

letters

design weapons systems without bomb tests, it may be argued that the question of a test ban is irrelevant. If the military does not yet have scientists as good as the people at CERN, Fermilab or Stanford who design high-energy machines, or as good as the original team that predicted bomb performance with devastating accuracy, then better people could be hired.

It seems much more important to persuade the scientific community both that better bombs are not needed and that the effort is a waste of good people. We can, I think, face with equanimity the degradation of the existing stockpile; at least some of the miserable things won't go off. I, for one, am not in the least confident that our chance of survival is significantly increased thereby.

If the Russians find that their stockpile has not degraded, would they not conclude that ours hasn't either? But even if it has, this is still no reason for the deterrent to fail. Surely the balance of terror does not depend on whether a few devices fail to explode. Many of us now believe that the exciting story of the evolution of life on this planet will come to an end if the arms race continues much longer. It is galling to think that the vested interests of 10 000 individuals are responsible for it.

HANS MOTZ
Oxford, England

9/83

Defining determinism

In his article "How random is a coin toss?" (April, page 40), Joseph Ford makes a series of statements on randomness, determinism, continuum and infinity that are quite surprising to me. First of all, he seems to adhere to a definition of determinism that is at variance with the simple classical "nothing happens without cause." On the other hand, this traditional type of determinism has never been in contradiction to our daily experience of how "minute causes may cause big effects," as far as I see. Ford's argument cannot convince me that recent developments in nonlinear dynamics could have any impact on this familiar picture of classical macroscopic determinism.

Secondly, I do not believe that either classical physics or quantum mechanics have ever been based on the assumption of "infinite computational and observational precision." To the contrary, most of us physicists learned very early in life that no parameter can be measured with absolute precision on a continuous scale. I don't think we need arithmetic complexity theory to get that. Certainly, no physicist has

ever made use of an "incalculable irrational number having positive Kolmogorov complexity," and I do hope that no one ever lost his time on calculating anything, either $\sqrt{2}$ or π or e to a precision of more than a hundred decimals.

So, Ford may readily do away with his undefinable numbers. I would be very surprised if that had any effect on any one of the theories by which we try to describe the basic features of what we experience as physical reality.

E. BIEDERMANN
Boeblingen, West Germany

7/83

In response to Keyworth

It was distressing to read the letters (September, page 11) responding to George Keyworth's thoughtful and civilized Guest Comment. In May, Keyworth was "pained" to call the APS Council's resolution on nuclear war "arrogant." In September, his pain is perhaps relieved as he sees how far his critics excel him at *ad hominem* invective. One correspondent calls Keyworth "inexperienced and short-sighted" and speaks of his "emotionalism." A second correspondent (who probably has not bothered to read a single scientific paper by Keyworth) says that he holds his position "not as the result of any distinctive contributions to science but as a reward for his dedicated service to the Los Alamos nuclear-weapons factory." This correspondent seems to feel that "first-rate scientists" (possibly including himself; Fellows of APS, perhaps?) should be above "insulting and threatening" criticism by second-raters such as the President's science adviser—criticism which he describes as "intimidation" and "repression." A third correspondent calls Keyworth "a company man" and "politically motivated," and a fourth elegantly dismisses his ideas as "crap."

These letters read like a burlesque of an unusually entertaining faculty meeting. Unfortunately, they are meant in earnest. Anyone who reads them will see how easily we in APS lose our heads and abandon our commitment as scientists to civility, reason and truth, once we enter the political arena. We, and the Council, must balance the good we may do for the external world against the harm we surely do to our science, our Society and ourselves.

JAMES E. FELTEN
University of Maryland
College Park, Maryland

9/83

I applaud APS President Robert Marshak's firm support for the APS Council resolution on nuclear war (May,

page 9). If Presidential Science Advisor George Keyworth is shocked to see APS stepping into the political arena (May, page 8), then he may have lost sight of the reality that nuclear war is more than just a game in global politics.

The days are gone when physicists need only concern themselves with physics, politicians with politics and clergymen with religious matters. We embrace the separation of state and religion only to the extent that one does not dictate to the other. All disciplines of human endeavor have always been and are ever more intimately intertwined with one another. The issuing of considered statements on nuclear war, whether they are from a body of physicists or Catholic bishops, ought to be taken seriously and viewed by politicians as contributions from responsible citizens of the planet Earth rather than as intrusions into the political process.

If we recognize the interdependence of all nations, all elements of societies, and all walks of life, and if we learn to use dialog rather than rhetoric as the principal form of communication, then we may graduate from this precarious Age of Information to the Age of Enlightenment!

BILLY W. LOO
University of California
Berkeley, California

7/83

Error in history

C. P. Snow's *The Physicists* was reviewed in *PHYSICS TODAY* (September 1982, page 75). I would disagree with Joan Bromberg's conclusion, that despite its errors in fact, it "is an excellent book to hand, with suitable cautions, to undergraduate students in physics and history." In my opinion, the book is just too unreliable to be used. Trivial mistakes—James Franck becomes two people, James and Josef—should have been corrected before publication. It is a disservice to Snow that they were not. But other errors make one think that perhaps Snow didn't really know how physics is done, or worse, preferred a good story to the truth. For instance, Snow describes Fermi's early prediction of transuranic elements, and says (page 91), "It was a pity, people thought later, that Rutherford, who had died shortly before, wasn't on the scene. It was just the sort of problem Rutherford would have seen straight through." This is fantasy—Rutherford died in 1937, more than three years after Fermi's results, and no one "saw through" the problem. Painstaking persistence solved it. Snow then tells a muddled story of the fission discovery, and decides that physicist Lise Meitner was Otto Hahn's "collaborator" who had "complete trust in her old chief." In fact, the two