similar question in fields other than physics. In the mathematical arena, one could ask "Why no new Euler?" Of course, Leonhard Euler's name could be replaced by the names of several other great mathematicians, but an argument could be made that Euler shared with Einstein an amazing intuition that, it seems, is a trait of a select few. I believe William Dunham's wonderful book gives insight to Euler's intuition.[1]

It may be the common opinion among modern mathematicians that many of Euler's methods would not stand up to current mathematical rigor. And, as an engineer, I dare not take issue with that. But it seems one reason why no new Euler has arisen is that for scientists and engineers, at least, the flame of intuition too often is extinguished in the very first university mathematics class they take. Certainly mathematical rigor has its place. But an intuitive line of thought that leads to a correct mathematical result ought not to be discouraged, beyond a possible admonition about where such thinking could lead one astray. In fact, intuitive thinking ought to be celebrated, as long as we non-mathematicians do not make any claims to rigor or demand that mathematicians strictly agree with us.

A new Euler would not necessarily emerge from the non-mathematician class, although that possibility cannot be ruled out either. Paul Dirac, Richard Feynman's hero, comes to mind immediately as one who resembled Euler in the way he did some of his mathematics. His book on quantum mechanics shows how he masterfully created a new mathematical formulation in order to do his physics.[2] The mathematicians were left the task of showing that his results could also be proven rigorously. After all, who will argue with one whose non-rigorous mathematics leads to the discovery of a new particle?

References

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2. P. A. M. Dirac, *The Principles of Quantum Mechanics*, 4th ed., Oxford U. Press, New York (2000).

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As far as I can see, the main issue in the discussion of "Why No 'New Einstein'?" is whether increased funding and better organization can produce more Einsteins per century. Lee Smolin holds the positive view, while Paul Roman disagrees.

A possible clue to resolving the issue lies in Lev Landau's classification of outstanding genius physicists, as narrated by his close associate Evgeny Lifshitz at a talk given in 1974 at the Abdus Salam International Centre for Theoretical Physics (ICTP) in Trieste, Italy. According to Landau's classification, Isaac Newton received the highest rank, 0, followed by Albert Einstein at a rank of 0.5, then by Niels Bohr, Werner Heisenberg, Erwin Schrödinger, Paul Dirac, Satyendra Nath Bose, Eugene Wigner, and a few others at 1, and so on. Landau had given himself a modest rank of 2.5. The classification continued to the rank of 5 for mundane physicists.

It is tempting to consider the Smolin–Roman debate in the light of the Landau classification. The principle of better funding and more purposive organization, which is the bedrock of Smolin's thesis, seems to work fairly well for ranks numerically greater than 3, largely on "statistical" grounds. To cite another example, young workers from developing countries, who would usually rank at 4 to 5 on the Landau scale, considerably increase their productivity in the environments of ICTP and CERN, but are not often able to maintain the same tempo on getting back to their home environments. However, the principle's effectiveness tends to decrease rapidly for physicists ranked in the opposite direction. Actually, the critical value of 2.5 is signal enough against the idea that highly talented physicists can be mass produced. Below that value, one should have genuine doubts about the working of Smolin's thesis, which leaves the field open for Roman's counter-thesis to come into play. Indeed, by the time a physicist reaches rank of 1 on the Landau classification, the idea that an organized and structured environment is best for the mass production of talent probably fails altogether.

Let me illustrate with a concrete example from physics the hazards of thinking that talent can be mass produced. After the success of the Glashow-Weinberg-Salam theory of electroweak interactions, serious attempts were made worldwide to generalize the GWS framework so as to also include the strong-interaction sector within its ambit and thus pave the way for a grand unified theory of all three interactions. But Nature did not yield to such preposterous demands to conform to tailor-made theories. The ambition for mass production of Einsteins must contend with such a reality.

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Lee Smolin's response to letter-writer William Carter (PHYSICS TODAY, January 2006, page 16) indicates that he is unaware of the changes that have occurred at the arXiv e-print server. Smolin says, "I do not think the issue of journals is key, now that we have the arXiv e-print server." When the server was at Los Alamos National Laboratory, it was a government-sponsored resource and therefore fairly accessible. Now that it is at Cornell University, any unknown researcher must have the endorsement of a certified endorser to publish a paper.[1] An independent researcher who isn't known to any endorser is simply locked out. And endorsers can lose certification by endorsing readers they know, if the ideas are too unfamiliar. Thus, for an independent researcher with new ideas, the e-print server is no more accessible than the mainstream journals. That's probably why its content as a whole has been so deadly dull lately.

Reference

1. See the arXiv endorsement policy at http://arxiv.org/help/endorsement.

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Smolin replies: My piece in PHYSICS TODAY was a brief summary of arguments made in my new book, *The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next* (Houghton Mifflin, 2006). Carlos Handy and Susan Ramlo echo many communications I have received in response. They tell stories of idealistic and creative young people burning with energy to contribute to physics who collide with a cynical and unsympathetic atmosphere when they enter graduate school. Their comments affirm the message of my essay and book, which is that physics will progress faster if we make sure to hedge our investments in risky foundational areas, and support a diverse range of ideas, research styles, and approaches. According to sociologist Richard Florida's work, a strong measurable correlation exists between economic growth and tolerance, which explains why cities like San Francisco and Toronto are prospering. My argument is an application of his insight to the physics profession.

Thus, although I agree with the thrust of Amin Dharamsi's remarks, I differ with him on attributing Albert Einstein's success to his "amazing intuition" alone. Einstein contributed because he held two convictions about nature that turned out to be right: the

relativity of inertial frames and atomism. Those were not consensus views at the time, so to pursue them he had to risk his career; and for that he needed a strong dose of intellectual independence and courage. Had he been as talented and intuitive but less independent, he might have been able to contribute to the development of existing ideas, following the great physicists of the time such as Max Planck and Hendrik Lorentz. But then he might not be remembered today and physics would be poorer for it.

On the other hand, Planck and Lorentz are to be admired because even if they were on the wrong side of the key issues, they recognized the importance of Einstein's work and encouraged and supported his career. They cared more for science than for their own legacies and research programs, so they put their support behind the young rebel whose work, it turned out, ended their own research programs. This shows that the contributions of Einstein, Lorentz, and Planck were due as much to their characters as to their cognitive and computational skills.

This illustrates why I disagree with Lev Landau's simplistic but common notion, raised by Asoke Mitra, that physicists can be ranked in a one-dimensional hierarchy. My view, supported by everything I have experienced in science, is that this status game is both wrong and destructive to the progress of science because the progress of physics requires a diversity of talents, approaches, and styles.

Let me offer a better metaphor than the schoolyard for thinking about how physicists differ—a metaphor suggested to me by Eric Weinstein. We can think of physicists as mountain climbers, with the new theory we are looking for as a high peak in the distance. Unfortunately, the landscape is foggy and we climbers can only see far enough to tell which direction is up from where we are. To discover the peak requires different kinds of climbers. At some points we need good technical climbers. Put them on any slope and they will make it quickly to the nearest hilltop. Many of them also like to climb in groups, so that they have an audience to whom they can show off their skills. The problem is that once they get to the top, they get stuck. To find other hills, which may lead to the real summit, we need climbers whose styles are more adventurous and individualistic, who prefer to leave the crowded lower peaks and strike off across perilous ridges. We also need a few loners who prefer to spend their time fording rivers and crossing valleys, discovering new mountains.

Einstein may have been the best valley crosser we've ever had; almost everything he did either sparked a revolution or was an attempt to do so. But contemporaries reported that many people were better at the technicalities. Landau worked in a different period, when the revolution was considered over and what was admired was great speed and technical climbing skills, based on established frameworks. Could Einstein have competed with Landau at what Landau did best? The evidence is that Einstein was not even good at working out the implications of his own theory of general relativity; most of the important exact solutions, which require only elementary methods from differential equations to discover, were quickly found by others.

So Mitra misunderstands my proposal. It is not to mass-produce prodigies. It is to find and support more valley crossers, who have trouble making good careers in an atmosphere that promotes great technical climbing skills as indicative of a good scientist. This does not require a big change in policy, as not many people qualify as valley crossers. What is needed is only that some agencies and foundations learn to act as venture capitalists, to give those who take the big risks needed to solve the big problems a chance to do their work.

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Teaching physics mysteries versus pseudoscience

Physicists properly join today's arguments involving the teaching of Darwinian evolution. There is, however, a social issue closer to the responsibility of physicists: Quantum physics is increasingly invoked to promote pseudoscience.

Such promotions may start with correct statements of the intriguing implications of quantum mechanics, move to legitimate hyperbole, and then go off into complete hype. Take a recent "international hit" movie as our case in point. It's strangely titled *What tHe #$*! Do wΣ (k)πow!? (What the Bleep Do We Know!?)* An article in *Time* magazine described it as "an odd hybrid of science documentary and spiritual revelation featuring a Greek chorus of PhDs