pared 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