers points out, Bullard came from a wealthy family and experienced opportunities less available to his working-class peers. Social background is always relevant to biography. I thank Cinna Lomnitz for the welcome correction of my error regarding Harold Jeffreys. I did know that Jeffreys was a fellow of St. John's College; I spent several enjoyable weeks in the school's archives reading his manuscripts.

Bullard and Blackett actually had more in common than their interest in geophysics. Both were government advisers and department directors, both involved themselves in operational research and the governance of science. Their personalities, of course, could not have been more divergent.

Gregory Good

(ggood@aip.org)

American Institute of Physics
College Park, Maryland

Roots and risks of total nuclear disarmament

Notably absent from Sid Drell's otherwise comprehensive review of nuclear reductions (PHYSICS TODAY, July 2010, page 30) is one of the most efficacious and irreversible nuclear-disarmament measures—demilitarization of fissile materials.¹

Demilitarizing weapons-grade uranium and plutonium is an established industrial practice: These fissile weapons materials are blended with industrial-grade low-enriched uranium oxide, resulting in the mixed oxide that fuels commercial reactors.

In fact, most fuel rods in civilian US power reactors contain at least some weapons-origin fissile material. Civilian nuclear reactors can profitably consume weapons-source materials while rendering them militarily useless.

Fissile conversion and demilitarization is a valuable disarmament method because it is cost-effective and irreversible in the long term; the fuel supply is reliable; and industry personnel have decades of experience in the process.

Demilitarization, which applies to both fission and thermonuclear weapons, would preclude reconstruction of proven weapons and reduce fears of treaty violations among both

nuclear- and non-nuclear-weapons states.

Coupled with a ban on production of weapons-grade materials, demilitarization would most durably and tangibly impede nuclear rearmament and would be attractive to a wide array of nations.

Reference

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Alexander DeVolpi

(waterfoxg@gmail.com)

Oceanside, California

I disagree with my friend Sid Drell about the implications of a world ostensibly without nuclear weapons. In such a world, the only countries with nuclear weapons would be Iran, North Korea, and the like. A treaty renouncing nuclear weapons would be a modern-day repeat of the folly of the 1928 Kellogg-Briand pact, the General Treaty for the Renunciation of War.

Such symbolic gestures are not harmless. The signatories of Kellogg-Briand included all the aggressors of the 1930s. Democracies are slow to recognize aggressor nations because their motives are incomprehensible to us. Our enemies arm before we realize their intent, and our defensive measures follow only after long delay. We listen to our Winston Churchills very late. As a wise Roman said: "If you wish for peace, prepare for war."

Disarmed, we would face the threat of even a single nuclear weapon without the ability to deter it. In the age of the intercontinental ballistic missile, not even the oceans offer strategic depth.

An American renunciation of nuclear weapons would be followed by a rush to proliferation as a dozen or more regional powers, no longer protected by an implicit or explicit American guarantee, build their own nuclear forces. Such a world, in which a multitude of rivalries and enmities become nuclear confrontations, would certainly be more dangerous than the present one.

Jonathan Katz

(katz@wuphys.wustl.edu)

Washington University in Saint Louis
Saint Louis, Missouri

Drell replies: In my article I emphasized the need for "a mechanism for international control of the entire [nuclear] fuel cycle at all stages." Establishing such a mechanism will be critical to making substantial progress toward a

world without nuclear weapons. "Demilitarization of fissile materials," as urged by Alexander DeVolpi, would be one of the important measures requiring cooperative enforcement.

As to Jonathan Katz's concerns about potential dangers in a world without nuclear weapons, I am also gravely concerned that with the spread of nuclear know-how, materials, and weapons, we face a growing danger of their falling into the hands of rogue states or terrorist organizations and enabling mass murder on an unprecedented scale. In today's world, relying on nuclear weapons for deterrence is becoming increasingly hazardous and decreasingly effective. Achieving a world of zero nuclear weapons cannot mean returning to the world of pre-1945. Knowledge of nuclear weapons will still exist, as will the capability to reconstitute them.

Getting to zero will take hard work on policy and technical issues. It will require an international consensus on strict compliance-monitoring procedures that are considerably more intrusive than achieved so far. A framework for increasing mutual trust and transparency was established by the comprehensive data exchanges and on-site verification measures negotiated in the New START treaty, and I am optimistic that detection of reconstitution efforts will be possible, as is essential by the time zero is achieved.

Those who see hope for a safer world without nuclear weapons need to get to work on eliminating them. My article is a call to action to meet that goal.

Sidney Drell
(drell@slac.stanford.edu)
Stanford University
Stanford, California

Cold fusion and reproducibility

In response to Bernard J. Feldman's review (PHYSICS TODAY, July 2010, page 50) of David Goodstein's book, *On Fact and Fraud: Cautionary Tales from the Front Lines of Science*, I offer a note on reproducibility and cold fusion. High-temperature superconductors were initially very difficult to reproduce, and many obscure results were noted but not regularly reproduced. If the researchers had sat on the results until they were totally reproducible, the field would have taken years longer to develop. The cold-fusion results suffered from actually being reproducible—so long as the experiment was flawed in

the same way as the original. I think Feldman has way overstated the importance of reproducibility to first publishing. Besides, as much as it felt like we had been foolishly led astray in the end, wasn't it fun to examine the possibility of cold fusion and those very odd and interesting electrochemical effects?

Fred McGalliard
(frederick.b.mcgalliard@boeing.com)
Seattle, Washington

Bernard Feldman is to be commended for his thoughtful review of David Goodstein's new book. However, Feldman has seriously misrepresented the scientific facts about the contentious topic of cold fusion. In particular, he suggests that Goodstein's "sympathetic view toward scientists working in cold fusion" is misguided because cold fusion is "a prime example of a field characterized by unverified results." Although the vast majority of the early attempts to reproduce the associated effects failed, not only were the effects reproduced, but with time, the reasons for the difficulties that were encountered have become well understood.

An important source of confusion is that Martin Fleischmann and Stanley Pons did not discover a colder version of conventional fusion. They discovered something else: a new form of aneutronic nuclear fusion, involving a two-deuteron reaction in which helium-4 is created without the emission of high-energy particles or gamma rays.¹⁻³ It is not altogether surprising that Feldman is unaware of that. Effectively, mainstream scientific journals have maintained an embargo against cold-fusion papers that report positive findings. That failure has, in fact, become a topic in the mainstream ethics-in-science literature.⁴ Twenty-one years after cold fusion was first announced, a more "normal" dialog about the subject⁵ is badly needed.

References

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