National Science Foundation to establish and implement appropriate priorities. The decision to "increase" funding for the Condensed Matter Sciences Section by 0.5% and for the Special Programs Section by 0.2% in fiscal year 1988, the first new budget year since the discovery of high-temperature superconductivity, appears particularly inexplicable; the 7.4% increase for the more engineering-oriented section of DMR demonstrates that even in a year of tight budgets, funds could be found for projects of priority to the NSF management. While part of this increase may have gone for research in processing of superconducting materials, the budget of the Polymers Program also increased by 14.2% from 1987 to 1988. Equally perplexing is the essentially flat funding (before inflation) of the Special Programs Section at a time when NSF management has presented centers as a major new initiative.

In the face of these substantial reductions in real dollars for condensed matter science, the issue was not whether cuts would be required, but only when and how the cuts would be carried out. Perhaps NSF's senior management would have preferred a policy of more gradual attrition that attracted less attention from the community. Instead of making scapegoats of rotating program directors who struggled to maintain the condensed matter sciences under extraordinarily difficult circumstances not of their own making, the senior management at NSF should analyze the policies or oversights responsible for the substantial cuts in real funding for condensed matter science over the past four years, and should explain why the foundation has failed to respond in a positive way to the revolutionary scientific developments in high-temperature superconductivity.

NSF's priorities, as reflected in practice through its patterns of budgetary allocations, are a legitimate matter of public concern. Individuals and groups interested in the future of condensed matter physics should urge that NSF remedy its disregard, in practice, for condensed matter science.

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NICHOLSON REPLIES: Joseph Serene's letter refers to the reductions in outyear commitments in condensed matter sciences discussed in the April PHYSICS TODAY (page 61). In that report I am correctly quoted as stating that the reductions were made necessary because overly optimistic budget projections led some program officers to make commitments that could not be sustained, and that this situation should not have been allowed to develop.

I believe it would have been appropriate for Serene to have noted that during the worst period of overoptimism in 1987, he was the section head for Condensed Matter Sciences at NSF and thus bears significant responsibility for the decisions that were made. The current section head and rotating program officers, who have had to deal with the effects of those decisions, had no part in their making. It was certainly not my intention to make "scapegoats" of them.

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Inflation Anti-Defamation

In the news story "Large-Scale Structure, Streaming and Galaxy Formation" (October 1987, page 19), it is claimed that "models based on the inflationary paradigm are . . . severely strained by . . . recent observations." The main arguments, according to the quoted remarks of Richard Bond, are that "it is difficult to make a scale-invariant spectrum [produced by inflation] work with large-scale streaming" and that if Neta Bahcall's results (on the two-point correlation function $\xi(r)$ at separations r on the order of 50–100 megaparsecs) are correct, "then they are completely inconsistent . . . with *any* plausible theory based on the inflationary paradigm for density fluctuations." It is claimed also that the situation could be improved with a spectrum of fluctuations growing on large scales or with non-Gaussian fluctuations. But James Peebles is quoted as saying, "Within inflation, no one has come up with a source for non-Gaussian fluctuations."

We would like to note that within inflation it is quite possible to obtain spectra of fluctuations of almost arbitrary shape[1,2] as well as non-Gaussian fluctuations.[1-3] It is possible also to obtain exponentially large domains with a different density of matter inside each,[2,4] or with almost equal total densities but different densities of baryons, or with the same density of all particles but a different spectrum of fluctuations inside each.[2,5] Thus the inflationary paradigm is much more flexible than is sometimes believed.

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One should emphasize that whereas we do know many ways to modify particular details of the inflationary scenario that relates to the theory of formation of the large-scale structure of the universe, at present we do not know any alternative internally consistent scenario of universe evolution that would not use inflation. Observational data can be used to verify different theories of galaxy formation, but it is not very easy to abandon the inflationary paradigm, which serves as a unique basis for *all* these theories. Moreover, some recent investigations by Andrei G. Doroshkevich, Anatoly A. Klypin, Lev Kofman and Alexey A. Starobinsky show that the large-scale streaming can be quite consistent with even the simplest scale-invariant spectrum of fluctuations produced during inflation. In particular, the last two authors show that near the minima of the Gaussian distribution of the gravitational potential $\varphi(r)$, the average streaming velocity v is directed toward these minima (great attractors), and that near some of these minima the streaming velocity can be considerably bigger than the dispersion $\langle v^2 \rangle^{1/2}$, obtained by averaging over all space. According to this model, we are falling not onto a large mass, but into a deep hole!

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BOND REPLIES: I completely agree with Andrei Linde and Lev Kofman that the cosmological reasons for invoking inflation are so compelling that any discrepancy with the data should be very firm before the theory is dismissed. It is the set of all data as currently interpreted that appears to strain the inflationary picture. This includes the very tight Soviet constraints[1] on large-angle microwave background anisotropies compared with the reported observation[2] of anisotropy at 8°, the clustering of clusters[3] compared with the clustering of galaxies[4] and of galaxies about clusters,[5] as well as the abundance of rare events such as a great attractor. The latter may well turn out to be an overly coherent interpretation of a complex large-scale velocity field; other methods relating the data to theory do not indicate a problem with the inflation picture for universes dominated by cold dark matter.[6] I also agree that constraining our velocity field to one that would arise near a large mass overdensity (or deep potential minimum) such as a great attractor certainly leads to larger velocities,[7] but the problem shifts to one of assessing whether the constrained structure (great attractor) is too rare to be accommodated by the inflationary theory in question. This depends upon how coherent the velocity field is.

Density fluctuations from inflation that break the "natural" scale-invariant outcome of inflation are certainly possible.[8] What is debatable—and Kofman and I have had many enjoyable sessions on this point—is how probable these "designer" spectra are, which build a specific characteristic scale required by the large-scale data into the fluctuations by tuning scalar potential parameters or initial field configurations. Similar tunability problems afflict attempts to build non-Gaussian fluctuations that would agree with all of the data, although this is an area that has not been systematically explored.

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'Mini-Universes': Too Big to Be Seen?

The article by Andrei Linde on inflationary theory (September 1987, page 61) was refreshing because it opened up possibilities rather than closing them down. But Linde says that "any observer in the inflationary universe can see only those processes occurring nearer than H^{-1}, as illustrated in figure 3." If this is true, then exterior "mini-universes" have no scientific meaning because they cannot be observed. If, however, other mini-universes can exist within the space–time of our own universe, then we have the very interesting possibility of observing regions where physics is much different from our own, present-day, terrestrial physics.

He only implies in the next few sentences that two inflationary domains might "eat one another, or do each other any damage." I think it might be worthwhile from the standpoint of many unexplained observational anomalies in astronomy to discuss a little more fully whether what Linde calls "classical space–time domains emerging from space–time foam" could be related to young matter emerging from singular regions in extragalactic space.[1]

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Linde replies: Halton Arp raises several interesting questions. Domains with low-energy physics different from ours in general may be small, and this actually may have important observational consequences.[1] However, typically their size is much greater than the size of the currently observable part of the universe. One will be able to see such domains in the distant future when the size of the observable part of the universe, which is proportional to its age, has become large enough. But even now these domains have a quite real scientific meaning for those who live in them.

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Don't Belittle Big Science

I read Leo Kadanoff's essay "The Big, the Bad and the Beautiful" (February, page 9) with mixed emotions. I applaud his statements about the importance of basic research and the criteria that should be used in its evaluation: "The true value of science is in the development of beautiful and powerful ideas.... We should push for the kinds of work that are of enduring intellectual and technical value." But I am dismayed by his suggestion that the merit of a scientific project depends upon its size.

Nothing intrinsic to big science prevents it from leading to valuable, beautiful ideas; for that matter, nothing inherent in small science guarantees that it will be worthwhile. Indeed, it is revealing that Kadanoff offers no criticism of a field that exemplifies big science—high-energy physics—on its merits. Past support of large particle physics projects has been rewarded with the discovery of CP violation and hence better understanding of the evolution of the universe; with the observation that protons and neutrons are composed of even smaller constituents; and with the unification of the weak and electromagnetic interactions, and the indications of a possible common origin of all forces—all beautiful and powerful ideas of enduring intellectual value. Similar examples in other fields are readily available. The work that Kadanoff himself favors, with "neutron sources that might give insight into the new superconductors," requires big facilities: large, expensive, high-flux reactors. If the goal of a scientific project is worthy, it should be pursued in whatever style is most appropriate.

I also disagree with the opinion that small science offers a better training ground for graduate students. Just as