

Why No 'New Einstein'?

Lee Smolin

I am sure I am not the only one in this year of Einstein who receives calls from journalists asking, "Why is there no new Einstein?" While we have ready answers, there is still the disquieting worry that perhaps a lesson might be learned from the fact that this one person, who was initially unable to find an academic job, did more to advance physics than most of the rest of us put together have since.

Many of Einstein's contemporaries testified that he was not unusually talented mathematically. Instead, what enabled him to make such tremendous advances was a driving need to understand the logic of nature, tied to a breathtaking creativity and a fierce intellectual independence. But Einstein does not stand alone. One can cite many examples showing that big advances in physics come when unusually creative and intellectually independent individuals ask new questions and forge new directions.

It follows that new Einsteins are unlikely to be easily characterized in terms of research programs that have been well explored for decades. Instead a new Einstein will be developing his or her own research program that, by definition, will be one that no senior person works on. He or she may even feel the need to focus on the really deep and difficult questions, such as the foundations of quantum mechanics, that are ignored in leading American research institutes and departments, despite the work of a few leading physicists who had established their credentials in other topics first. After all, if Einstein himself felt the deepest problems were in the foundations of quantum theory, and if those problems remain unsolved half a century later, why should we expect a new Einstein to ignore them?

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Over the past few years many have expressed concern about the challenge to US physics from growing investments abroad, from the European Union to China. I believe our future success in physics will depend on how well we can answer a single question: Are our universities, institutes, and foundations doing all they can to identify and promote individuals who have the creativity and intellectual independence that characterize those who contribute most to physics? I say that they are not. They are not even doing as well as some of their competitors do to support those with the most creativity and independence. Simple changes could greatly improve the extent to which American science is hospitable to very talented physicists.

Creativity and independence

The mechanisms we have constructed to ensure fairness and quality have the unintended side effect of putting people of unusual creativity and independence at a disadvantage.

► Those who follow large well-supported research programs have lots of powerful senior scientists to promote their careers. Those who invent their own research programs usually lack such support and hence are often undervalued and underappreciated.

► People with the uncanny ability to ask new questions or recognize unexamined assumptions, or who are able to take ideas from one field and apply them to another, are often at a disadvantage when the goal is to hire the best person in a given well-established area.

► In the present system, scientists feel lots of pressure to follow established research programs led by powerful senior scientists. Those who choose to follow their own programs understand that their career prospects will be harmed. That there are still those with the courage to go their own way is underappreciated.

► It is easy to write many papers when you continue to apply well-understood techniques. People who develop their own ideas have to work harder for each result, because they

are simultaneously developing new ideas and the techniques to explore them. Hence they often publish fewer papers, and their papers are cited less frequently than those that contribute to something hundreds of people are doing.

To give the advantage to people who are unusually creative and independent, we should change the measures we use to judge quality and promise.

Sometimes it is asserted that more independent and creative thinkers constitute a greater risk in hiring. But I think an examination of the careers of individual physicists shows that on the whole the opposite is true. It is the creative and independent thinkers who are more likely to continue to make important contributions throughout their lifetime. They are driven by their own curiosity and need for understanding, rather than by career motives. Their research is not going to fall off when the techniques they learn in graduate school run out, for they have the ability to invent new ideas and directions and learn new techniques.

We also greatly underestimate the risks of having large numbers of people follow speculative but trendy research programs, even those led by very accomplished senior people.

String theory

Over the past 20 years, string theory has attracted the effort of a large number of theorists and mathematicians. Nevertheless it is clear that the program has not progressed as originally envisioned. Many key conjectures remain unproven, including the basic claim that the theory gives finite answers.¹ The hope for a unique theory and the promise of new falsifiable predictions have dissolved with the discovery of evidence for vast numbers (greater than 10^{300}) of theories. The well-studied versions disagree with experiment, and little is known explicitly about the many versions that are conjectured to agree with observation. Despite much effort, no evidence has been found to confirm the key hypotheses, including hidden

dimensions, grand unification, and supersymmetry. I suspect most practitioners can agree that if string theory is to fulfill its promise, it needs an infusion of new ideas and directions.

The problem is that the kind of people most likely to have such ideas have not recently had easy times making careers, compared to less independent people, who were content to follow the fashion in string theory. Each year, one or two trendy directions came and went, often leaving unsolved problems. Young string theorists feel a lot of pressure to follow the changes, if they are to have the benefit of recommendations from senior people. Several young string theorists have told me they simply have neither the time nor the freedom to ask their own questions or develop their own ideas.

Alternatives to strings

More worrisome, young theorists who pursue alternatives to string theory have had great difficulty finding any academic positions in the US. This is true of those who pursue alternative programs in particle physics, like technicolor and preon models, and also true of those who pursue alternative approaches to quantum gravity, such as dynamical triangulations, causal sets, and loop quantum gravity. These subjects are all pursued much more vigorously outside the US, because leading researchers in these areas are drawn to leave US universities by offers of very good opportunities elsewhere.

One approach barely represented in the US is quantum gravity phenomenology, which studies how to test quantum gravity theories experimentally by means of high-energy astrophysics experiments such as the Gamma Ray Large Area Space Telescope and the Pierre Auger Observatory. The experiments are supported in the US, but most theorists who are developing the relevant phenomenology are outside the US.

Other examples show the hazards of too much concentration of resources on a few areas, to the exclusion of others. For decades, the foundations of quantum mechanics got virtually no support in the US; it was believed to be a direction without promise. In the last 10 years the fast-moving field of quantum information has shown that important experimental and theoretical results about foundations of quantum mechanics were always there for the finding. In other cases, such as LIGO, the concentration of resources on a large project has weakened the ability of NSF to support individual

scientists working on their own approaches to fundamental questions.

Some modest proposals

But my purpose here is not to argue for or against any existing research program. It is to propose that, under the pressure to support programs advanced decades ago by now influential senior scientists, we have forgotten that theoretical physics is most often advanced by people who ignore established research programs to invent their own ideas and forge their own directions. Such people are often, but not always, young people, whose careers are the most vulnerable. If we do not explicitly make room for these kinds of people, they will leave physics or they will continue, as now, to leave the US to do the physics they want to do.

Some other countries seem to be better at making room for the independent thinkers. The UK, through the Royal Society Fellowships, is able to pick very bright mavericks who would not be hired in the US, and give their careers good starts. France picks a small number of very talented young scientists and gives them permanent positions; that security immunizes them to some extent from sociological pressures. Canada has opened the Perimeter Institute, whose specific mandate is to be a home for independent foundational thinkers, and other such projects are in planning stages around the world.

In addition to the importance of selecting individuals over research programs, science as a whole benefits from diverse points of view. When a group of researchers aggressively pursues a research program but has little interaction with either experiment or outsiders, the group tends to overinterpret results, undervalue risks, and complacently postpone facing up to hard questions and negative results. This is groupthink—a well-documented phenomenon in government, intelligence agencies, and business. When it happens in an academic specialty, the fault is not with a scientist who aggressively promotes his or her program. The whole scientific community makes the rules that allow consensus to be established without sufficient evidence.

It is ironic that the US, which rightly encourages racial and gender diversity, worries less about ensuring the creative and intellectual diversity on which the health of science depends. Some obvious recommendations follow from a comparison of practices in the US and elsewhere.

► Young scientists should be hired

and promoted based only on their ability, creativity, and independence, without regard to whether they contribute to any research programs established by older people.

► To prevent overinvestment in speculative directions that may end up as dead ends, departments should ensure that different points of view about unsolved problems, and rival research programs, are represented on their faculties.

► Scientists should be penalized for doing superficial work that ignores hard problems and rewarded for attacking the longstanding open conjectures, even if progress takes many years of hard work. More room could be made for people who think deeply and carefully about the really hard foundational issues.

► Research groups should seek out people who pursue rival approaches, and include them as postdocs, students, and visitors. Conferences in one research program should be encouraged, by those funding them, to invite speakers from rival programs. Instructors should encourage students to learn about competing approaches to unsolved problems, so that the students are equipped to choose for themselves the most promising directions as their careers advance.

► Funding agencies and foundations should take steps to see that at every level scientists are encouraged to freely explore and develop all viable proposals to solve deep and difficult problems. Funding should go to individual scientists for individual thought and not to research programs. A research program should not be allowed to become institutionally dominant until supported by convincing scientific proof of the usual kind. Before such proof is demonstrated, alternative and rival approaches should receive encouragement to ensure that the progress of science is not stalled by overinvestment in a direction that turns out to be wrong.

► A foundation or agency could create a small number of Einstein fellowships, to go specifically to theorists under 40 who invent their own ideas and programs aimed at solving foundational problems in physics. As Einstein told us, to solve such problems requires concentration for years, regardless of fashion, so these fellowships should offer 10 years of support and go only to theorists whose work cannot be categorized as a contribution to an existing approach.

Reference

1. E. D'Hoker, D. H. Phong, *Phys. Lett. B* **529**, 241 (2002). ■