of physician-scientists and suggests several well-thought-out solutions. ASCI vigorously endorses the need to address this urgent problem.

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Origin and Ancestor: Separate Environments

Nicholas Galtier *et al.* (Reports, 8 Jan., p. 220) argue on the basis of calculated values of the guanine plus cytosine (G+C) content of ribosomal RNAs that the last common ancestor of extant life on Earth was not a hyperthermophile. They correctly point out that this neither supports nor invalidates claims that life originated at high temperatures. They clearly state that the environment in which life began may not have been the same as the one in which the last common ancestor thrived.

This thrust of the report by Galtier *et al.* is accurately reflected in the accompanying item in This Week in Science (8 Jan., p. 143). However, in Gretchen Vogel's News of the Week article "RNA study suggests cool cradle of life" (8 Jan., p. 155), the two environments have been conflated.

It is important to recognize that the arguments for and against a thermophilic last common ancestor are almost irrelevant to discussions of the temperature at which life originated. The last common ancestor seems to have been an organism with a biochemistry about as complicated as that of a contemporary bacterium, and it must, therefore, have had a complex evolutionary history. During the course of their evolution, the predecessors of the last common ancestor may have adapted one or more times to changes in the temperature of their enviroment. Therefore, it is broadly accepted that the nature of the last common ancestor, whether or not it was a thermophile, may not provide evidence about "the cradle of life."

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Health Care Costs

There is something fundamentally wrong when scientists produce cost-benefit analyses defending the value of what they do, as in "Effects of medical research on health care and the economy" by Herbert Pardes *et al.* (Policy Forum, *Science*'s Compass, 1 Jan., p. 36).

The need to support scientific research to comprehend ourselves and the world around us ought to be self-evident. But when it comes to judging whether research will illuminate our understanding of human disease and reduce health care costs, projections are dangerous because they are likely to be blurred by factors that have not been adequately analyzed. The result is false public expectations and damage to the credibility of science.

In the early 1970s, I became involved in reporting the recombinant DNA developments and other findings about the gene and the cell that had emanated from scien-

tists at my institution (Stanford). The talk then was that gene therapy was just around the corner. Twenty-eight years have passed, and gene therapy is still a dream largely unfulfilled. With few exceptions, most scientists went for the hype, ignoring the technical challenges involved and the need to elevate the level of basic scientific knowledge required to make gene therapy a reality.

Another practical fallout of the biological revolution was its effect on pharmaceuticals. It was predicted that recombinant DNA technology would lead to novel and cheaper drugs, ignoring the huge investments required to set up biotechnology companies for producing such drugs and proving their biomedical application.

Novel drugs for Alzheimer's disease, osteoarthritis, AIDS, cancer, high cholesterol, and assorted genetic disorders have been developed, of course, but are far from cheap or definitive, as predicted. When the biotechnology revolution came upon us, drug costs amounted to only a sliver of health-care spending. But managed care's push to make drugs the first line of defense in treating chronic illnesses has caused spending to skyrocket, reaching about $94 billion last year (*1*); that is only the tip of the iceberg because the figures do not include the costs of treating the drugs' side effects. Health plans now spend as much as 14% of their budgets on pharmaceuticals and are expected to reach 20% by 1999. One reason managed care plans have performed so dismally during the past two years is the cost of drugs.

While some novel drugs have eliminated costly surgeries, and reduced mortality from infections and degenerative diseases such as AIDS, the notion that biomedical research must be supported because it is good for the economy is not good enough. A more compelling reason for vigorous public support of research is that our fund of knowledge is woefully inadequate for the far greater number of diseases that need to be confronted. The new knowledge depends on research that explores the basic biology and chemistry of a wide range of cells and organisms (*2*).

And if we really want to be all encompassing, we must support increases for the neglected field of nutritional research. Its costs are only a meager 0.12% of health care expenditures in the United States, but because experts have stipulated its potential for saving billions of dollars in the prevention of degenerative diseases, it is imperative that it become a national priority (*3*).

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Response

Even where there is consensus on the intrinsic worth of scientific research, policymakers must still determine appropriate funding levels within a climate of tightening budgets and competing social needs. Congress *will* rely on quantitative assessments and cost-benefit analyses to do this; if scientists do not weigh in with their own assessments of scientific progress, decisions will be made without them.

Andreopoulos cautions us not to overpromise about the speed with which biomedical progress will translate into lower health-care costs, but it is just as hazardous to be overly pessimistic, based on slower-than-expected advances in new fields, as it is to be overly optimistic.

The controversy around the value of the National Cancer Act (*1*) illustrates the downside of taking too narrow a view. Far from having lost the "war on cancer," targeted funding produced improved survival rates for patients under 55, better chemotherapies with fewer side effects, and molecular and genetic knowledge that led to more effective treatment of AIDS. Andreopoulos seems to take a similarly narrow view of the growing use of pharmaceuticals. Such growth must be understood as being offset by reductions in length of therapy (for example, days of acute hospitalization), as replacing older, less efficacious therapies and as "value added" in terms of length and quality of life.

Ultimately, fundamental biomedical knowledge about the basis of disease will be our best route to reducing health care costs. This effect has been seen in the area of infectious diseases, and will hold true for chronic illness as well. The economists and mathematicians who report to policy-makers cannot factor in future progress, however, if medical scientists are not active participants in the debate. Biomedical research is clearly one of society's best investments; those of us concerned with its future must take every opportunity to say so.

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The Cosmological Constant

James Glanz discusses the hypothesis, based on observations of supernovae, that our universe may be expanding at an accelerating rate ("Cosmic motion revealed," Breakthrough of the Year, 18 Dec., p. 2156). There is, however, a fundamental assumption involved that is not stated. All the measured supernovae must follow the same law of luminosity versus time.

A similar assumption applies to finding the distance of Cepheid variable stars and seems to be true. However, the distant supernovae did not start out with the same elements as those nearby: nearby supernovae initially contained the materials from earlier supernovae. If a stellar model builder could show that the requisite small amounts of heavier elements increase the luminosity of a supernova by 10 to 15%, then the need for acceleration would vanish. We could then reset the cosmological constant to zero, stop looking for grand sources of acceleration, and accept the viewpoint of Albert Einstein in his later years.

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Response

In concluding that cosmic expansion is accelerating, the supernova teams have taken up each of these interesting issues in great detail.

First, the luminosity "law" for the specific type of supernovae in question is not an assumption but an empirical fact. Extensive studies of nearby regions of the universe have shown that the rise and fall for intrinsically brighter supernovae is slower than for dimmer ones. This law allows the astronomers to calibrate the actual brightness of the supernovae quite accurately.

The law holds for nearby supernovae in a whole range of environments—from old elliptical galaxies to younger spiral galaxies. Among those environments, the range in the abundance of heavy elements is probably wider than the difference between a typical nearby galaxy and a distant one, so there is no compelling reason to think that distant supernovae behave much differently from nearby ones. Strengthening this conclusion are detailed observations of how the spectra of nearby and distant supernovae evolve during the explosion. Major differences in composition should be reflected in the spectra, but they are virtually identical.

Finally, computer models have shown that, while variations in heavy-element composition should have subtle effects on the rising part of the curve, the overall shape remains largely unaffected.

The supernova teams are expanding their work in each of these areas. So far, however, no such effect has been able to shoot down the conclusion that the expansion of the universe is accelerating. Strange as it seems, the best available evidence points to a cosmological constant that is not zero.

— **James Glanz**

Analyzing Solitaire

Dana McKenzie quotes Persi Draconis to the effect that "we cannot analyze the common game of solitaire," but explains