

LETTERS (continued from page 15)

led the CERN council to request contributions from nonmember states scheduled to be major users. These contributions should be not only for the detectors but also for the accelerator itself. The American contribution to the accelerator is not intended to replace some missing member state's contribution, but rather to hasten completion of the project.

The LHC could be built by the CERN member states alone, as had to be demonstrated when the project was approved in 1994. The initial stand-alone scenario assumed that a 10 TeV machine would be operating in 2004 and that it would be completed at 14 TeV by 2008 (after a one-year shutdown). However, with the extra contributions now expected from the US, Japan (which has already made two generous contributions in cash), Russia (which has pledged an important contribution in kind) and others (Canada, India and Israel, so far), the now-approved and ongoing plan is to complete the full machine at 14 TeV as early as 2005. And 2005 already looks far away to the many physicists eager to explore the new promising domains that will be opened by the LHC!

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Theory Is Tied in Nots, but Strings May Have 'Signatures'

Gordon Kane is to be complimented on his trenchant commentary about experimental tests of string theory [PHYSICS TODAY, February, page 40]. When a realistic string theory—namely, one that is mathematically complete, calculable and in agreement with existing experiments—is eventually formulated, it certainly will make testable predictions of the type Kane describes.

At present, no satisfactory realistic string theory exists. It may therefore seem surprising that some experiments potentially testing strings are actively being performed.

The point is that there may be detectable “string signatures”: observable physical effects from strings that are forbidden in conventional particle physics. They could serve as signals of strings even in the absence of a specific realistic model.

Kane mentions one possibility that my colleagues and I have suggested:

a mechanism in strings producing *CPT* (charge conjugation-parity-time reversal) violation that could be detectable in the *K* system.¹ Current experimental sensitivity to these effects² is close to the Planck scale, and experiments now being designed will reach it.

The idea has been extended to the *D* and *B* systems.³ The first experimental measurement of a *CPT*-violating parameter in the *B* system has been performed.⁴ This result provides a bound near the Planck scale on possible *CPT*-violating effects involving the *b* quark. Any future *B*-system measurements are likely to be an order of magnitude more sensitive. Effects may also be observable in other sectors of the Standard Model.⁵

So, if anything, Kane has understated the situation. Not only will a realistic string model eventually make testable predictions, but the framework of string theory can already be experimentally investigated now.

References

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Author-to-Author Contact Simplifies AIP Figure Reprint Policy

Both David Stern (“Letters,” February, page 11) and Keith Seitter (“Letters,” May, page 94) complain about the time and trouble involved in authors obtaining permission to reprint figures in, say, review articles. They each offer what they hope will be a solution to the problem.

The American Institute of Physics policy regarding copying of figures, tables etc. is printed in the front matter of all its journals: “Permission is granted to quote from the journal with the customary acknowledgment of the source. To reprint a figure, table, or other excerpt requires the con-

sent of one of the authors and notification to AIP.”

Here at AIP (where I am journal publisher), we believe that the requirement to get the original author's permission, besides being a common courtesy, is good policy because an author might well wish to supersede the original figure with new data, or simply to disown the figure even if nothing better is forthcoming. I remember that the physicists on the advisory committee that helped us establish the wording of our “permissions” statement insisted on the inclusion of this particular statement about obtaining the author's permission.

Notice that we do not require the copying author to get our permission, but only to notify us and provide “customary acknowledgement of the source.” However, for the editors and production staff at most publishing houses, it is much easier to insist that their authors get permission for everything, from everybody, than to try to keep track of the various permissions policies in place elsewhere. Despite our policy, we still receive many letters “requesting permission,” rather than providing notification (we are happy to reply with our approval), and I suspect that Seitter at the American Meteorological Society still gets them too.

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New Elements Could Be Better Identified: Namely by Number

I read with dismay your May story (page 52) on the naming of elements 104 through 109. While creating new elements through anthropogenic means can contribute to an understanding of our physical universe, I believe it is extremely pretentious to name these elements, since, for all intents and purposes, they are not found in nature. Worse yet, for learned men and women to engage in pointless wrangling over such names demeans the noble goals of physics, and it makes even me, a devoted student of science, question how society's money is being spent at publicly funded institutions.

I am not indicting the fine work done by the people involved in creating the new elements. Rather, I am encouraging them to put aside self-congratulation and recognize that these created elements mainly serve academic purposes and should not be held in the same esteem as the discov-

ered elements.

Ernest Rutherford and Niels Bohr are well remembered for their contributions and might even be offended that ephemeral, man-made elements invoke their names. I hope scientists can reign in their egos and let their achievements speak for themselves. I also hope that when I read the May 2002 issue of *PHYSICS TODAY*, there will be a story on the creation of, say, element 115 that will commend the work of all involved.

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Science Arms Us with Facts, Sometimes Is Disarmed by Authority

In reference to the ongoing debate in *PHYSICS TODAY* and other publications regarding the scientific method, I find it difficult to understand the great difficulties that consideration of the method seems to raise.

Succinctly stated, the scientific method has always been: Valid, reproducible data taken in context is truth, and the simplest theory that agrees with all the data and that permits predictions of reality is the best explanation of truth. In an ideal world, science reaches the truth by a planned and systematic series of measurements that decided between dispassionately proposed, opposing theories. In practice, however, the process is human—that is, intuitive, ingenious, democratic, autocratic, complex, slow, chaotic, argumentative, sometimes spiteful, etc. This reality is greatly oversimplified by historians who seem to prefer the ideal. What is important is not the detailed process that occurs, but that the final state reached meets the two proper criteria of truth: agreement of theory with data, and simplicity. Of course the process cannot be too inefficient or society cannot afford it.

In their much-discussed-in-your-columns book *The Golem: What Everyone Should Know about Science* (see, for example, *PHYSICS TODAY*, January, page 11), Harry Collins and Trevor Pinch seem to believe that the opinions of authorities play a far bigger role in the consensus process than does the existence of valid data. That is often the case in the early formative part of the process pertaining to a specific issue, but I doubt that it is true in the final stages, when truth is ascertained on the basis of a wide

web of evidence, as David Mermin has so ably contended.

I also note that the true authorities that carry the most weight seem armed with a lot of facts (namely, data) and knowledge of the relationships between those facts. It is very important to science that the proper authorities—that is, an open peer group—conduct this process. Nevertheless, the history of science is replete with other authorities, such as religious leaders, governments and deified individuals, declaring “truth” and imposing it on the system, usually with its eventual removal taking between one and sixteen centuries.

The danger to science from a book such as *The Golem* is indirect, in that the book gives solace and support to other authorities.

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NSF Review Process Should Be Revamped, Not Taken for Granted

Having been busier than usual conducting my research program on the molecular-level understanding of liquid water (see *PHYSICS TODAY*, April 1996, page 9) with no National Science Foundation funding whatsoever, I have only recently seen the brief story in your January 1997 issue (page 52) on the NSF’s then-proposed guidelines for judging proposals.

For those of us who have struggled in second-tier research institutions trying to promote new scientific ideas, the since-adopted changes (to go into effect in October) are laughable. Any scoring procedure, whether based on two or four criteria, is a trivialization of the review process. And it is much easier for the NSF staff simply to average the scores than actually evaluate the science.

By definition, a reviewer of a proposal, and certainly the NSF staff itself, simply cannot know more about a new research idea (as opposed to an ongoing or derivative one) than the



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We had better publish this quick before our numbers change again

author, particularly an author who has had a history of creating new ideas. The term “peer reviewer” seems to imply that there is a group of other scientists “out there” who, if they so desire, can duplicate what the author of a proposal wants to do—again, a trivialization of the development of scientific ideas. The word “gatekeepers” used by John Fanchi in his letter to the editor criticizing scientific refereeing (*PHYSICS TODAY*, August 1996, page 15) seems closer to the truth, since one doesn’t need to know much to guard a gate. Unlike the author of a scientific paper, the author of a proposal has no means of addressing a reviewer’s criticisms directly. A new proposal must be written, a year goes by, and it is never clear that the same reviewer or the same hollow criticisms will emerge anyway.

History has shown that criticism of a really new idea can very often be wrong. Guglielmo Marconi could not obtain financial support for his wireless, believed to be line of sight by the nonionosphere-thinking experts of the day, and the Wright brothers were actually ridiculed (“flyers or liars?”) even after demonstrating the validity of their ideas. Throughout its existence, the NSF has based its choices for research funding on the narrow perspectives of often short-sighted, prejudiced or microscopically focused self-serving reviewers. This situation is hurting American frontier science, especially now that research funds have become so limited that a