LETTERS

LHC Evidence for More Dimensions Accelerates Futuristic Discussion

I was very happy to read Gordon Kane's essay (PHYSICS TODAY, May, page 13), which describes the startling new discovery made at the Large Hadron Collider. But I read the article with some measure of disappointment as well. Let me explain.

Up until 13 years ago—that is, until the late 1990s—I was a practicing experimental particle physicist, performing studies of *CP* violation in what were called fixed-target experiments. Having worked in that mode for over 20 years, I then decided to switch fields for a variety of reasons (including the lack of support for this style of physics), and have spent the intervening years dabbling in experimental cosmology, the detection of gravitational waves and precision tests of quantum mechanics, among other things. I have thus been largely removed from the themes of experimental particle physics, so I welcomed the chance to catch up.

Why was I disappointed? I noticed that Kane mentions many individuals—Coulomb, Klein, Kaluza, Yukawa, Maxwell, Antoniadis, and Quiros—and they are all theorists. But what about the experimenters who made the discovery? Already when I left the field, it seemed that it was difficult for individuals to achieve recognition in the very large collaborations even when they clearly were responsible for crucial advances. Kane's article is entitled "*Experimental* Evidence for More Dimensions Reported," and he concludes with, "For the first time in a long time, *experiment* is a little ahead of theory" (emphasis added). Yet no experimenter is mentioned in his article! Are the experiments now so large and complex that individuals are completely overlooked?

Nevertheless, it appears that, over the past 13 years, all the advances have come on the experimental side: the discovery of supersymmetry at Fermilab (I remember Kane's long-ago claim, in a *CERN Courier* article, that it would most likely be discovered at CERN's Large Electron–Positron collider), the discovery of a Higgs boson (I've been away from the field so I don't know where or when this was accomplished) and now this very recent discovery of one or two very narrow, very high mass states. Yet the most recent of Kane's theory references dates back to the 1990s.

I also would have liked to see what each of the two detectors contributed separately to this discovery. In particular, with respect to the two events indicating a narrow state with a mass around 1.9 TeV, did one detector see both events, or is there some measure of a "confirmation"? As I write this, I am remembering the debates in the 1990s about the necessity of having multiple redundant detectors.

Finally, I tend to agree with Kane that this recent discovery is a genuine surprise, the kind of which had "not happened for nearly four decades in *collider* particle physics" (emphasis added). But my friends who are still pursuing fixed-target physics are telling me that there are surprises there. Laurie Littenberg informs me that an unusually large signal for $K^+ \to \pi^+ \nu\bar{\nu}$ (I think the first evidence for this came from Brookhaven National Laboratory when I was still active) has been established by a Fermilab experiment at a level many standard deviations from the Standard Model. Lincoln Wolfenstein (who invented the superweak model nearly 50 years ago and whom I bump into socially from time to time) informs me that the pattern of *CP*-violating signals and other rare decays in the K sector just doesn't fit with those observed in B decays: It seems that a sizable "superweak" contribution still remains. And hints of a signal in $K_L \to \pi^0 \mu e$ are being pursued vigorously (as I understand it, the signal level agrees with one of Kane's early predictions). Yet the field still seems to be neglecting this important probe of new physics.

Despite my disappointment, it was very nice to see that Gordy is still active in the field.

By the way, it was gratifying to

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learn that the US–Japanese e^+e^- collider was finally built and is now in operation (even if its energy is not yet sufficient to explore these new states). Long ago, I was involved in a National Research Council study that recommended going ahead with a complete design for this machine. I'd love to visit the facility; where is it located?

Bruce Winstein
University of Chicago
Chicago, Illinois

[Editor's note: Gordon Kane's essay, one of the three cowinners in our Physics Tomorrow Essay Contest, was dated May 2011]

Kane replies: Bruce Winstein raises several interesting and subtle questions. Although he and others have been pursuing the fundamental experiments he mentions, some experimenters have been willing to pay the admittedly high price of working in a large collaboration because there is no other way to directly probe nature to smaller distances than by working at colliders and looking for the new particles that signal new physics. Fortunately, the sociology of the detector groups has evolved since the late 1990s, so that papers reporting new collider data now carry the collaboration name plus the names of the main people directly involved in the analyses being reported. In this case, however, my "Search and Discovery" account was based on one very recent conference report, and the papers are still not available, so we cannot yet be sure who was responsible for the main discovery. Evidently, though, a few graduate students and postdocs working with the data late at night noticed the relevant events and searched for others, but very detailed and subtle analyses were then needed to determine the lepton and jet energies (to learn whether the parent states indeed had a common mass) and to confirm that the events were real, and I did not wish to report names until the collaborations have made them public.

The Higgs boson was discovered at CERN, in 1999, just as the *CERN Courier* had predicted. Only a few events were produced, but they were enough to measure the mass. Then many thousands of Higgs bosons were produced at Fermilab, and, knowing the mass, the researchers could measure several important branching ratios, including the two-photon one. Although a few superpartners were produced at the Large Electron–Positron collider, separation from background there was not successful, and the superpartners were eventually detected and studied at Fermilab (in retrospect, one could then work out what had occurred at LEP), as reported in earlier "Search and Discovery" columns in Physics Today.

Of course, there are many more theoretical works relevant to the indirect effects of the extra dimensions on our world, and how we can observe them. In fact, since my account was published in Physics Today, several others have appeared that were being worked on independently as mine was being written. Although the field moved a little slowly for some years before the events I described happened, it recently became quite active, even before the reported discovery was made. Unfortunately, space limitations kept me from describing all of the work.

Winstein's "surprises" serve as a reminder that the degree of astonishment usually derives more from the beholder's perception than from the event itself. The excess rate compared to the Standard Model for the K decay to pion and neutrinos was predicted by supersymmetry; once the light superpartner masses and couplings were measured, they were systematically included in predictive loop calculations. Similar expected effects were found in B decays. In essence, starting a few years ago, the default was the Supersymmetric Standard Model rather than the historical Standard Model; consequently, these indirect effects of superpartners in loops are really not that surprising.

Finally, selecting the location of the US–Japanese electron–positron collider was an interesting saga that is worth recalling briefly here.

After the years of the Clinton Administration, the US government increasingly realized that fundamental research is a powerful source of economic strength, partly because research at the frontier necessarily leads to inventing new techniques that then often develop into new products and industries, even if the results of the research are themselves unlikely to be commercialized, and also because larger numbers of bright young people are drawn into science and technology when the country is visibly more active in fundamental research. As the government learned, once those young people were excited by science, they stayed and were productive in many areas often unrelated to the ones that had originally attracted them. Thus, the collider was built as a US government facility.

As younger people with more scientific sophistication rose in the State Department, it became easier to bring other countries into the process earlier, and as Japan emerged from its financial crisis of the late 1990s, it was happy to join the US as a major partner in the collider. Following Europe's example, the US government and the science community agreed that the US research labs should be centralized at one facility, even though the technical breakthroughs that had lowered the cost of the collider so that it was affordable had been made at the Stanford Linear Accelerator Center. And that is how the collider ended up at the Fermilab site, with the enthusiastic support of the lab director. Winstein will not have far to go to visit it.

GORDON L. KANE
(gkane@umich.edu)
University of Michigan
Ann Arbor, Michigan

Seventy-Plus Years in Physics: Bethe Finds His Match—in Family

I believe one can take exception to Kurt Gottfried's remark, in his review of Hans A. Bethe's new book (PHYSICS TODAY, July, page 65), that no other physicist of this or any other era could have written Bethe's opening sentence: "This book contains a selection of my publications of the 70 years during which I have been active."

In fact, Bethe's own father-in-law, Paul P. Ewald, could have written the same sentence. Ewald's doctoral research on crystal optics (completed in 1912 under Arnold Sommerfeld) was the impetus for Max von Laue's famous investigations that launched the field of x-ray diffraction. Ewald continued his research in optical and x-ray phenomena (including the development of both the reciprocal-lattice theory and the dynamical theory of x-ray diffraction) for over 70 years, until he passed away in 1985 at the age of 97. His last paper was published posthumously in *Acta Crystallographica* (volume 42, page 411, 1986).

REUBEN RUDMAN
(rudman@panther.adelphi.edu)
Adelphi University
Garden City, New York

Physics Update: 'Fractional' Flux Quanta May Be Random

With regard to your "Physics Update" story on quantum boxes for Cooper pairs (February, page 9), I want to point out that the "fractional" flux quanta in Andrey Geim and company's measurement of magnetization as a function of flux are basically random portions, not rational fractions, of the quantum. There are, however, other examples of fractional quanta that are neatly determined and have recently appeared in the literature.

In an array of wires containing Josephson junctions at a temperature of 0.3 K, the resistivity as a function of flux clearly shows that fractional flux occurs. The values of $1/2$, $2/5$, $1/3$ and $1/4$ are clearly seen,[1] and the theory can perfectly explain fractions of less than $1/2$.

For a *single* Josephson junction with various phase shifts, the smallest value of the flux permitted by the present theories is $1/2$ of $hc/2e$. However, my colleagues and I have found that $1/4$ flux quantum is the minimum.[2] Our result is deduced from the turning point in the magnetization as a function of temperature in the paramagnetic Meissner effect.

Thus, the smallest commensurate value of the flux reported to date is $1/4$, according to us.[2] Arguments have been put forward in support of the elementary flux being $hc/2e$, but other, smaller values have not been contradicted.[3]

References

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3. N. Byers, C. N. Yang, Phys. Rev. Lett. **7**, 46 (1961).

KESHAV N. SHRIVASTAVA
(knssp@uohyd.ernet.in)
University of Hyderabad
Hyderabad, India

Lawsuit Update: More on APS/AIP's Dispute with Gordon & Breach

Irwin Goodwin took the time to speak with me at length about the case he covered in "Court Rules for APS and AIP in Dispute with Gordon & Breach over Survey of Journals" (PHYSICS TODAY, October 1997, page 93). In addition to correcting one misquotation, I would here like to mention a few brief points that are pivotal, but unfortunately were omitted from Goodwin's story. Since my letter is appearing 11 months after it was submitted, I also want to take this opportunity to update your readers on the status of the case.

Fundamentally, G&B objects to the

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