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 theory, the area in which John made his greatest contributions. I can still remember discussions John and I had about his vision of the use of neutrinos for astronomical purposes, in which I heatedly denied the possibility of any such thing as neutrino astronomy. Well—he was right, and I was shortsighted. (I don't recall what Konopinski's position on this was.)

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LIGO precision understated

I enjoyed Philip Bucksbaum's enthusiastic reference to the precision measurements attained by LIGO, the Laser Interferometer Gravitational-Wave Observatory (PHYSICS TODAY, June 2006, page 57). Bucksbaum and your readers may be pleased to know that these achievements were actually understated.

LIGO instruments detect changes less than a thousandth of a proton diameter over a distance of 4 km. A proposed LIGO upgrade is expected to improve displacement sensitivity by a further order of magnitude.

The Laser Interferometer Space Antenna, a planned space-based detector, will not have such fine displacement resolution, but it will span 5 million km, so its strain sensitivity will be comparable to LIGO's. LISA's greater length means space and terrestrial experiments will sample complementary, nonoverlapping frequency ranges, and thus target different realms of astrophysics.

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Superparticles on the energy horizon

In his article "Is String Theory Phenomenologically Viable?" (PHYSICS TODAY, June 2006, page 54), Jim Gates extrapolates from past discoveries to estimate the time of discovery of new massive elementary particles such as those predicted by supersymmetry. He uses the discovery dates and measured masses of the neutron and the W boson, and proposes that "one can crudely estimate the rate at which humanity is progressing in its ability to detect massive particles: about 1.5 GeV/c^2 per year. Thus, if Nature is kind enough to provide light superpartners, one might still expect about a century to pass before a superparticle is directly observed."

A more appropriate extrapolation would be from the discovery of the bottom quark ($mc^2 \simeq 4.5$ GeV) in 1977 to the 1995 discovery of the top quark ($mc^2 \simeq 175$ GeV), a rate of approximately 10 GeV per year. Given that 10 years have passed since the top discovery, using these two points to extrapolate might lead Gates to conclude that we're already overdue for another discovery.

Neither of these extrapolations is relevant, however. The timing of discoveries at accelerators will be determined by investment in facilities such as the Tevatron, the Large Hadron Collider, and the International Linear Collider, and in talented people who work on the machines and detectors and who analyze the data, including theorists who develop the techniques and tools necessary to compare the data to standard model predictions. I hope these efforts have Gates's full support and that of the string theory community.

Henry Frisch
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Gates replies: A longer article would have indicated that the advance of the energy frontier is a complicated function. Henry Frisch has made reasonable suggestions for some of its independent variables.

Though possessing a career-long interest in supersymmetry, I recall from graduate school that given a scatter-plot of data to make a linear fit to a complicated function, one generally does better using the longest possible baseline. Thus, my estimate is likely the more accurate. Besides, when a theorist finds a number that is correct to within an order of magnitude, victory is usually declared.

Researchers excited by superstring/M-theory are foremost and thoroughly dedicated and well-trained *physicists*. Accordingly, they are rooting most enthusiastically for the success of their experimentally driven colleagues, if for no other reason than the opportunity for vindication. It would be a point of great pride to have clearly perceived "the mind of God."

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Salivating over chocolate

Erich Windhab's Quick Study item about the complex rheological characteristics of chocolate (PHYSICS TODAY, June 2006, page 82) seems to be missing one ingredient—saliva. Doesn't the combination of chocolate's taste and odor stimulate the generation of saliva and thereby affect the fluidity of the product even while it is melting? Therefore, doesn't this factor alter the conclusions of the study?

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Windhab replies: Saliva does interact with the chocolate during consumption. That interaction affects not only the flavor of the chocolate but also its flow properties. However, the effects are taken into account in the article's conclusions that refer to sensory properties. Consumers' strongly differing saliva productions during chocolate consumption could make sensory testing results inconsistent, but we did not see such differences among our test participants.

Erich Windhab
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Corrections

February 2007, page 40—The energy of attraction given by equation 2 should have a minus sign.

February 2007, page 41—In equation 4, the area *A* should be in the numerator, not the denominator. ■