of my bet implied that the discovery of the antiproton deserves the Nobel Prize. A few years after the discovery, it became clear that the properties of protons and antiprotons are not completely described by Dirac's original theory, since they interact differently with leptons of different helicity—perhaps a "weak" justification for my bet!

Reference

1. M. Goldhaber, *Science* **124**, 218 (1956).

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Some of the information given in Owen Chamberlain's obituary needs correction. Let me quote the problem sentences:

> In the summer of 1943, [Emilio] Segrè and his group, including Owen, moved to Los Alamos, New Mexico, where they were assigned the task of determining the spontaneous fission probabilities of uranium and plutonium isotopes. Their initial results suggested an unexpectedly high rate of spontaneous fission, but later they determined that most of the "spontaneous" fission events were actually induced by cosmic rays. Nevertheless the rates were high enough to require a complete redesign of the bomb trigger.

That there would be high spontaneous fission rates was not unexpected. What was unexpected was that reactor plutonium would have as much plutonium-240 as it did. Plutonium-240, like uranium-238, has a high spontaneous fission rate. That has nothing to do with cosmic rays. As Niels Bohr and John Wheeler first argued, it is a barrier penetration matter. These wartime results are summarized in Segrè's 1952 paper,[1] which also has a brief summary of the theory. Also, the last sentence is misleading. What the results required was the use of implosion for the plutonium weapon. The uranium weapon—Little Boy—was able to use the original gun-assembly design, as Hiroshima sadly proved.

Reference

1. E. Segrè, *Phys. Rev* **86**, 21 (1952).

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Jaros, Nagamiya, and Steiner reply: We thank Maurice Goldhaber for sharing with us his entertaining account of the circumstances surrounding "the bet." We also thank Jeremy Bernstein for his clarification of our statements about the spontaneous fission results obtained by Emilio Segrè's group at Los Alamos. Our very brief comments about spontaneous fission were based on Segrè's autobiography. With respect to the effect of cosmic rays, Segrè writes,

> Among other things, we found that the "spontaneous" fission at Los Alamos was greater than at Berkeley, which was not surprising because cosmic-ray neutrons, more abundant at a high altitude than at Berkeley, were obviously responsible for the effect. We had only to screen the chambers suitably to make it disappear.[1]

The impact of spontaneous fission on bomb design is addressed in his next paragraph:

> We soon recognized that Pu^{239} had a rate of spontaneous fission high enough to interfere seriously with the proposed methods of bomb assembly through predetonation. . . . Our results brought the Los Alamos lab to a real crisis. . . . Spontaneous fission in plutonium was so frequent that the plutonium alternative for making a bomb was excluded unless one could invent and develop a totally different assembly method.[1]

Reference

1. E. Segrè, *A Mind Always in Motion: The Autobiography of Emilio Segrè*, U. California Press, Berkeley (1993), p. 196.

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Galaxy collisions and ordinary dark matter

Bertram Schwarzschild's Search and Discovery item (PHYSICS TODAY, November 2006, page 21) brings back vivid memories from the early 1950s, when I was a junior faculty member at Yerkes Observatory. Such notable astrophysicists as Subrahmanyan Chandrasekhar, Gerard Kuiper, William Morgan, and Gerhard Herzberg at Yerkes and Lyman Spitzer and Martin Schwarzschild at Princeton University had ongoing discussions about the dynamics of galaxy collisions.

Those notables generally agreed that only the gas in each galaxy collides and is combined and left behind. All condensed matter, stars, and failed stars down to gram-sized debris proceeded without significant collisions, affected only by the gravitational field of the galaxies and perhaps of some individual stars.

Kuiper was adamant that most of the baryonic matter in the nascent galaxies never condensed into stars, remaining invisible but discrete entities. He pointed out that the optical cross section per gram of matter drops rapidly with increasing size, thus the effect of this dark matter on optical extinction would be minimal. Because of this dependence, he stated his opinion that most of the baryonic matter in the universe would be invisible to detection except by gravitational effects.

Herzberg agreed that dark matter would have no spectroscopic signature in the observable spectral regions. Spitzer added that this dark matter would be heated by the stellar radiation field to a temperature of a few degrees kelvin, a function of the stellar radiation intensity distribution in each galaxy and the albedo of the matter. At that time there was no prospect on the horizon that detection of dark matter at 3–5 K would become possible.

In the 1950s model that arose from the discussions, there was no need for exotic dark matter. That simple model of a collision between galaxies and of the role ordinary dark matter plays in such an event deserves to be restated, since it seems to have become eclipsed when exotic dark matter took center stage.

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Emil Konopinski, mentor to Bahcall

I enjoyed reading the obituary of my friend John Bahcall (PHYSICS TODAY, July 2006, page 63). Unfortunately, the brief note that he was a postdoctoral fellow at Indiana University did not mention Emil Konopinski's contribution to John's education. I think it is fair to say that it was Konopinski who taught John the fine points of neutrino