letters

Expanding the record on Einstein and peer review

A few comments come to mind in response to Daniel Kennefick's beautiful piece of scholarship on Einstein and peer review (PHYSICS TODAY, September 2005, page 43).

At the end of the 1936 Einstein–Rosen paper as it was finally published in the *Journal of the Franklin Institute*, there is the following note:

> The second part of this paper was considerably altered by me after the departure of Mr. Rosen for Russia since we had originally interpreted our formula results erroneously. I wish to thank my colleague Professor Robertson for his friendly assistance in the clarification of the original error. I thank also Mr. Hoffmann for kind assistance in translation. A. Einstein

Dan Kennefick clearly knows this; he had sent me a copy of the article. By present American Physical Society ethical standards (see http://www.aps.org/statements/02_2.cfm), Howard Percy Robertson would have been entitled to coauthorship, since he had made a substantial contribution to the interpretation of the paper as it finally appeared. Of course, standards of the time were quite different from those we have today.

It is worth quoting the version of this story that appeared in Abraham Pais's admirable scientific biography,[1] which Kennefick also cites (his reference 2). In his notes on Einstein's collaborators, Pais includes the following about Nathan Rosen:

> In the course of working on this last problem [cylindrical gravitational waves] Einstein believed for some time that he had shown that the rigorous relativistic field equations do not allow for the existence of gravitational waves. *After he found the mistake in the argument, the final manuscript was prepared and sent to the* Physical Review [emphasis mine]. It was returned to him accompanied by a lengthy referee report in which clarifications were requested. Einstein was enraged and wrote to the editor that he objected to his paper being shown to colleagues prior to publication. The editor courteously replied that refereeing was a procedure generally applied to all papers submitted to his journal, adding that he regretted that Einstein may not have been aware of this custom. Einstein sent the paper to the *Journal of the Franklin Institute* and, apart from one brief note of rebuttal, never published in the *Physical Review* again.

This account clearly contradicts Kennefick's article on the timing and the details of the discovery of the error, and it contradicts Einstein's own note at the end of the 1936 paper. Of course, Rosen was in Russia while this was happening. It is likely that Robertson never revealed to Einstein his role as the referee, or Einstein might not have continued to boycott the *Physical Review*. And although he did not publish in that journal again, he did send several articles to *Reviews of Modern Physics*—under the same editor, John T. Tate—which also published, in 1949, an entire issue dedicated to Einstein on his 70th birthday.

Reference

1. A. Pais, *"Subtle Is the Lord . . .": The Science and the Life of Albert Einstein*, Oxford U. Press, New York (1982), p. 494.

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In his fascinating article "Einstein Versus the *Physical Review*," Daniel Kennefick notes that "the gravitational wave paper [1936] was Einstein's first encounter with the anonymous peer-review system practiced in American journals at that time." Kennefick says two of Albert Einstein's previous *Physical Review* papers had not been refereed.

As a beginning graduate student at the University of Wisconsin, I was greatly pleased that my professor, Heinz Barschall, added my name as an author to a paper he submitted to the *Physical Review* in 1948; it was my first paper as an author. I had provided only minor assistance in taking the data and operating the accelerator we used in the experiment, and Heinz's generous gesture was important to me. With so little experience, I was not surprised that Heinz was quite annoyed when the paper was returned with a referee's comments, even though they were not unfavorable. He explained that the *Physical Review* had a regular policy of accepting papers from recognized physicists at well-known institutions without submission to a referee and that he never before had had a paper sent to a referee. That included a 1938 theoretical paper he had written with John Wheeler, which presented what was perhaps the first solid evidence of very strong spin–orbit forces in nuclei.[1]

Editor John Tate's submission of the Einstein–Rosen paper to a referee might therefore have reasonably been seen by Einstein as unusual and even disrespectful. As a one-time editor myself (*Physical Review Letters*, 1978–84), I know that being correct, and even helpful, as Tate certainly was, does not defuse an author's objection to an editor's criticism.

Reference

1. H. Barschall, J. Wheeler, *Phys. Rev.* **58**, 682 (1938).

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Daniel Kennefick's interesting article states, "With Nathan Rosen, his first American assistant, Einstein published two more papers in the *Physical Review*: the famous 1935 paper by Einstein, Boris Podolsky, and Rosen (EPR) and a 1936 paper that introduced the concept of the Einstein–Rosen bridge, nowadays better known as a wormhole." To my knowledge, that information is not

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complete. Einstein and Rosen published another, earlier paper, "The Particle Problem in the General Theory of Relativity."[1]

Reference

1. A. Einstein, N. Rosen, *Phys. Rev.* **58**, 73 (1935).

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On the merits of mathematical models

I offer a few comments regarding "Is Economics the Next Physical Science?" by J. Doyne Farmer, Martin Shubik, and Eric Smith in the September 2005 issue of PHYSICS TODAY (page 37). Physicists have deployed mathematical models of interacting entities for two purposes: to establish the existence and properties of such entities—for example, quarks and other subatomic particles—by comparing precise calculations with precise measurements; and to predict and understand the properties of systems, such as doped semiconductors comprising known entities. To imagine an analogous research program for econophysics is to see at once how little can be expected from this field.

Human beings are unique, purposeful individuals whose properties cannot be captured in a few numerical parameters the way one specifies the properties of atoms and particles. It is implausible that we will add to our understanding either of human nature or of market economies by treating people as mathematical Tinkertoys. Yet that is precisely what econophysics appears to have in common with mainstream neoclassical economics.

There exists an older, more modest economic tradition, one that explores the implications of individual human choice in the pursuit of individual goals. The focus is not on solving equations but on reasoning directly about the actions of human beings as we know them through introspection and common experience. This approach offers a rich and fruitful insight that extends beyond the problems ordinarily regarded as "economic." As I have demonstrated elsewhere,[1] it can explain much about the conduct and dissemination of scientific research itself.

Farmer and coauthors seem puzzled by the weakness of price predictability, given that there are long-memory correlations among closely related data in the stock market. May I suggest an explanation? Price predictability offers profit opportunities, which people are likely to discover and exploit. In general, however, to exploit profit opportunities is to eliminate them. We can expect, therefore, that price predictability will be limited by profit-taking market speculators to a level at which only the most astute of them earn enough to stay in the game. There is no particular reason to expect other correlations in market data to be so limited, if they do not offer direct profit opportunities.

I had a look at the University of Fribourg website recommended by the authors. Some econophysicists seem to think that by criticizing mainstream economics, or by referring dismissively to "Adam Smith's invisible hand," they are also undermining the case for free markets. On the contrary, the mainstream's preoccupation with mathematical models of equilibrium obscures the market's dynamism and underlies counterproductive anti-trust legislation. At one time, uncritical faith in mathematical modeling lent false plausibility to the notion that one might in fact measure the parameters, solve the equations, and thereby centrally control an economy. Mainstream economists eventually shook off that pipe dream. Econophysicists would do well to avoid it.

Reference

1. A. Walstad, *Perspect. Sci.* **9**, 324 (2001); *Independent Rev.* **7**, 5 (2002).

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The article on economics and physics by Doyne Farmer, Martin Shubik, and Eric Smith reviews several interesting studies for senior- or graduate-level physics students who are well grounded in statistics. A much simpler study, one that I think would be informative and suitable for freshman physics and economics students, would be a measure of the operational efficiency of a country.

Other economic systems might also offer appeal, such as a comparison of the operational efficiencies of farms. Selecting a study on countries, however, has the advantage that the data for the total annual input energy consumed in a country are readily available from government websites.