Mixed Reactions to 'No New Einstein'

I enjoy looking through my husband's PHYSICS TODAY. Yes, MBAs and PhD physicists can coexist, though I've never caught him reading my *Forbes*. Lee Smolin's "Why No 'New Einstein'?" (PHYSICS TODAY, June 2005, page 56) presented compelling ideas about fostering creativity at the graduate level and beyond. However, I believe the problem starts far earlier than Smolin would believe. Our oldest daughter is finishing a double major in computer and software engineering. It's taken a lot of energy and focus to keep her creativity alive. When she was in first grade, her teacher handed out a rectangular sheet of paper and told the kids to "cut it in half the long way." My daughter cut it diagonally, from corner to corner. The teacher told her that was wrong. I don't doubt that it wasn't what the teacher intended, but it was clearly the more correct interpretation.

Can you imagine what a bright, creative teacher could have done with that situation? But that would mean a first-grade teacher with more than minimal math skills. It would mean throwing out the morning's lesson plan, "No Child Left Behind" tests be hanged. It would mean making education an adventure instead of a sentence.

Our daughters have also had some superlative teachers—one gave extra credit if you could solve the math problem another way and explain why. Talk about throwing down the gauntlet! And there was the teacher of advanced-placement history, who asked random extra-credit questions that had us reviewing each morning's newspaper, trying to second-guess what would catch his fancy that day. We guessed right only about half the time, but we had some interesting discussions about the morning's headlines.

I truly believe it is not nature versus nurture, but nature amplified by nurture, that fosters creative genius. Western culture has come to equate creativity with thinking of a new place to put a body piercing. Until we begin to value and nurture true creativity from infancy on, I fear the next Einstein will remain dormant.

Readers of PHYSICS TODAY are in a unique position to provide some of that nurturing. Certainly encourage creativity in your own home, but be willing to step outside those walls. My husband and I do liquid-nitrogen demonstrations for schools and scout troops. (A downside is that we are now personae non grata at a local school that received calls about gunfire after we blew up a 2-liter soda bottle.) And, with heavy consulting from the actual scientist in the family, I teach after-school science classes.

The benefits of nurturing creativity go far beyond a single Einstein. What about the next Bill Gates, or the next Sergey Brin? Okay, I admit to having a business bias, but can you imagine life without Microsoft Windows? or without Google?? Right now, the US is living off the creative capital of its past. If this country does not rededicate itself to investing in creativity, the future will be greatly diminished, intellectually and materially.

Marlys Stapelbroek
(stapelbroek@cox.net)
North Tustin, California

While reading the June 2005 issue of PHYSICS TODAY, I was struck by Lee Smolin's comments, and by a brief news item on page 27, "Scientists Boycott Kansas Anti-evolution Hearings." I recalled that about 35 years ago, when I was young and idealistic, I applied to several universities for a junior faculty position, going out of my way to point out that I planned to spend a lot of time developing my courses, and that I felt quality teaching needed increased emphasis. I quickly discovered that virtually all science department heads viewed teaching as a necessary encumbrance, and wanted someone who would focus almost solely on research with quick and sure payoffs in terms of funding.

I eventually ended up as a researcher at Oak Ridge National Laboratory because I reasoned that if I was going to spend my life doing research, I should not plan to make a living at a university where the necessary encumbrance of teaching would detract from department goals. What struck me was that the reasons Smolin gave for no new Einstein were related to the anti-intellectual attitudes these days, especially toward the applied sciences. Those attitudes lead to a public that is unwilling and intellectually unprepared to accept the overwhelming evidence in favor of evolution. Basically, the quick dollar-payoff is what has been motivating science departments, to the exclusion of anything "risky" such as hiring the "independent and creative thinkers" Smolin mentions, or such long-term and vague payoffs as educating the next generation. Higher education in the US has "sown the wind" and it may be reaping the whirlwind.

T. J. Blasing
(blasingtj@tds.net)
Knoxville, Tennessee

To the Opinion piece by Lee Smolin I would add a note on a related problem with the present system: Editors of the principal journals reject manuscripts that challenge prevailing theories or fall outside mainstream research. This practice eliminates new ideas in fundamental physics and encourages routine articles in established fields. The editors protect themselves from many crackpot submissions, but also from the few potentially great concepts. An organization or journal that screens original articles specifically to identify great ideas would be a valuable asset.

Another part of the equation is that original ideas can come from physicists who, like me, are retired. We no longer have a career to worry about, and may have received graduate training in broader, more fundamental physics. We do not have the pressure of publishing papers. The search for new Einsteins should not be limited, as Smolin suggests, to a few young scientists who are set aside to develop creativity. There are greater numbers of retired scientists,

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many having proven their creativity in diverse fields.

William W. Carter
(wwcarter@wcbr.us)
Charlottesville, Virginia

I have a few things to add to Lee Smolin's reasons why no new Einsteins are coming forth today. Today's scientists are jet-setting, grant-swinging, favor-trading hustlers looking for civil servants who will provide them with a pipeline into the US Treasury. Not only do they get peer pressure to behave this way, they also get arm-twisting from the academic bureaucracy that wants to get its 50% to pay for its bloated overhead. You can't be a used-car salesman and have deep thoughts about the structure of the universe at the same time. You've got to move product—in the case of scientists it's reports and journal publications—and keep moving it even after tenure removes some of the pressure. As for the assorted Beltway Bandits (private industries fulfilling government contract work), some of whom are quite talented, there is no tenure, only the next contract.

Big Al Einstein was not like that. His personal life may have left some things to be desired, but he had professional integrity. Even Ezra Pound had something good to say about him. These days Einstein would be teaching at a third-rate local college in a lower-echelon state university system, if he got an academic position at all. Or he might wind up in a cubicle at some agency that serves as the employer-of-last-resort for physics PhDs. He might even be selling minivans.

One thing I regret about my career at the National Geodetic Survey is that I did not have my hand on the spigot of a pipe leading to the Treasury. Those who did had lots of friends doing them lots of favors, and got to see the world at taxpayers' expense. Everyone else counted the days until retirement.

Foster Morrison
(turtle_hollow@sigmaxi.org)
Turtle Hollow Associates, Inc
Gaithersburg, Maryland

Lee Smolin's Opinion piece is wonderfully exciting and long overdue. His section on creativity and independence should be inspirational to all who believe that a university science program should be more than a sorcerer's apprenticeship.

My only disappointment, however, is his proposal for a source of funding for creative and independent researchers. That proposal misses the point that Einstein's research during his patent-office tenure must have been unfunded. Important questions might include the following: What was Einstein's relationship with his bosses? Did he have to do his research on the sly, as a "weekend problem"? Or did his bosses, like the Medici, encourage or even require that he pursue an independent research program, perhaps because Einstein and his bosses lived in a world as yet unconquered by cost accounting?

If Einstein's bosses were Medicean, then the funding for his research was his patent-office salary. Let's suppose his duties there were the equivalent of a full teaching load. That load would not have been increased as punishment if he had failed to pay for his research from outside sources. Neither would his job have been at risk. Apparently there existed no artificial barrier between teaching (or a teaching equivalency) and research. Until contaminated by federal and corporate dollars this must have been how most research was funded in major universities—and it may be how most research in the humanities is funded today, namely by university administrators who recognize that research is teaching.

Today we have the sorry situation that research must be funded either internally by committee decision or externally. If the researcher fails to pay for his research, then the teaching load (or teaching-equivalency load) is increased or he may lose his job. This state of affairs is accompanied by strong propaganda, to which the young researcher is likely to succumb, that unfunded research, to use the language of sport or business, is not competitive. Other language is used to suggest the worthlessness of unfunded research: It is "personal," or a "hobbyhorse," or a "sandbox."

The concept that research is teaching has vanished from the modern scene. In fact the successful grantee may eventually be coaxed away from research and teaching into administration, which is the apotheosis of all human endeavor whose worth, methods, performance, accomplishments, and raison d'être are beyond the reach of peer review. The highest risk in a research laboratory attaches to the research itself; one should do as little of it as possible and what is done should be supported with infinite protocol, planning, and caution. This requires administration.

It may surprise some to learn that this cost accounting of a researcher's university training and intellectual gift has paradoxically increased that researcher's level of idleness as a scientist. For example, at some of the national laboratories, a PhD-level scientist might be encouraged to occupy what I will call a technical sinecure—a job that is technical but not scientific, one that a person trained at a lower level could perform—in return for certain abstract quantities such as reputation as a scientist and the quality of degree in order to window-dress the laboratory without requiring a commitment to fund any research. Who could possibly take the responsibility for funding research? One obtains a glimpse of the erroneous research philosophy in play here. Anything for pay must be for real work. The quid pro quo is some free time and the use of the facilities to do some "personal" research. The paradox is that the cost of one's full-time equivalent does not buy the use of his or her training and talent in any meaningful way to carry out the mission of the laboratory.

Working for the Medici could also be hard. Giorgio Vasari, a biographer of some of the early Renaissance painters, has told how Lippo Lippi was locked in his room in a Medici palace to complete some pictures but escaped by knotting together his bedclothes and letting himself down to the street. Robert Browning imagines in "Fra Lippo Lippi" that the painter, on returning after a night's entertainment, was detained by the police just steps away from the palace. Lippi says to the police,

I am poor brother Lippo, by your leave!
You need not clap your torches to my face.
And here you catch me at an alley's end
Where sportive ladies leave their doors ajar.
Aha, you know your betters? Then you'll take
Your hand away that's fiddling on my throat,
And please to know me likewise. Who am I?
Why, one, sir, who is lodging with a friend
Three streets off—he's certain . . . how d'ye call?
Master—a . . . Comiso of the Medici.

Burke Ritchie
Livermore, California

The interesting and challenging Opinion column by Lee Smolin is based on two assumptions: first, that it is beneficial and desirable for humanity to have many geniuses; and second, that administrative, bureaucratic, and financial measures could achieve this goal. I disagree with both assumptions.

Physics geniuses such as Galileo, Isaac Newton, James Clerk Maxwell, and Albert Einstein—and also geniuses in other fields—appear at a rate of about one in a century. Nobody can tell what produces a genius; presumably it is a confluence of genetic, physiological, environmental, historical, and societal factors. How could one then suggest means to increase the frequency of genius emergence? Furthermore, for "ordinary" people, and for society, it seems to take a long time to understand, appreciate, and apply the deep insights and teachings of geniuses. Then why should we wish to have more of them, more frequently than nature produces them? Besides, not even geniuses create new knowledge only from within themselves: Rather, a slow, natural, and cooperative process of ripening insight and understanding of a field erupts, through the genius of the inspired person, into a new worldview, or at least a new paradigm.

Smolin's major proposal is the creation of special institutions, independent of academia, that would foster nontraditional-thinking, innovative young scientists in making breakthroughs not envisaged in customary settings. Of course, such centers would need substantial financial backing, and so would the young geniuses-to-be. Thus again much boils down to a pledge of money. But as the author reminds us, and as is commonly known, Einstein did much of his pioneering work when he was not sponsored by any institution or establishment. Even later, he worked more creatively than anybody else at that time, in a traditional (not special) academic group environment. Money and its large-scale dispensation to envisioned goals is no panacea. And "social engineering" never leads to acceptable results anyway.

Smolin says, "This one person [Einstein] did more to advance physics than most of the rest of us put together have since." With utmost respect for and admiration of Einstein, I disagree. While he singlehandedly revolutionized our understanding of space and time, a group of individuals—Werner Heisenberg, Erwin Schrödinger, Paul Dirac, Niels Bohr, Eugene Wigner, and the like—taught us a new picture of matter at least as astounding, and they more generally revolutionized our whole thinking about nature's laws by developing quantum theory.

I also oppose Smolin's claim that foundations of quantum theory are nowadays a neglected field. For the past 30 years, especially inspired by the insights (individual, but not engineered in special centers) of people like John Bell, Eugene Wigner, John A. Wheeler, Hans D. Zeh, Roland Omnès, and very many others, tremendous progress has been achieved in clarifying the foundations, meaning, and interpretation of quantum theory. Those clarifications were verified and confirmed by numerous magnificent experiments.

As a final remark, no statistical evidence supports Smolin's concern that talented, creative, young US physicists are "brain-drained" en masse to other parts of the world. What I see is the continued influx of foreign scientists, not only Asian students who do not return to their mother countries, but also, for example, a stream of distinguished Russian scientists.

Paul Roman
(p.k.roman@web.de)
Ludenhausen, Germany

Lee Smolin gives some very good reasons for the modern lack of Einstein-type scientists. I can add some reasons that exist in Denmark.

Here PhD students are generally treated like employees and are chosen with increasing frequency to perform short, narrowly defined tasks in connection with, for example, projects funded by the European Union. Instead of receiving financial support to study a novel problem under a professor's guidance, PhD students are increasingly left to fill in the blanks on projects that are already well defined; the projects are all laid out in a contract already, it seems, and the students have little room in their schedules for developing individual projects. Some typical EU-funded PhD projects are simply uninspiring and tend to involve programming and computer data-wrangling. These are not unimportant skills, of course, but a PhD study should also include time for creative thinking, especially as the end of school approaches.

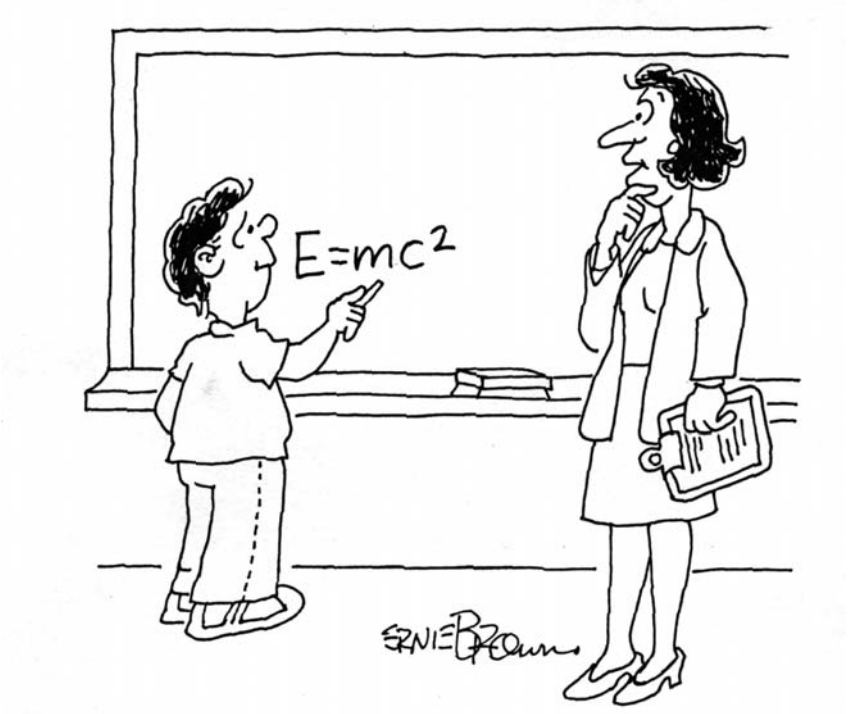
Peter Thejll
(pth@dmi.dk)
Danish Meteorological Institute
Copenhagen, Denmark

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.

Lee Smolin
(lsmolin@perimeterinstitute.ca)
Perimeter Institute for Theoretical Physics
Waterloo, Canada

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.