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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JOAN CARTIER

We had better publish this quick before our numbers change again

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 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

single less-than-excellent rating can sink a proposal (unlike in the 1960s and 1970s). What is more, the gross waste of time caused by NSF paperwork requirements imposed on both the NSF staff and practicing scientists having to write, rewrite and review proposals, at the expense of the science itself, will certainly be felt in the future.

The question is, Does the NSF now do more harm than good to American science, or would it be better to just take the grant money, add to it all the skyrocketing administrative costs and distribute the funding on the basis of recent past performance—numbers of papers, quality of journals in which the papers appear and any documented recognition of the work by others? Using these guidelines, such grant applications could be evaluated much more objectively and in a far less time-consuming manner than by the current ponderous procedures. Starting scientists would get a standardized starter grant. Why should starting scientists in a university be judged by the NSF anyway? They have already been judged more carefully by their universities. That's how they obtained their jobs in the first place.

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DOE Clarifies Budget Plans for High-Energy and Nuclear Physics

I am writing to correct a serious and unfortunate error relating to the R&D budget of the Department of Energy (DOE) as reported in Irwin Goodwin's story in your March issue (page 61). I also take issue with his coverage of the DOE budget included in a follow-up story in the April issue (page 47); although that piece is an improvement over the previous story, it still contains certain inaccuracies.

In the table and text of the March story, Goodwin has apparently missed the new structure in the fiscal year 1998 budget whereby the High Energy Physics (HEP) and Nuclear Physics (NP) Programs are not fully contained in the general science and research account, but also have funding in the (new) science asset acquisition account.

To see a complete and accurate picture of the two programs, the funding from both accounts must be combined. The combined funding for what used to be general science and research increases from $996 million in FY 1997 to $1017 million in FY 1998. Within this total, the HEP program increases by only $4.96 million, which is all related to a transfer of funds and corresponding responsibility from the DOE Environmental Management program. Similarly, the apparent increase in the NP program results from the inclusion in the FY 1998 request of the FY 1999 construction funding ($16.62 million) for the Relativistic Heavy Ion Collider (RHIC) project. If these special changes are set aside, the HEP and NP budget requests for FY 1998 are both exactly the same as the FY 1997 actual budgets.

Thus, Goodwin's statement in March that DOE's research programs would rise more than 4% from FY 1997 to FY 1998, "mainly because of increases in the general science account for US participation in CERN's Large Hadron Collider" is totally incorrect. This error is compounded by the omission of the science acquisition account from the accompanying table, and by the inclusion of the LHC in that table without acknowledgment that the LHC funding is already included in the total for general science and research given four lines above in the table.

Likewise, the increase in the NP program from FY 1997 to FY 1998 shown in the April story (see table on page 48) is illusory. The actual situation is that both the HEP and NP programs are being proposed for flat funding. Thus, US participation in the LHC is being done in the context of a flat budget, and the LHC funding is being obtained by adjustments in other parts of the HEP program.

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Luminescence from Porous Silicon: Mechanism Debated

The mechanism of light emission in porous silicon is controversial, yet the limited review given in the PHYSICS TODAY article by Reuben Collins *et al.* (January, page 24) presents only the quantum confinement point of view.

It is clear from the literature[1] that quantum confinement can play a role in the absorption process, especially in freshly made porous silicon. However, in view of the experimental evidence available and in particular the lack of correlation between the particle size and emission energy, it is hard to justify an emission model based solely on quantum confinement and to totally disregard suboxide- and/or oxyhydride-related models as the source of room-temperature red-light emission in porous silicon, especially in the case of stable oxidized porous silicon.

Reference

1. For a discussion of the various emission models, see S. M. Prokes, J. Mater. Res. **11**, 305 (1996).

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Collins, Fauchet and Tischler reply: In explaining the luminescence from porous silicon, we face a choice that tends to divide those working in the field: Either we (1) try to find one basic explanation to account for the strong luminescence in samples prepared with a wide range of surface terminations, or (2) we propose very different models for different samples as suggested by Prokes and Glembocki. The goal of our article was to convey that a growing body of evidence suggests that quantum confinement plays a central role in the mechanism of luminescence from a wide range of strongly luminescent samples, while noting that there is still uncertainty in understanding the mechanism. In particular, whether emission occurs from pure quantum confined states or can also involve surface states or defect levels remains an open questions--as discussed in the article.

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Fixing CIA's Corona Camera Cleared Up Film, Fogged History

Albert Wheelon's article on the Corona satellite reconnaissance system in your February issue (page 24) contains a major factual error. He states that it was on his initiative that the Drell team became involved with and identified the source of the