Yes, Economics is a Science

Raj Chetty, Harvard University
NY Times, 20 October 2013

There's an old lament about my profession: if you ask three economists a question, you'll get three different answers. This saying came to mind last week, when the Nobel Memorial Prize in Economic Science was awarded to three economists, two of whom, Bob Shiller of Yale and Gene Fama of the University of Chicago, might be seen as having conflicting views about the workings of financial markets. At first blush, Shiller's thinking about the role of "irrational exuberance" in stock markets and housing markets appears to contradict Fama's work showing that such markets efficiently incorporate news into prices.

What kind of science, people wondered, bestows its most distinguished honour on scholars with opposing ideas? "They should make these politically balanced awards in physics, chemistry and medicine, too," the Duke sociologist Kieran Healy wrote sardonically on Twitter. But the headline-grabbing differences between the findings of these Nobel laureates are less significant than the profound agreement in their scientific approach to economic questions, which is characterized by formulating and testing precise hypotheses. I'm troubled by the sense among skeptics that disagreements about the answers to certain questions suggest that economics is a confused discipline, a fake science whose findings cannot be a useful basis for making policy decisions. That view is unfair and uninformed. It makes demands on economics that are not made of other empirical disciplines, like medicine, and it ignores an emerging body of work, building on the scientific approach of last week's winners, that is transforming economics into a field firmly grounded in fact.

It is true that the answers to many "big picture" macroeconomic questions — like the causes of recessions or the determinants of growth — remain elusive. But in this respect, the challenges faced by economists are no different from those encountered in medicine and public health. Health researchers have worked for more than a century to understand the "big picture" questions of how diet and lifestyle affect health and aging, yet they still do not have a full scientific understanding of these connections. Some studies tell us to consume more coffee, wine and chocolate; others recommend the opposite. But few people would argue that medicine should not be approached as a science or that doctors should not make decisions based on the best available evidence. As is the case with epidemiologists, the fundamental challenge faced by economists — and a root cause of many disagreements in the field — is our limited ability to run experiments. If we could randomize policy decisions and then observe what happens to the economy and people's lives, we would be able to get a precise understanding of how the economy works and how to improve policy. But the practical and ethical costs of such experiments preclude this sort of approach. (Surely we don't want to create more financial crises just to understand how they work.)

Nonetheless, economists have recently begun to overcome these challenges by developing tools that approximate scientific experiments to obtain compelling answers to specific policy questions. In previous decades the most prominent economists were typically theorists like Paul Krugman and Janet Yellen, whose models continue to guide economic thinking. Today, the most prominent economists are often empiricists like David Card of the University of California, Berkeley, and Esther Duflo of MIT, who focus on testing old theories and formulating new ones that fit the evidence. This kind of empirical work in economics might be compared to the "micro" advances in medicine (like research on therapies for heart disease) that have contributed enormously to increasing longevity and quality of life, even as the "macro" questions of the determinants of health remain contested.

Consider the politically charged question of whether extending unemployment benefits increases unemployment rates by reducing workers' incentives to return to work. Nearly a dozen economic studies have analyzed this question by comparing unemployment rates in states that have extended unemployment benefits with those in states that do not. These studies approximate medical experiments in which some groups receive a treatment — in this case, extended unemployment benefits — while "control" groups don't. These studies have uniformly found that a 10-week extension in unemployment benefits raises the average amount of time people spend out of work by at most one week. This simple, unassailable finding implies that policy makers can extend unemployment benefits to provide assistance to those out of work without substantially increasing unemployment rates.

Other economic studies have taken advantage of the constraints inherent in a particular policy to obtain scientific evidence. *An excellent recent example concerned health insurance. In 2008, the state of Oregon decided to expand its state health insurance program to cover additional low-income individuals, but it had funding to cover only a small fraction of the eligible families. In collaboration with economics researchers, the state designed a lottery procedure by which individuals who received the insurance could be compared with those who did not, creating in effect a first-rate randomized experiment. The study found that getting insurance coverage increased the use of health care, reduced financial strain and improved well-being — results that now provide invaluable guidance in understanding what we should expect from the Affordable Care Act.*

Even when such experiments are unfeasible, there are ways to use "big data" to help answer policy questions. In a study that I conducted with two colleagues, we analyzed the impacts of high-quality elementary school teachers on their students' outcomes as adults. You might think that it would be nearly impossible to isolate the causal effect of a third-grade teacher while accounting for all the other factors that affect a child's life outcomes. Yet we were able to develop methods to identify the causal effect of teachers by comparing students in consecutive cohorts within a school. Suppose, for example, that an excellent teacher taught third grade in a given school in 1995 but then went on maternity leave in 1996. Since the teacher's maternity leave is essentially a random event, by comparing the outcomes of students who happened to reach third grade in 1995 versus 1996, we are able to isolate the causal effect of teacher quality on students' outcomes. Using a data set with anonymous records on 2.5 million students, we found that high-quality teachers significantly improved their students' performance on standardized tests and, more important, increased their earnings and college attendance rates, and reduced their risk of teenage pregnancy. These findings — which have since been replicated in other school districts — provide policy makers with guidance on how to measure and improve teacher quality.

These examples are not anomalous. And as the availability of data increases, economics will continue to become a more empirical, scientific field. In the meantime, it is simplistic and irresponsible to use disagreements among economists on a handful of difficult questions as an excuse to ignore the field's many topics of consensus and its ability to inform policy decisions on the basis of evidence instead of ideology.

Maybe Economics is a Science, But Many Economists Are Not Scientists

Paul Krugman, Princeton University
NY Times, 21 October 2013

Raj Chetty stands up valiantly for the honour of his and my profession, arguing that economics is too a science in which careful research is used to falsify some hypotheses and lend credibility to others. And in many ways I agree: there is a lot of good research in economics, maybe more than ever as the focus has shifted somewhat from theoretical models loosely inspired by observation — which, as he suggests, was my forte — to nitty-gritty empirical work. But while there are clearly scientific elements in economics, a lot of economists aren't behaving like scientists.

Look at Chetty's examples of scientific work that informs current policy debates [refer to the italicised section above]. OK, he's right that these two examples show how evidence could be used to inform policy debate (although understanding the effects of unemployment insurance, I would argue, requires embedding it in a macro story about how the number of jobs is determined.) But are such results actually being used to inform policy debate? Have conservative economists like Casey Mulligan said "OK, we were wrong to argue that extended unemployment benefits are the cause of high unemployment"? Have economists who oppose Obamacare said, "OK, we were wrong to say that Medicaid hurts its recipients?" You know the answer.

And it's not just policy debates. Whole subfields of economics, notably but not only business-cycle macro, have spent decades chasing their own tails because too many economists refuse to accept empirical evidence that rejects their approach. The point is that while Chetty is right that economics can be and sometimes is a scientific field in the sense that theories are testable and there are researchers doing the testing, all too many economists treat their field as a form of theology instead.

Eugene Fama: Efficient Markets, Risk Premiums, and the Nobel Prize

John Cochrane,
The Grumpy Economist, 7 November 2013

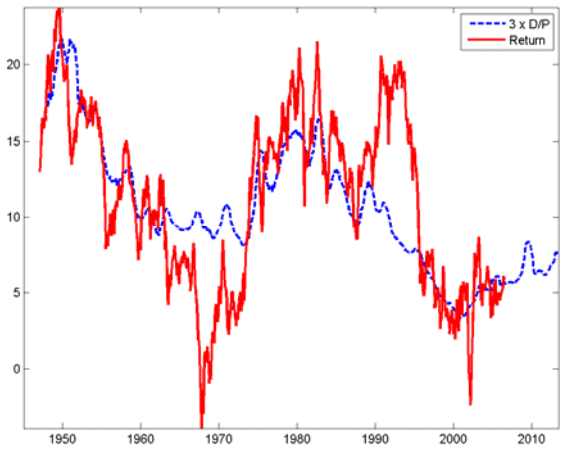
In 1970, Gene Fama defined a market to be "informationally efficient" if prices at each moment incorporate available information about future values: *"A market in which prices always `fully reflect' available information is called `efficient.'"* If there is a signal that future values will be high, competitive traders will try to buy. They bid prices up, until prices reflect the new information. "Efficient markets" just says that prices in a competitive asset market should not be predictable. "Efficient markets" is not a complex theory. Think Darwin, not Einstein. Efficiency is a simple principle, like evolution by natural selection, which organizes and gives purpose to a vast empirical project.

That empirical work is not easy. The efficient market hypothesis has many subtle implications, most of them counterintuitive to practitioners, especially those who are selling you something. For example, efficiency implies that trading rules -- "buy when the market went up yesterday"-- should not work. The surprising result is that, when examined scientifically, trading rules, technical systems, market newsletters, and so on have essentially no power beyond that of luck to forecast stock prices. This is not a theorem, an axiom, a philosophy, or a religion: it is an empirical prediction that could easily have come out the other way, and sometimes does. Efficiency implies that professional managers should do no better than monkeys with darts. This prediction too bears out in the data. It too could have come out the other way. It should have come out the other way! In any other field of human endeavour, seasoned professionals systematically outperform amateurs. But other fields are not as ruthlessly competitive as financial markets. 43 years later, "efficiency" remains contentious.

Some of that contention reflects a simple misunderstanding of what social scientists do. What about Warren Buffet? What about Joe here, who predicted the market crash in his blog? Well, "data" is not the plural of "anecdote." These are no more useful questions to social science than "how did Grandpa get to be so old even though he smokes" is to medicine. Empirical finance looks at all the managers, and all their predictions, tries to separate luck from ex-ante measures of skill, and collects clean data. Another part of that contention reflects simple ignorance of the definition of informational "efficiency." Every field of scholarly research develops a technical terminology, often appropriating common words. But people who don't know those definitions can say and write nonsense about the academic work.

An informationally-efficient market can suffer economically inefficient runs and crashes -- so long as those crashes are not predictable. An informationally efficient market can have very badly regulated banks. People who say "the crash proves markets are inefficient" or "efficient market finance is junk, you did not foresee the crash" just don't know what the word "efficiency" means. The main prediction of efficient markets is exactly that price movements should be unpredictable! Steady profits without risk would be a clear rejection. I once told a reporter that I thought markets were pretty "efficient." He quoted me as saying that markets are "self-regulating." Sadly, even famous academics say things like this all the time. There is a fascinating story here, worth study by historians and philosophers of science and its rhetoric. What would have happened had Gene used another word? What if he had called it the "reflective" markets hypothesis, that prices "reflect" information? Would we still be arguing at all?

Starting in the mid 1970s, Gene started looking at long-run return forecasts. Lo and behold, you can forecast stock returns at long horizons.



The blue line is the ratio of dividends to prices. Think of it as prices upside down. It goes down in the big price booms, such as the 1960s and 1990s, and goes up in the big busts such as the 1970s. It also wiggles with business cycles. You see the astounding volatility of stock valuations, which Bob Shiller shares the Nobel Prize for pointing out. The red line is the average return for the 7 following years. So, times of high prices, relative to dividends are reliably followed by 7 years of low returns. Times of low prices are reliably followed by high returns. This pattern is pervasive across markets – stocks, bonds, foreign exchange, real-estate. Even more surprising are the dogs that don't bark: Times of high prices are not followed by higher dividends, earnings or profits. Does this fact imply that markets are inefficient? No: *"The theory only has empirical content, however, within the context of a more specific model of market equilibrium,…"*

Gene's 1970 article emphasized that you can get better returns, by shouldering more risk, and the reward for bearing risk can vary over time and across assets, and that's how he interprets these facts. *Discounted* prices should be unpredictable. So how you measure discount rates is crucial. For example, in December 2008, prices fell and expected stock returns rose. In this view, typical investors answered: "Yes, I see it's a bit of a buying opportunity. But stocks are still risky, and the economy is falling to pieces. I just can't take risks right now. I'm selling." Many university endowments did just that.

The facts still imply a huge revision of our world view: Business-cycle related variations in the risk premium, rather than variation in expected cashflows, account entirely for the volatility of stock valuations. This view changes everything we do in finance and related fields from accounting to macroeconomics. There is another possibility: perhaps people were irrationally optimistic in the booms, and irrationally pessimistic in the busts. And a third more recent challenge: perhaps the institutional mechanics of financial intermediation cause variation in the risk premium. When leveraged hedge funds lose money, they sell. If not enough buyers are around, prices fall. These views agree on the facts so far. So how do we tell them apart? Answer: we need "models of market equilibrium." We are not here to tell stories. We need economic models, psychological models, or institutional models, that tie price fluctuations to more facts, in a non-tautological way. And, that is exactly what a generation of researchers like myself spend a lot of its time doing, a sure sign of how influential these facts are.

Financial economics is a live field, asking all sorts of interesting and important questions. Is the finance industry too large or too small? Why do people continue to pay active managers so much? What accounts for the monstrous amount of trading? How is it, exactly, that information becomes reflected in prices through the trading process? Do millisecond traders help or hurt? How prevalent

are runs? Are banks regulated correctly? The ideas, facts and empirical methods of informational efficiency continue to guide these important investigations.

Gene's bottom line is always: Look at the facts. Collect the data. Test the theory. Every time we look, the world surprises us totally. And it will again.

Abolish the Nobel Prize for Economics

Dr Oliver Hartwich, The New Zealand Initiative
National Business Review, 25 October 2013

The economics Nobel Prize is a funny thing. Not just because it does not exist. When Alfred Nobel donated his fortune to the awards in his name, he included Physics, Chemistry, Literature, Peace and Medicine. Economics, meanwhile, only got included when the Swedish central bank established the prize "in memory of Alfred Nobel" in the late 1960s. Since then, the economics Nobel Prize has been almost as controversial as the Peace prize. The reason is simple. Whereas in Physics, Chemistry and Medicine, the scientific relevance of a discovery or an invention can be demonstrated relatively easily, especially with the typical benefit of a few decades of hindsight, economic theory does not advance in a similar fashion.

In the history of economic thought, there have been few genuine eureka moments. Adam Smith's understanding of the division of labour comes to mind (1776). David Ricardo's explanation of comparative advantage is another such breakthrough (1817), as was the joint but independent discovery of marginal utility by William Jevons, Carl Menger and Léon Walras (1871-74). Since these glorious early days of economics with their path-breaking insights, it is fair to say that the refinement of economics has not produced similar advances in knowledge. In an odd kind of way, at least this corresponds to the law of diminishing returns. In production processes, you typically realise the biggest output increases in the beginning. Why should it be any different in the production of knowledge? However, the real tragedy for economics is not with the production of new economic knowledge. Unfortunately, we often discard and abandon the things we have learnt in the past. As Friedrich Hayek, himself a Nobel laureate, once said in a lecture, "in economics you can never establish a truth once and for all but have always to convince every generation anew." Instead, Hayek believed that "knowledge once gained and spread is often not disproved, but simply lost or forgotten."

The challenge for economists is often not so much the design of a grand new theory but to keep alive the ideas of great past economists and apply them to new policy challenges. That is worthwhile, no doubt about that, but is it really prize worthy? The other main problem with the economics Nobel is that there remains a substantial amount of disagreement within the economics profession. The prize committee then has to do something that economists typically try to avoid: pick winners. In their past decisions, it is obvious how uncomfortable the committee often were with their choices. We can tell because they often hedged their bets, which leads to the bizarre outcome that in economics it is possible to share a Nobel Prize for saying opposite things. It is unthinkable in the natural sciences to give a prize to two scientists whose research led them to diametrically opposed conclusions. Not so in economics, where we have become used to it. Thus Hayek, an arch-liberal, had to share his prize with Swedish socialist Gunnar Myrdal in 1974. For the same reason, this year's prize is shared by Eugene Fama and Robert Shiller. The former explained why market prices typically reflect all available information; the latter why markets tend to exaggerations and bubbles.

The problems with the economics Nobel Prize unfortunately do not end with such ambiguity. The prize is also notorious for being awarded to people whose subsequent behaviour may be embarrassing. Myron Scholes, the 1997 winner, is the perfect example. Scholes may have produced great work in the field of financial markets theory. However, his personal record in the markets will forever be overshadowed by the failure of his hedge fund Long-Term Capital Management (LTCM). A year after Scholes had won the prize, LTCM collapsed, losing $4.6b of investment. In a similar way, one may well feel that Paul Krugman may have deserved his 2008 Nobel for his contributions to trade

theory. Whether Krugman's New York Times column and political activism would warrant similar accolades is a matter for debate.

The prize is not only controversial for the people who have received it but also for those who have not. For example, the foundations of public choice theory were jointly laid by James Buchanan and Gordon Tullock, not least through their joint book The Calculus of Consent: Logical Foundations of Constitutional Democracy. But only Buchanan won the Nobel, presumably because Tullock had always been the more outspoken, or shall we say politically incorrect, part of the couple.

Finally, the prize has become an annual occasion to test one's professional self-esteem. Having waited with bated breath for the winner, it all too often happened that I and some well-informed economist colleagues scratched our heads after the announcement. Who is Lloyd Shapley? Or Christopher Pissarides? Or Reinhard Selten? Shortly after this moment of embarrassment, one then wonders what happened to one's favourite economists who once again missed out. Say, Alberto Alesina for his work between macroeconomics and politics, Vito Tanzi for his analysis of long-term government spending trends, Richard Epstein for his work in law and economics or Israel Kirzner for his theory of entrepreneurship and his development of Austrian school methodology.

No matter how you look at it, the economics Nobel Prize does not make much sense. Yet it is hard to abolish something that does not really exist, so here are a few recommendations on how this odd prize could be reformed:

- Award it posthumously for the great discoveries in economic science, starting with Adam Smith (if the committee can't agree on him, Smith could share the prize with Karl Marx);
- Give the prize to politicians who demonstrate a rudimentary understanding of basic economics (hard to find – we may have to turn it into a biannual or even rarer prize);
- Turn it into a specific challenge, say a prize for the economist who shows a way to revive the stuck WTO trade talks; and
- Combine it with the literature award (so columnists like Paul Krugman still stand a chance).

If none of this works, the Riksbank might award the prize to itself every year – for keeping Sweden out of the eurozone.