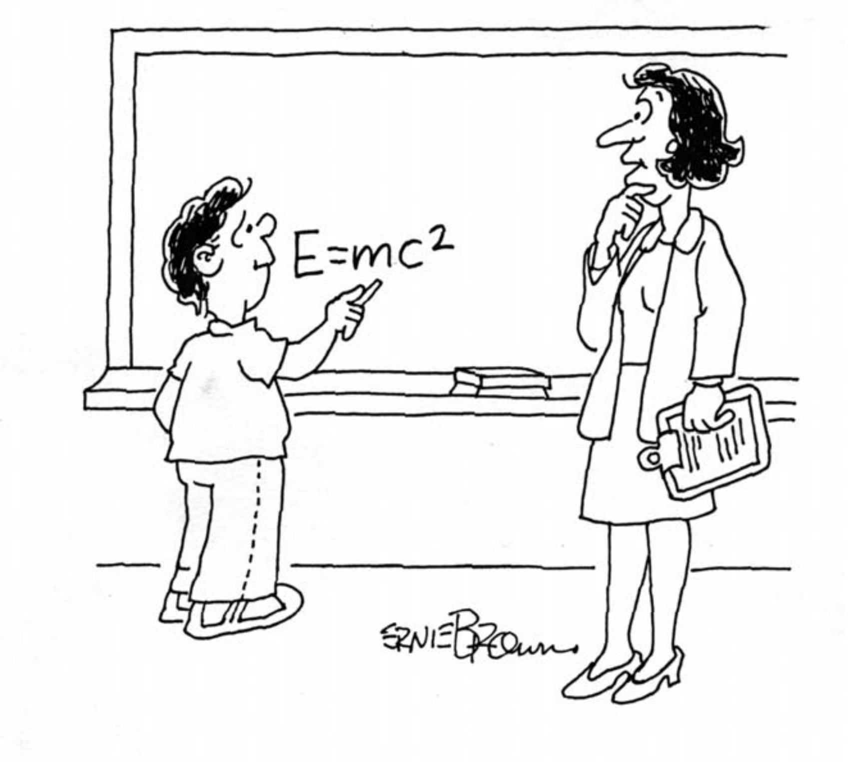
Smolin replies: The letters all make useful points. I agree with Marlys Stapelbroek that there are certainly ways in which schools could do more to encourage the creativity of young people. But my essay was focused on a simpler problem: Is the progress of science hindered by the current hiring and funding practices in the US, and could it be speeded up if a greater number of independently minded, original thinkers were supported?

Paul Roman seems to believe that we are fated to have no more than the odd genius per century and that nothing can be done to increase the rate of progress of fundamental physics. But certainly, if there are many more physicists working now than at any time in the past, shouldn't we expect the number of highly creative individuals of superior talent to increase as well? If the number of physicists has increased dramatically but the rate of progress has not, perhaps we need to examine whether something may have happened to the working conditions to slow down progress. Based on half a career's worth of observation, I think the answer is straightforward: not enough support and encouragement for creative, intellectually independent scientists who prefer developing their own ideas to following popular trends, and too much pressure to conform to the research programs of powerful senior scientists.

I am not proposing social engineering as a remedy, nor am I proposing that the problem be solved mainly by the establishment of new institutes. Although we should always be grateful for the support of science shown by those who found new institutes, my comments were addressed primarily to how existing institutions and foundations make choices about whom they hire and support. My proposals would open up more opportunities to scientists who pursue risky, independent, and novel solutions to problems that our best efforts over decades have failed to solve. Equally important is safeguarding the intellectual independence of the brightest young scientists, so that their rejecting well-supported research programs to pursue their own ideas does not involve a risk of professional suicide.

These proposals do not involve huge changes or expense. We already support a lot of research aimed at foundational problems; the question is just making sure that the criteria we use to pick where that support goes matches the risky and foundational nature of the science. In addition, the number of good scientists who have the talent, courage, and independence to contribute new ideas that might solve the hard foundational issues is, in any case, not large.



"Very interesting, Jason, but I'm pretty sure it's been done."

But even if they are not expensive, I believe that adopting the proposals I made in my essay will significantly increase the rate of progress in physics. For example, recently, prominent string theorists and particle physicists have told me they worry that they have been asking the wrong questions, and that progress may require a new set of questions. At such moments, science needs independent, foundational thinkers.

The utility of my proposals is testable: Any department or foundation could adopt my proposals and then, after a decade or so, measure the outcome.

Nor are any of my proposals new. Companies interested in being at the cutting edge of technological innovation or biomedical research do not make excuses by claiming that technology can only progress at a fixed rate. The availability of venture capital has encouraged the adoption by technology companies of principles like those I propose, and the result has certainly been an increase, perhaps even an exponential one, in the rate of technological and biomedical progress. I once asked a very successful venture capitalist how his firm decided what level of risk to take on. He said, "If more than 10% of the companies we help start up are making money after five years, we know we are not taking enough risk to maximize return on our investment." My proposals amount to suggesting that foundations, agencies, and universities treat a small part of the funds that go to support physics with this kind of high-risk, high-payoff strategy in mind.

It is not necessarily harder to judge quality and promise once the criteria are adjusted to emphasize originality and intellectual independence. I once asked Stuart Kauffman, a MacArthur fellow who pioneered the study of complex systems and their application to systems biology, his advice about how to identify young scientists with the promise to do important original work. "It's easy," he said. "By the time they are a few years past their PhD, the ones who are going to have many good, original ideas have already come up with several." After many years of reading postdoctoral and faculty job applications, I have learned that the few with truly original ideas stand out: They have high-quality single-author papers; they rarely collaborate with people senior to them; they choose to work on projects that, if successful, will be highly significant; and their research proposal is based on ideas not heard before.

Paul Roman claims that the foundations of quantum theory are not neglected and then proves my point by mentioning a list of people who are either dead or close to the end of their careers. Were the field well supported, he would be able to name important contributors in their twenties and thirties. In fact, young people are contributing important new results and ideas to the foundations of quantum theory, but none are working at US research universities. Let me name a few of them: Chris Fuchs, Lucien Hardy, Rob Spekkens, Antony Valentini, and David Wallace.

As to the absence of statistical evidence for an outflow of researchers from the US, the point is not quantity but quality. Quantum gravity and foundations of quantum theory are small fields, and not long ago most of the key ideas and results came from physicists and mathematicians at US universities. That is no longer the case. Work on quantum gravity was initiated mainly in the US by pioneers such as Peter Bergmann, Stanley Deser, Bryce DeWitt, James Hartle, Charles Misner, and John Archibald Wheeler. There were at one time active groups working in quantum gravity and mathematical general relativity at the Universities of California at Berkeley and Santa Barbara; the Universities of Chicago, Maryland, North Carolina, Pittsburgh, Texas, and Wisconsin; and Princeton, Syracuse, and Yale universities. Many groups are now working in string theory and a reasonable number are working on LIGO (the Laser Interferometer Gravitational-Wave Observatory) and numerical relativity. But only two universities—Maryland and Pennsylvania State—have more than one faculty member active in quantum gravity. Were the field dying intellectually, the scarcity would be warranted, but the opposite is true: Recent progress is impressive and rapid, with major new results coming from loop quantum gravity, Planck-scale phenomenology, causal dynamical triangulations, and causal set models. The only major country where support for this field is shrinking is the US. Abroad, the field of nonstring quantum gravity is flourishing. A recent international meeting on nonstring approaches to quantum gravity, the Loops '05 meeting, had more than 150 participants from around the world. But only 6 out of 80 speakers were from the US. France, Germany, the UK, and Canada were each better represented than the US.

I appreciate William Carter's point that important novel ideas and results do come from people at any age. But I do not think the issue of journals is key, now that we have the arXiv e-print server.

To T. J. Blasing's observation that anti-intellectualism in American culture may be a contributing factor, I add that some countries—France and the UK, for example—seem to have an intellectual culture that values independent and iconoclastic thinkers; one can see the results in a more diverse and critical scientific culture.

Burke Ritchie points out why someone like Einstein could do great work in a patent office—he was immune from pressure that even tenured professors and career researchers in government labs suffer to ensure that their research is funded. But I do not think the answer is to let our most independent and creative physicists work in patent offices. The case to be made, then, is that the progress of science requires a variety of minds and of scientific personalities. Many contribute by doing relatively low-risk mainstream work and following the big, clearly defined research programs. But equally important are those few who go their own way and follow their own unease with foundational issues by generating and developing their own ideas. What is needed is an understanding that scientific funding and hiring are not games to identify those who excel at clever solutions to narrowly defined questions. They are both about ensuring the progress of science, which requires making various kinds of investments, within which the high-risk, high-payoff work done by foundational thinkers has a small but absolutely necessary place.

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Discussing (or Not) Our Nuclear Future

A potentially enormous change in the way the US manages its nuclear weapons program is playing out with very little discussion.

Several books have been published this year on Robert Oppenheimer and Los Alamos. They remind us that even when Manhattan Project scientists were working flat out to develop and build the bombs, most of the scientists kept discussing the larger issues of national policy and how the bombs were to be used. Contrast that with today.

At present the major medium of discussion of the future of the Los Alamos National Laboratory and by implication the nation's nuclear weapons program seems to be the LANL blog (http://lanl-the-real-story.blogspot.com/). Discussion there of the impending change in laboratory management ranges from apprehension about benefits to character assassination of those figuring in recent Los Alamos controversies. Few comments have addressed the larger issues, and responses to them have ranged from nonexistent to derisive.

Few people now working at the lab recall, or know those who recall, the Manhattan Project and the dispirited days after World War II. Fascinatingly, some of the blog blather resembles withdrawal behaviors that were manifested 60 years ago in reaction to the new and dreadful reality of the bomb.

Most of today's adults were born and educated without having to learn to dive under their desks in case of nuclear attack, during which time we could contemplate the futility of that little action in the face of megaton weapons. Understanding of the danger of nuclear weapons is being lost as they are being conflated with chemical and biological agents as weapons of mass destruction. The reality is that there are nuclear weapons and then there is everything else.

The management of one of the nation's design laboratories by a private contractor reflects a change in US nuclear weapons policy. The possibility of a private contractor directing nuclear weapons design work was a subject of intense discussion at various times during the history of the weapons laboratories. It is now a done deal.

Other changes may follow. The reliable replacement warhead is under consideration for funding by Congress. The Overskei Report[1] describes one possible future: a single-site weapons development and manufacturing complex, with decreased competition between the design laboratories.