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

electrostatic corona discharge that early on threatened the viability of the program. As Itek's chief engineer for the Corona camera system, I can attest that although the Drell team was made aware of the problem, neither Sidney Drell nor any member of his team ever visited Itek to observe the phenomenon or sent suggested solutions to Itek.

Wheelon states that the Drell team and Itek engineers traced the problem to the rubber rollers that were used to move film through the camera. In fact, there was never any doubt as to the source of the corona discharge; we could see the discharge by looking through the observation window in the wall of the vacuum chamber. Eliminating the discharge was the challenge, and it was the Itek test and quality control personnel who, after scores of tests, finally found a method of cleaning the rollers and thereby bringing the discharge under control. It was Edward Purcell, a Nobel laureate at Harvard University, who visited Itek and validated Itek's solution.

I first sent Wheelon a letter of correction after he made his camera-fixing claim public at the CIA's May 1995 event honoring the Corona program. I did so again in July 1996. His persistence in misstating history is mystifying.

Frank J. Madden
Quincy, Massachusetts

WHEELON REPLIES: Corona satellite photography was playing a vital role when film darkening was first observed. The problem grew and afflicted more and more film. Having established regular photographic coverage of the USSR and China, President Kennedy and his Cabinet deemed it unacceptable to lose this extraordinary resource. When I became responsible for Corona as the CIA's deputy director for science and technology, Director John McCone made it clear that we must solve the problem—*soon*.

We turned to Itek, which was responsible for camera design and manufacture. CIA program people met repeatedly with Itek engineers to establish a course of corrective action. Itek's response was not reassuring. So McCone and I established a panel of outside experts to examine the problem—in parallel with Itek's efforts. I believed that the key to the problem lay in physics. I therefore asked Sid Drell to lead the effort. His panel included Dow Smith of Itek.

The Drell panel analyzed all possible sources of trouble, eliminating them one by one. The panel and Itek arrived at the same solution at roughly the same time. Both received the thanks of a grateful government. (Incidentally, Ed Purcell was present at our request when Drell gave us his report.)

The Corona camera problem in no way diminishes Itek's extraordinary contribution to national security. Such problems do occur in daring technological efforts. Frank Madden played a leading role in the design of the camera and remediation of its only problem. I sorrow that he continues to ignore the contributions of others.

Albert D. Wheelon
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New Results Are Right on the Quantum Dot

I would like to briefly add some important information to the PHYSICS TODAY story entitled "Experiment Signals a New Phase of Quantum Dot Measurements" that appeared in the January issue (page 19).

Amir Yacoby *et al.*[1] not only proved coherency of electron transport in the quantum dot (as explained in the story) but also pointed out that the phase of the transmission coefficient is periodic—that is, it repeats itself for a large sequence of Coulomb-blockaded peaks. Subsequently, Ralf Schuster *et al.*[2] (whose work is also covered in the PHYSICS TODAY story) reconfirmed this measurement and measured the actual phase itself—in a four-terminal configuration. In the process, they discovered the unexpected abrupt phase slips that take place between Coulomb blockaded peaks.

The work of both groups was theoretically supported all along by the condensed matter theoretical group at the Weizmann Institute of Science. Alex Kamenev, Yuval Oreg, Yoseph Imry, Yuval Gefen, Yehoshua Levinson and Moshe Shechter pointed out the role of the Onsager symmetry in the two-terminal geometry giving rise to the phase rigidity observed experimentally.[3] (The four-terminal measurement is one way to relax this constraint.) Recently, the unexpected periodicity of the measured phase an electron gains in a quantum dot and the abrupt phase slippage have been subjects of theoretical debates. Note in particular the work done by Oreg and Gefen,[4] who were the first to study systematically the effect of strong electron–electron interactions on the phase slippage in the quantum dot.

References

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3. Y. Gefen, Y. Imry, M. Ya Azbel, Phys. Rev. Lett. **52**, 129 (1984).
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Call for Emission Limits Heats Debate on Global Warming

I would like to follow up on your last major story regarding the controversy about global warming stemming from last summer's publication of the "Second Assessment Report" (SAR) of the Intergovernmental Panel on Climate Change (IPCC) (PHYSICS TODAY, August 1996, page 55). Your story focused on the disputed text changes made in the SAR's chapter 8 ("Detection of Climate Change and Attribution of Causes") and reflected the fact that, until then, the controversy had centered on the changes themselves—their legality, authorship, purpose and importance.

After you went to press, however, a far more serious problem arose when statesmen at the July 1996 conference of parties to the UN Climate Treaty in Geneva accepted as a basis for urgent policy action the IPCC's main conclusion, derived from chapter 8, that the "balance of evidence suggests . . . a discernible human influence on global climate." This innocuous-sounding but ambiguous IPCC phrase—which appears to have been based mainly on two research papers by Benjamin Santer *et al.*[1] (Santer was the convening lead author of chapter 8)—was misinterpreted by the Geneva meeting attendees to mean that a major climate catastrophe is upon us.

A "ministerial declaration" by the US and like-minded nations issued at that meeting called for amendments to the current treaty that would mandate "legally binding targets" for emission limits to carbon dioxide—and in effect constrain the generation of energy. Such global controls on energy use would have serious economic consequences, impacting mainly on the world's poor.

In announcing this drastic shift from the current voluntary policies, US Undersecretary of State Timothy Wirth declared, "The science calls