Seeking Answers From Cold Fusion Review

As a physics teacher who is uncertain how to answer students' questions about cold fusion, I welcome the upcoming US Department of Energy investigation of recent claims in this controversial area. I agree with Toni Feder (PHYSICS TODAY, April 2004, page 27) that "skepticism about the credibility and reproducibility of cold fusion remains widespread."

I have some questions I'd like to see the DOE investigators answer. Is it true that unexpected emission of neutrons, protons, tritons, and alpha particles (at rates significantly above the background) has been observed in several cold fusion experiments? Has accumulation of helium-4, at the rate of about one atom per 24 MeV of excess heat, been confirmed by many scientists, as reported by electrochemist Michael McKubre in Feder's story? Have highly abnormal isotopic ratios been found in some cold fusion setups? Is there any indication that leading cold fusion scientists are incompetent or that their data are fraudulent? Is the research methodology that cold fusion scientists use different from that used in other areas of physical science? Answers to these questions will help me decide what to think about cold fusion and what to tell students about it.

Speculations about practical applications of new findings should be de-emphasized at this time. They will emerge naturally when basic scientific claims are recognized as valid and when researchers in cold fusion are no longer treated as if they were con artists and charlatans. The "chilling effect" mentioned by Randall Hekman in the PHYSICS TODAY story prevents young scientists from entering the area of cold fusion research. I also agree with chemist Allen Bard that being able to reproduce experimental results is not good enough; it is only a preliminary step. But wasn't poor reproducibility the central point of criticism when cold fusion was first investigated 15 years ago? In my opinion, experimental claims should not be disqualified solely on reproducibility; validation should depend on credentials of researchers and, above all, on methodologies they used in particular experiments.

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Questionable Questions in Analysis and Synthesis

In "Analysis and Synthesis IV: Limits and Supplements" (PHYSICS TODAY, January 2004, page 10), Frank Wilczek describes the question "Why is the Solar System as it is?" as "discredited." But to do so is to discredit the many fields of physics—including geophysics and planetology—that dare to address phenomena that are not "universal" or "clean." Although our own solar system has a history that is perhaps accidental and idiosyncratic, it is nonetheless the "limited slice of the world" in which all of us live. A deeper understanding of the system's admittedly messy history is essential if we are to address intelligently such issues as global change and resource management. And on a philosophical level, knowing the particular happenstances of our history is as important to our humanity as knowing the story of one's own family or culture. To study that deep history is no less creditable or scientific than to seek transcendent explanations for worlds to which we have no access.

There are so many messy, intellectually challenging questions to which the legions of brilliant, un- and underemployed physicists might fruitfully turn their thoughts. I am saddened to see such lines of inquiry devalued.

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Frank Wilczek says that the question of precisely when a radioactive nucleus will decay has been "rendered questionable by quantum mechanics." Apparently, most physicists take that for granted. However, using quantum mechanics as the reason we physicists can't solve complex subatomic problems is simply too convenient. We can just as easily think of classical, deterministic problems that exhibit the same statistical characteristics as subatomic problems do. As an example, I offer a *gedanken* experiment: the radioactive wiffle ball.

Take a baseball-sized wiffle ball, place a BB inside, and shake it vigorously. After a time, the excited wiffle ball will emit a BB and thus become stable. Repeat the experiment thousands of times, and you will observe that radioactive wiffle balls have a half-life. Should an outside observer assume that the internal processes of the wiffle ball are random? No, what we have is a deterministic problem with an infinite number of initial conditions. The behavior is describable only statistically, but is not due to random processes. Statistical behavior at any level is not proof of randomness in the physical world.

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Wilczek replies: Each correspondent has a valid point. I enthusiastically agree with Marcia Bjørnerud: The nonuniversal problems that arise in describing our specific place in the world are not only valid but often fascinating and important. I was building toward this major point in the entire series, and it was emphasized explicitly in the final sentence: "Such necessary concessions to reality compromise the formal purity of the ideal of understanding the world by analysis and synthesis, but in compensation, they allow its spirit much wider scope."

I also agree with Joe Lacetera, though more reservedly. The idea that the statistical aspect of quantum theory might reflect our incomplete comprehension of an underlying deterministic theory has had some extremely eminent champions, from Albert Einstein at the beginning to Gerardus 't Hooft today. It is a difficult program, however, since the success of quantum theory is broad and deep, especially in the atomic and subatomic realms. I'd be more optimistic about finding surprises in the recent, promising, but relatively poorly tested application of quantum theory to cosmology, as I mentioned in the column: "We can test the hypothesized quantum origin of primordial fluctuations by checking whether those fluctuations satisfy statistical criteria for true randomness."

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Shapley and Hubble: Different Views Brought Galaxies Into Focus

The existence of galaxies beyond the outer limits of our Milky Way system has only become a certainty within the last century. Much of the credit for that discovery goes to Edwin Hubble and Harlow Shapley, who were, in many respects, the two outstanding early 20th-century US astronomers most devoted to the study of external galaxies. Both were born in rural Missouri; Shapley in November 1885 and Hubble in November 1889. As soon as they had obtained their doctorates, both were hired by George Ellery Hale to work at Mount Wilson Observatory in California.

Both men entered astronomy almost by chance. Hubble started out by training as a lawyer. Perhaps his legal training contributed to the clear and convincing way in which he presented scientific arguments. Shapley began his career as a journalist; that training made many of his articles and books a joy to read.

In 1918, Shapley used observations of the distribution of globular clusters to establish that the center of our galaxy was located in the constellation Sagittarius.[1] We now know that his estimated distance of 17–25 kiloparsecs to the galactic center was larger than the actual distance of 8 kpc. Shapley was unaware of the existence of interstellar dust, which makes clusters appear dimmer, and hence more distant, than they really are. His discovery that the Sun is located far from the center of our galaxy had an impact on human thought similar to the paradigm shift caused by Copernicus's change from a geocentric to a heliocentric model for the universe. Jan Oort in the Netherlands and Bertil Lindblad in Sweden were subsequently able to show that the Milky Way system is in differential rotation around the galactic center in Sagittarius; they thus supported Shapley's discovery.

With Adelaide Ames, Shapley discovered and studied large-scale structure in the universe;[2] that work turned out to have a profound influence on modern theories of the universe's early evolution. Surprisingly, large-scale structure never appears to have attracted Hubble's interest. In his monumental study of the distribution of galaxies, Hubble concluded that, after correction for the effects of dimming by dust in the galactic foreground, the distribution of galaxies is essentially uniform on large scales. Furthermore, he found that counts of the surface distribution of galaxies were essentially Gaussian in log N, where N is the number of galaxies per square degree in the sky. Perhaps Hubble obtained the result he wanted (and so