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difficult even under the far easier circumstances in the US. How many of the Los Alamos physicists can explain why they continued to work on the bomb after the original motive of keeping ahead of the Germans had disappeared? How many are willing, like Bob Wilson,² to admit they should have stopped?

In looking back 50 years, let us recognize that the Oppenheimers and the Heisenbergs were honorable men caught in a terrible moment of human history. Let us not talk about "apologetics" but let us try to learn the lessons that may help us face our responsibility in the future.

References

1. E. Heisenberg, *Inner Exile: Recollections of a Life with Werner Heisenberg*, Birkhäuser Boston, Cambridge, Mass. (1984).
2. R. R. Wilson, *Bull. Atom. Sci.*, June 1970, p. 30.

LINCOLN WOLFENSTEIN
*Carnegie Mellon University
Pittsburgh, Pennsylvania*

Your unsentimental journey through the Nazi and Allied A-bomb projects (PHYSICS TODAY, August 1995) was fascinating and timely in this 50th anniversary year. Your articles—taken together with *Time Bomb* by Malcolm C. MacPherson (E. P. Dutton, 1986)—illustrate that "history as wishful thinking" is just as dangerous in atomic apologetics as it is in other areas.

MacPherson's book demonstrates how lucky the Allies were to have gotten the A-bomb. For one thing, the Allies used relatively abundant and inexpensive graphite as a moderator whereas the Germans used scarce heavy water (they also looked at graphite but got discouraging results from their tests). One can imagine Werner Heisenberg slapping his forehead on learning that the Allies had used graphite.

For another thing, although the Germans had access to uranium from Czechoslovakia, uranium was so scarce in the US that the whole Manhattan Project was endangered. But by a fantastic stroke of luck, back in the fall of 1940, a Belgian named Edgar Sengier, the chairman of the Union Minière de Haut-Katanga, had shipped 1140 metric tons of rich uranium ore (originally from the Belgian Congo) to a warehouse on Staten Island. When General Leslie Groves sent Captain Kenneth Nichols on a top secret mission to find uranium, Sengier was already expecting him.

Absent Sengier's prescience, the Allies might have been as hamstrung by a lack of uranium as the Germans were by a lack of heavy water.

The problems that came into being with the dawn of the nuclear age are still with us, and among the most serious is the proliferation of nuclear weapons. This will continue to be a problem as long as plutonium is seen as a feasible energy source. The plutonium used to generate power is the same as the plutonium used in a bomb. The consequences for consistent foreign policy are readily apparent, as demonstrated by recent events involving Iran and North Korea.

America's traditional antipathy toward government involvement in science, which waned in the postwar era, is now undergoing an ideological resurgence. Many individuals are pressing for less government involvement in science, as in business and the arts. However, if there is one area in which the interests of science and government should coincide, it is in the development of energy alternatives, such as fusion power, which could leapfrog plutonium as an energy source.

In conclusion, I offer a conjecture about Otto Hahn's statement that "the fast [neutrons] in 235 do the same as the 238, but 130 times more" (see Bernstein and Cassidy's interpretation of Hahn, page 35): Could it be that Hahn was simply referring to the fact that uranium-238 is approximately 130 times more abundant than uranium-235?

MARK E. SINGER
Winnetka, Illinois

Manhattan Project: Book Faulted, Heisenberg Paged

The article "Groves and the Scientists, Compartmentalization and the Building of the Bomb" (PHYSICS TODAY, August 1995) lists in its references *Manhattan Project: The Untold Story of the Making of the Atomic Bomb*, a 1967 book by Stephane Groueff. Readers without an intimate knowledge of the project will most likely accept without question the accuracy of the Groueff text. However, as a research scientist and subsequently division director in the wartime Substitute Alloy Materials Laboratory (Columbia University), I would like to point out an erroneous report in his account of the SAM research.

The diffusion separation cascade

for concentrating uranium-235 had to be sealed as completely as possible from the external atmosphere. That required reducing the air leakage through the shaft seals of the gas compression machinery to an unprecedentedly minute amount. Groueff alleges that the design of this seal was undertaken by me at SAM and independently by a staff engineer at the Kellogg Corp (engineers for the construction of the cascade), and that I designed a seal that completely failed a cascade pilot plant test. Groueff's claim and much of the accompanying detail are erroneous. No seal was designed by me or members of my group; the failed seal mentioned by Groueff involved only Kellogg personnel.

HENRY A. BOORSE
*Barnard College
New York, New York*

The wartime weapons laboratories at Los Alamos were all connected by a public address system. If one was unable to reach a person at his or her regular phone extension, one could call and ask the telephone operator to page the person. Many times each day, paging calls for specific named individuals were heard throughout all the laboratories. I remember that, on a few occasions, one heard the PA system call out, "Werner Heisenberg, please call extension ____" or "Werner Heisenberg, please report to the director's office."

ALBERT A. BARTLETT
*University of Colorado at Boulder
Boulder, Colorado*

Spin Model Skirmish

The article "Where Does the Proton Really Get Its Spin?" by Robert L. Jaffe (September 1995, page 24) explains very clearly why this question has been exercising the minds of many theoretical and experimental physicists ever since the surprising measurements from the European Muon Collaboration. However, despite our generally favorable opinion of the article, we feel that we must react to the author's statement that "because the Skyrme model has many problems with more traditional hadronic phenomenology, no one takes it very seriously as a way out of the spin crisis" particularly because this assertion is made as a comment on a joint paper we wrote.

It is true that the phenomenology of the Skyrme model is neither completely understood nor perfect, but the model does have some striking successes to its credit—for example in fitting pion-nucleon scattering phase

shifts. Moreover, it has been related by Edward Witten to the underlying theory of quantum chromodynamics in the limit of light quark masses and a large number of colors. Further, the Skyrme solitonic approach has been derived explicitly by Yitzhak Frishman and collaborators in a two-dimensional reduction of QCD with any number of colors. The writers of well over a thousand papers have taken the model sufficiently seriously to discuss its applications in many aspects of particle and nuclear physics.

For these reasons we think it suggestive that the Skyrme model predicts that Σ , the quark helicity contribution to the nucleon spin, vanishes for light quarks in the limit of a large number of colors. Neither of these assumptions is exact for the actual physical situation. However, the available experimental data from the EMC, followed by that from the Spin Muon Collaboration and the Stanford Linear Accelerator Center, lie within the likely accuracy of these approximations. The Skyrme model can thus provide an example of a limit in which the so-called spin crisis can be explained. We urge others, as well as ourselves, to strive to overcome the model's imperfections and relate it to the more familiar constituent quark model, which did not prepare us for the EMC result.

STANLEY J. BRODSKY

*Stanford Linear Accelerator Center
Stanford, California*

JOHN ELLIS

CERN

Geneva, Switzerland

MAREK KARLINER

Tel Aviv University

Tel Aviv, Israel

JAFFE REPLIES: I apologize to my friends for my patently incorrect assertion that "no one takes it [the Skyrme model] very seriously as a way out of the spin crisis," and for my having associated my negative remark so closely with their stimulating paper (which I have read with interest and refer to often). I should have said "I do not take it very seriously as a way out of the spin crisis"—a phrase that I would apply at one level or another to all of the models on the market.

Exciting physics almost always spawns controversy and, as I tried to indicate in my article, theorists continue to debate the origins of the spin crisis passionately. Meetings on QCD spin physics are enlivened by debates among adherents of the Skyrme model, gluonic anomalies, chiral quark models, and so on. There is no satisfactory model of hadron struc-

ture: Quark models do not adequately describe chiral symmetry; the Skyrme model and other models based on chiral dynamics do not give an adequate description of many quarkish features of hadron structure. Richard Feynman used to classify theorists and their models as those who worship "the quarks" and those who worship "the pion." I think it best to view them all as toy models, useful for gaining insight into phenomena but dangerous to take too seriously.

This is not the place to debate the virtues of the Skyrme model at length. Briefly, I have two reasons for not taking it very seriously as a solution to the nucleon spin problem.

First, the problem lies in a flavor average channel, where the model has particular difficulties. The model cannot be formulated in a world with only one quark flavor—although QCD has no fundamental problems with such a world. In addition, the model predicts that the mass of the flavor-average pseudoscalar meson (M_η) should vanish, and the mass of the flavor-average baryon (M) should go to infinity in the same limit (that of an infinite number of colors) that it predicts $\Sigma \rightarrow 0$. In the real world $M_\eta > M_N$.

Second, the model is most successful describing a world of only up and down quarks. Extensions to include strangeness have been problematic, but the appearance of strange quarks in the nucleon is at the heart of the spin problem.

I agree with Brodsky, Ellis and Karliner that these reasons should spur us not to abandon the model but rather to make further efforts to understand and improve it. I look forward to many lively debates on this subject in the future.

ROBERT L. JAFFE

*Massachusetts Institute of Technology
Cambridge, Massachusetts*

APS Statement on EM Fields Debated

The lively controversy regarding possible health effects of environmental electromagnetic (EM) fields has prompted the American Physical Society to go on record (as reported in the *New York Times* on 14 May 1995) as saying that "conjectures linking cancer to power line fields have not been scientifically substantiated." Further, the APS has taken a policy position arguing for elimination of research in this area on the grounds that using funds to combat "a threat which has no persuasive scientific ba-

sis" leads to the neglect of "more serious environmental problems."

One might be tempted to dismiss the statements of a group whose expertise is so far afield from the medical issues they are considering. However, given the many contributions of physicists to solving problems in biology, it is important to give full consideration to the APS assertions. The issues that need to be addressed can be framed in two questions:

▷ Are the physicists aware of relevant work in biology that contradicts their conclusions?

▷ Is the highly simplified model of a cell that is critical to their conclusions relevant to processes in cells that can be affected by EM fields?

Our answer to both questions is no.

For a start, the authors of the APS statement do not appear to know of the work carried out by several biological laboratories that shows that weak EM fields have significant effects on biochemical reaction rates, including changes in protein biosynthesis.¹ The changes caused by EM fields are similar to the response of all cells to stress ("the stress response"), whereby cells synthesize stress proteins to control damage brought about by environmental stimuli. In other words, cells interpret EM fields as a noxious stimulus, and there is strong evidence that the fields stimulate the synthesis of stress proteins. It is obvious that if the cells are compromised or overexposed, the stress response may not be able to compensate. One cannot simply overlook these scientific developments because they have not been published in physics journals.

The physicists seem not only to have ignored the published biological evidence, but also to have based their conclusions on calculations using a highly oversimplified theoretical model of a cell, not on real cells. Their theoretical creation has as its only structure a cell membrane, whereas a true cell contains many vital structures. This is a critical issue because the membrane may not be the most relevant part of the cell with regard to the stress response. (Recent measurements on DNA, for example, have shown that the double helix has a very high electronic conduction,² and bases can be made to flip out of the double helix.³ These new properties may be involved in interactions with EM fields.) Further, the physicists' oversimplification continues in that they have conceived of their model membrane as a simple uniform boundary layer, whereas an actual true cell membrane has multiple structures.