

A Kuhnian Perspective on Microeconomic Theory
or
How To Study Economics

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Introduction

What is “truth” in science? For theory, the question is both facile and central: theory is “true” if we do our math accurately, since it consists of drawing conclusions from a set of premises. But we are not mathematicians; what we really want is to understand the behavior of individuals and of social institutions. Now truth, as Pontius Pilate observed two millennia ago, is ambiguous. In our case, we will focus on one reason for this: science is *not* an objective undertaking, but rather *science is a social activity centered in a community of scholars working to solve puzzles*. Using this definition helps us understand the limitations of economics (and other sciences and more generally, intellectual endeavors). Equally important, this leads to a focus on the apprenticeship system through which a budding economist is initiated into the confraternity of practitioners. Doing so also provides useful guidance on how to go about learning economics.

To develop these ideas I draw upon the work of the late Thomas Kuhn, a historian and philosopher of science.¹ Let us start with the received textbook definition. To cite the “History of Science” entry in the *Encyclopedia Britannica*, “Science, then, is to be considered in this article as knowledge of natural regularities that is subjected to some degree of skeptical rigour and explained by rational causes.” At its core, it uses models, deliberate simplifications of reality, to help us sort our observations of the world around us, indeed to tell us what we should observe and what we should ignore.² It is also used to explain observations, and to predict behavior in applied problems, with the typical distinction in economics of “positive” predictions and “normative” prescriptions.

A second key element is that our knowledge of the world around us is not static; it has improved demonstrably over time. Science is progressive. How does it do this? The received wisdom is that of our textbook description of the “scientific method,” which is the core of what we should perhaps call “normal” science. It typically posits a set of steps along the following lines:

Figure 1: Stereotypical Presentation of "the" Scientific Method

- a *hypothesis* is formed
 - this is formalized
 - an experiment is designed
 - data are collected
- the results are analyzed. *testing* the hypothesis
 - controls are checked
 - this leads us to find support for the hypothesis and provisionally accept it, or to reject it, or to find weak evidence.
- in the latter two cases we *iterate*, to wit:
 - the hypothesis is modified (implicitly shifting the content of the underlying theory on its margin)
 - or*
 - when we have strong confidence in the underlying theory, the experiment is modified to generate different sorts of data, or the theory is extended in a search for additional implications that can then be tested.

Now we economists are in general unable to undertake controlled experiments, because no one will let us (thank goodness!?), a bias that is reinforced by our own professional scruples. There are exceptions, in the new field of experimental economics, which borrows laboratory methods from psychology, using for example the sorts of games we undertake in Economics 210. But in general, we have to rely upon data generated by “nature” – changes over time, or variation across individuals or regions or countries, including those generated by changes in laws and policies. Furthermore, we economists are seldom provided with enough financing to collect our own data (nor are we trained in survey techniques and so on). So most economists make do with existing data sets collected for other purposes, such as census data or price data collected by the government or by businesses. With luck this lets us pinpoint consistent correlations in the real world, but little more. Establishing causation is hard, and it is easy to fall into the trap of using *post hoc, ergo propter hoc*³ reasoning.

OK, so economics does not work quite the same way that physics does. Isn't that so obvious as not to merit mention? Perhaps. But the key point in what follows is that *physics doesn't work this way, either*. Certainly “normal” science is useful in its own way, for pinning down the value of a parameter or confirming that, yes, uneducated peasants in Africa prove to be very good at managing the complicated cocktail of antiviral AIDS medications when they know their life depends upon it. But you won't get a Nobel Prize for such work.⁴ The incremental adjustment of existing models is *not* how science progresses. Nor is it how science is learned, and to be honest with ourselves, it is not how science is done.⁵

Kuhn's Example

One trivial aspect is that normal science, as described above, is primarily a deductive undertaking, going from an existing theory to a hypothesis or to related empirical observations, such as the estimation of model parameters. Major advances in science, however, are inductive in nature, working from (anomalous) data and existing theories (perhaps from ancillary fields) to come up with a novel twist. The work of Newton, or of Copernicus – or in economics, of John Maynard Keynes or Milton Friedman – did *not* rely upon the incremental, deductive procedures of “the” scientific method.⁶

Rather than use examples from economics, which would typically require that you first learn a block of theory in order to follow the argument, I instead follow Thomas Kuhn and turn to the intuitively obvious example of the Copernican highlights this. Kuhn wanted to understand why the sun-centered view of the solar system was so slow to displace its earth-centered predecessor. As every contemporary scientist knew, the received geocentric Ptolemaic astronomy was unable to explain the motion of the planets without introducing *ad hoc* features to an otherwise circle-based system. Even these didn't work well, and so the earth-centered model was unsatisfying as a theory that would allow calendars to be improved – after all, fixing calendars was *the* central task of astronomers, not cosmological speculation.⁷ What Kuhn stressed was that Copernicus' system suffered from similar problems: it too posited circles, and it too failed to fit empirical data. It pointed toward the solution of some puzzles, such as why Mercury and Venus never moved far from the sun, so there were strong arguments in its favor on conceptual grounds. But on empirical grounds, it didn't lead to improved calendars, and there was no necessity to abandon the Ptolemaic model for a Copernican one.

Yet his ideas, first published in 1543, continued to circulate, if not convince. Tycho Brahe's (1546-1601) stupendous observational enterprise provide improved data, though Brahe was not a full Copernican. But his assistant, Johannes Kepler, employed Brahe's data and a metaphysics of the harmony of the spheres

Figure 2: Chronology of the Copernican Revolution

- Aristotle (384-322 BC): systematizer of classical science, into which the astronomy of Ptolemy (127-151 AD) was incorporated, rather than the heliocentric views of the 4th century BC Greek, Heracleides.
- Copernicus (d. 1543): late medieval cleric – doctor, lawyer, astronomer, whose final text was influential for its mathematical framework even if its cosmological claims were ignored.
- Tycho Brahe (d. 1601): Danish observational astronomer, who used better instruments and years of systematic observation to qualitatively improve the data on planetary motion. He proposed a mix of the heliocentric and geocentric models.
- Johann Kepler (1571-1630): Brahe’s final assistant, applied a neo-Pythagorean metaphysics of “harmony” while correctly positing in 1609 a Copernican solar system with elliptical orbits.
- Galileo (1564-1642) with his invention of the telescope around 1610 added observations of Jovian moons and the phases of Venus to provide the first independent evidence of orbiting bodies. But his heresy trial in 1633 made adherence to a geocentric view dangerous.
- Isaac Newton, whose theory of gravity and of motion provided a framework that unified many areas of science; his work on planetary motion dates to 1684-85, and was published in 1687.
- Fontenelle was the first “popular” proponent of the new heliocentric synthesis, through a series of publications in during 1700-1730.

(and *not* some rational scientific method) to model a solar system of elliptical, heliocentric orbits. This combination of theory and data resolved almost all the empirical problems of previous astronomical models, and finally offered the possibility of improved calendars. Meanwhile, Galileo’s early telescope (1609-1610) revealed 4 moons orbiting Jupiter, and that Venus had phases just like our moon, which should have clinched the argument. But just as his eyesight was more acute than normal, so was his pen sharper, and his mocking of the pope delayed the acceptance of his evidence. The original paper of Copernicus was in 1543; Galileo’s telescopic observations were made known in 1610. The latter’s 1633 trial for heresy, however, meant that serious discussion of heliocentric models disappeared for nearly a century from much of Europe. Nor was it due to ignorance or religious conformity; to the end the noted mathematician and non-conformist René Descartes (1596-1650) remained a staunch supporter of the heliocentric view, despite knowing the arguments of Copernicus and Galileo. Empirical results were not enough to carry the day.

It was only with the publication of Isaac Newton’s theory of gravity and motion that the tide turned. First, it contained a consistent and intellectually acceptable theory that encompassed many different phenomena. Second, it allowed the explanation of the data generated by a century of astronomy that used more sophisticated observational tools than were available to Copernicus. Not that it was a completely accurate system. In 1773 Pierre-Simon Laplace developed the mathematics needed to adjust these orbits for gravitational interaction with nearby planets. More recently, Albert Einstein’s theory of

relativity posited additional adjustments to the shape of orbits that were eventually confirmed through measurements of Mercury's orbit. Such later refinements are no longer a curiosity, since they need to be taken into account for GPS systems. But these were minor elaborations, at least from the standpoint of understanding our solar system. The main point is that it took 150 years for the clearly superior heliocentric view to displace the earth-centered models of Ptolemy.

Kuhn's Analysis

Kuhn found many puzzles here. Advances were not based on hypothesis testing; theories were held despite the clear knowledge that they were "wrong" for routine observational purposes; models which resolved such issues (Kepler's) did not seem to make a difference; models were chosen because of aesthetics or other non-evidentiary criteria. Finally, when change came, it was not uniform across Europe. Ptolemy's astronomy was part of Aristotle's larger, comprehensive explanation of the universe, and that was slow to disappear. Quite literally, Ptolemaic astronomy was not vanquished; astronomers wedded to the system did not convert to the new. Instead, it died out as adherents passed from the scene while younger scientists either avoided astronomy or followed Newton from the start. Until an alternative to Aristotle was available – and that arose only as the new physics developed alongside chemistry and anatomy – scientists apparently did not feel comfortable dispensing with Ptolemy.

How can we – how did he – resolve these puzzles? Kuhn's answer hinges on the fact that scientists form themselves into a community of practitioners, and "truth" is determined inside this community. This was quite uncomfortable for many philosophers, perhaps made more painful because their research agenda of building a logical foundation for science was at the time encountering what proved to be insurmountable difficulties. Practicing scientists also found Kuhn's message discomfiting. Indeed, critics derided him as claiming that truth is determined by a mob, though a careful analysis of the social undertaking of science shows this is not the case, or rather not wholly the case. But it is important to reiterate that the final victory of the Copernican view was more the result of the old school dying out than the conversion of astronomers to the new approach.

One of Kuhn's observations, not in itself novel, is that scientists in practice congregate themselves into professional societies. In my own case I am the member of several such groups – the Association of Japanese Business Studies, the Business History Conference, and the pan-discipline American Economic Association. I am on the periphery of, or a past member of, various other groups, including academic groups in Japan, such as the Law & Society Conference and the Regional and Urban Science Conference. Their annual conventions – and they *all* have them – attract varying numbers of participants, but the larger, discipline-oriented ones each have a core of 100-150 active members who are always there, and another 200 who show up from time to time. My experience thus matches that of Kuhn in the hard sciences, who hypothesized that most "subfields" would be turn out to be comprised of a group of about 200 scientists actively working with each other.

Scientists, then, are "joiners," and economists are no exception. Work tends to be collaborative, even though economists have no need for the large teams of the laboratory sciences. Such ties generate invitations to present on the seminar circuit of the "top 20" graduate programs, and at other institutions with groups of active researchers or involved in economic policy, such as the Fed, the World Bank, and major think tanks. Ideas are sold in these seminars, and the publication of repeated variations of the same basic theme helps garner attention. It is in such presentations that economists size each other up, where

they can probe the depth of the empirics and the cleverness of the ideas. Such reputation attracts graduate students – working under a well-known professors helps in the job market – and they in turn can help spread their mentors’ stories. It helps to be highly visible in professional associations. A number of the US-based Nobel laureates are past presidents of the American Economic Association, or former members of the Council of Economic Advisors. All are reflections of the communal nature of science.

How, then, does a new theory succeed? By convincing members of one of these groups to change? Hardly. That can happen, but on average the leaders in a group do not roll out the welcome mat for a young turk bent on usurping their power. Instead, those pushing major new ideas need to form their own academic subgroup, which can only develop over time.⁸ Furthermore, they need to convince academic departments that they should be hired. Are they quacks? Will anyone read their stuff? Will they get grants? If not, then they are at best deadweight in the institution; they might even drag it down. Such considerations aside, as a general rule social groups are internally conservative: if you want to join, you play by the rules, you don’t make the rules.

Well, how do you become a member of one of these subgroups? And what does being a member entail? As with becoming a W&L student, various things occur. First, younger members are indoctrinated into the group, through long and tortuous initiation rights and sometimes vicious hazing,⁹ in what is known as a PhD and (in the hard sciences) a post-doc. Members socialize with each other, read similar journals, begin to talk alike and (of course) think alike. As happens at W&L, you soon become able to spot outsiders: they use the wrong lingo, they drop the wrong names. They even dress wrong.¹⁰ This is *not* innocuous: unless you use the proper jargon, you can’t play the game. Even if as an undergraduate economics major you’re satisfied with remaining a peripheral participant, you still have to be able to understand what others are saying, and use some of the proper slang. Otherwise you won’t be allowed to hang around. We don’t force you as undergraduates to learn jargon merely to torture you. Of course precise terminology can also improve communication, but never forget that its role is also social, and that that is important to practitioners, not to be denigrated.

Figure 3: Disciplinary Matrix

- Socialization / organizational identity
 - Jargon
 - Professional organization
 - Journals
 - Personal networks
- Shared:
 - Symbolic generalizations [“tools”]
 - Exemplars [standard examples]
 - Stylized facts [complementary to and not independent of theory]
 - Models

Let us step back and generalize, as did Kuhn, but with the goal of shedding light on the apprenticeship process: jargon and similar marks of membership of a community are but one element of what he came to call a “disciplinary matrix.”

One element is a common empirical base to a discipline, which economists denote by the term “stylized facts.” These are not necessarily precise observations, indeed they are termed “stylized” because one of

the tasks of insiders is to delineate (or at least to know) in what way they are “fact” and how to interpret stylistic deviations. But they indicate the basic regularities that provide a starting point for work in a discipline.

Another is the use of a common set of “symbolic generalizations,” the analytic tools of the trade. This can be a set of graphs, or mathematical equations or (in literary analysis) standard ways to characterize the structure of a poem. In economics, for example, we employ a simple graph with “price” and “quantity” axes and “supply” and “demand” to calculate “equilibrium”. But these are mere tools, and are applied in different ways in different disciplinary matrices. For example, the equilibrium of “S&D” in microeconomics signifies something quite different from that of the short-run “AS & AD” in macroeconomics, though on the surface they are manipulated in a similar manner.

A third element is that a community of practitioners will share a set of standard models: core axioms or themes or insights, formalized in the harder sciences using symbolic generalizations. In some fields, however, theories are little more than a set of pigeonholes into which observations can be organized. Nevertheless, they are often suggestive of some causal feature, and are formulated to solve standard classes of puzzles.

The latter is more central than at first might seem to be the case, for apprentices have for centuries (if not millennia) been trained through the use of standard tasks. In the sciences, including economics, this is composed of mastering the solution of exemplary problems. Such exemplars are standard puzzles that are based on one or more stylized facts, provide practice in the tools of the trade, illustrate the application of a core theory or model, and are explicated with the use of specific jargon. Every economist for the past two centuries has studied the “water and diamond paradox” and even a Principles student can complete the phrase “guns and ...” Furthermore, they know that this is associated with a particular tool, a “production possibilities frontier,” and that it illustrates both tradeoffs (“opportunity costs”) and efficiency (“on the frontier”). The more astute may even spot that its curved shape is a reflection of “diminishing returns” and that it is a visual representation of one element of the classical growth models of macroeconomics.¹¹

These exemplars are a key feature, for it is by mastering them that one solves new problems. A piece of jargon may key the student in, pointing to the exam question as a probable variation of one such exemplar. Knowing that, the student can then pull out of his or her repertoire the model taught through that exemplar. That helps clarify the best tool – symbolic generalization – to use, and a richer set of jargon and stylized facts. New problems are solved by recalling similarities with known problems and their solutions. But to reiterate, each of these exemplars is embedded with a richer set of associations, all pointing at each other.

The diligent apprentice, then, can even build up a crib sheet of exemplars. Each entry has several columns: the exemplar, the theory it is used to teach, the tools used to solve the puzzle it posed, an empirical phenomenon or policy issue that motivated it, and a set of jargon used to present it. While there will be overlap – the same theory may have more than one exemplar, the same tool will be used many times, the same jargon can serve dual purposes – they form a congruent set: any one element can serve as a point of entry, opening the door to the whole group. Conversely, these different elements should *never* be studied in isolation: memorizing a list of jargon is counterproductive, severing the very links to the remainder of the disciplinary matrix that give it value, over and above its contribution to social identity. In sum, recognizing the centrality of the exemplar is perhaps the greatest direct benefit of struggling through Kuhn’s analysis of the nature of science.

Now becoming a journeyman requires more than merely mastering the tools; it also requires a

demonstration of quality work. Here we can return to a further implication of the community basis of a discipline: “truth” varies. To start with one example, to be a modern labor economist requires a demonstration of a particular sort of econometric prowess, but not much development of theory; it requires the ability to discuss the ins and outs of data, and “proxies” for variables that are seldom directly measured. Finally, at the level of the working group, there is typically a shared motivation: we care about poverty, or youth unemployment, or the status of women in the labor force.

Industrial organization is quite different. Practitioners of that discipline look for nice models, using lots of math, preferably written in the form of game theory. This math should be applied to illustrate a puzzle of industry behavior or corporate strategy. Case studies can sometimes be used, though it is *de rigueur* to include a regression; grad school and the job market demand it, as evidence of the mastery of basic skills. But “IO” economists don’t place much store in measures of profit, or even of price, and have low expectations. But an “IO” person really ought to know “their” industry well, and be familiar with a wide range of others. They should be able to tell why it matters – no longer because of antitrust policy, that was the previous generation, who were devoid of mathematics. Now it is because of its applications to corporate strategy in teaching MBA students. Poverty and all that stuff is too touchy-feely, absent from their conversation; they’re motivated by pulling in consulting fees, or for being recognized for developing a neat twist of a standard game theory model, or simply for “knowing beer” or semiconductors or (now passé) autos.

Figure 4: Desiderata for a Theory

- tractable (you can actually do the math)
- accurate (which generally is in tension with simplicity)
- orthodox (builds upon rather than displacing existing work)
- fruitful (generates new insights or applications)
- general (explains lots of phenomena)
- important (focuses on “key” questions)
- beautiful (a low priority in economics)
- clever (the *primary* consideration in economics)

Truth – that is, what we are about – thus varies from one disciplinary matrix to another. Some are concerned about neat models, or even pure math. Others want to be able to explain what’s going on with data, or theory that can applied in many different places, or to be able to contribute to policy. Of course an emphasis on models requires mastering the art of simplifying so that the math remains tractable. Those wanting to do applied work fudge the models: they have more variables than the modelers can handle, never mind that theirs are never quite the right data to match the theoretical concepts. In short, the desiderata of “quality” work cannot all be met simultaneously, and different communities value different elements.

One final point about communities is that they don’t mix. Partly because of the “comprehensive” exams

that most American-style graduate schools require, there is a core of basic models that economists of all stripes share.¹² However, graduate students quickly fall into membership in one or another seminar, work as a research assistant for a professor, and start coming up with a dissertation topic under the guidance of one or two advisors. Hence you quickly have people sort themselves into the “Trade and Development” workshop, or the “Labor and Population” workshop, or the “Industrial Organization” workshop, or the micro theory “Cowles Foundation” group – each graduate program has its own mix and attendant nicknames. Such seminars will have 1-2 senior professors and several junior faculty as “members,” with weekly paper presentations alternating among insiders and invited presenters, who are often the leading names in the field. By observing and eventually participating, a graduate student sees the give and take of their mentors, what questions fire people up, and learns about the network in the field, not just at their own but at other research institutions. They get pulled into one community, and when job-hunting rolls around, the community tries to take care of its own.

The long-run effect is that, first, new communities have a hard time establishing themselves, and second, that the communities are distinct. It is quite difficult to communicate across boundaries; an IO person finds little in common with someone in Labor, and neither speak to those on the Macro-Finance end of the spectrum. They can't: they don't share jargon, their styles of arguing are different, what motivates them will vary systematically, as will their criteria for what makes for a “neat” study. To top it off, their personal networks will not overlap.

And that is just among economists – try talking with someone in organizational behavior about incentives, and neither will understand the other.¹³ One will spout “principle-agent models,” the other the jargon of social psychology. Discourse really is difficult, and the concepts and many other dimensions may turn out to be incommensurable. Economists are infamous for opacity, for being unable to communicate their findings in a manner that is useful to policymakers. But economists are not alone. The better you become at playing the economics game, the harder it will be for you to explain it to outsiders.

Conclusion

Microeconomics is comprised of a series of partly overlapping communities of scholars, working on puzzles that they and their peers find compelling. Recognizing this, as Thomas Kuhn was the first to stress, is useful in many ways. It helps to understand how scientists actually work, serving as an antidote to the unfortunate addiction to “scientific method” from which many economists suffer. It explains the difficulty in reading the academic literature the profession churns out: the bias for writing for other specialists is not only strong, but overcoming it requires conscious and concerted effort at writing using “foreign” vocabulary, a skill that is not necessary and hence not valued within tight communities.

At a very practical level, the approach of Thomas Kuhn also highlights how one becomes an economist. This in turn points to the centrality of studying exemplars, and their interconnection with problem solving, with jargon, with models and with stylized facts. For the beginning student, the temptation is to study the latter elements in isolation. Doing so is bound to frustrate, indeed is a recipe for failure. That is not how we know the world around us, it is not how economics is done, and it is not how you should try to learn.

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This includes handouts used in Economics 210, “Microeconomic Theory,” during Fall 2003, as well as selected articles for those who might to read further. Because I am assuming that readers of this article

have read the handouts, I have avoided making specific citations.

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Notes:

1. Kuhn earned a Ph.D. in physics, working on quantum chemistry issues as of the 1940s. From the early 1950s, however, he turned his attention to the history of science, and his 1962 book, *The Structure of Scientific Revolutions* (University of Chicago Press) created a major stir, and was routinely assigned in a variety of college classes at least through the 1970s. He wanted, however, to make an impact as a philosopher and wrote extensively in that area. At the time of his death in 1996 at age 74, he was working on a volume focusing on the incommensurability of different intellectual disciplines, such that direct communication is impossible even when they use apparently similar vocabulary. He himself managed to maintain a foothold in 3 very different camps, and perhaps it was the need to express ideas in very different ways to these different audiences that made this problem so central to his later work.
2. Note that, although they would not employ the same terminology, literary analysis is undertaken in accordance with a similar set of concerns. Indeed, I believe that what I write below is as pertinent for an English major or history major as it is for someone pursuing a discipline that labels itself a "science."
3. Y happened after X, therefore X caused Y to happen – the phrase denotes a standard logical fallacy, the confusion of sequence or (for economists) correlation with causation. For example, when national crime rates fell in the late 1980s, politicians were quick to credit anticrime programs of that era, but the likely cause was a

demographic shift that lowered the number of young men, as well as lower male youth unemployment, rather than “anti-crime” measures.

To give another example, the Japanese government engaged in formal industrial planning (as did the French, the Soviets and many others in the 1950s and 1960s). Furthermore, Japanese industries such as textiles, shipbuilding, steel, autos and semiconductors in fact dominated industries world markets at one time or another. Added to that, jobs in the Ministry of Finance and the Ministry of International Trade and Industry attracted the top graduates of Japan's top universities. Compared to the US, the Japanese bureaucracy was clearly comprised of the country's elite. Outside analysts - most notably Chalmers Johnson in *MITI and the Japanese Miracle: The Growth of Industrial Policy*, Stanford University Press, 1982 - then attributed Japan's success to the bureaucracy's enlightened wielding of power. This argument attracted tremendous attention in Washington and in academic and business circles during the height of the infatuation with Japan's economic success and corresponding fears during Ronald Reagan's administration that American industry was in permanent decline, among both Democrats and Republicans. (Johnson himself was a noted Cold War hawk, with earlier books on the rise of the Chinese Communist Party and on "revolutionary change;" he had solid credentials in Republican circles. Ira Magaziner, who co-authored another study of Japanese industrial policy at about the same time, move in Democratic circles, and ended up as the manager under Hillary Clinton of an ill-fated attempt to reform the US healthcare system.)

But to return to the issue of logic: any careful analysis of the timing, intent, and details of actual policy suggests that in fact the government role was neither powerful nor, in most cases, constructive. Johnson and those who did similar studies never examined industries that did not succeed, focusing instead on government pronouncements and the writings of former elite bureaucrats. That the bureaucrats claimed a great role (notably trumpeting their role only after an industry rose to prominence) did not mean that without their efforts industry after industry would have failed to grow. After all, Japan boasted at an average 9% GDP growth rate from the mid-1950s through the first oil crisis of 1973; in that environment, it was unusual for an industry *not* to grow! The entire argument of Johnson and his peers is an example of the *propter hoc ergo post hoc* fallacy.

4. The Nobel Laureate Theodore W. Schultz came close: showing that peasants were rational, that “good” economic behavior did not require literacy, was one of his key contributions.

5. That is one finding of the work of sociologists and anthropologists: scientists are deluding themselves if they think that "scientific method" describes they way in which they actually do research. It is certainly a central claim to Kuhn's *The Structure of Scientific Revolutions*. Implicit in this paper - and explicit in places - is my own understanding of how economists work: a significant t-test does not change minds, and the lack of one does not lead to a model's rejection. Instead, the latter typically leads to successive modifications of the base model, until eventually a positive t-test (and hence publishable result) is obtained. But that is only one sort of data analysis; much is instead a careful exploration of what the data tell us, though again for the purposes of publication this may be disguised as hypothesis testing. Theory has different methodological standards; more on that below. See also Paul Diesing for a description of the antecedents to this formulation of the scientific method, attributed to Karl Popper in his efforts to counter what he showed to be facile arguments by the logical positivists (the "Vienna School") of the early 1900s. However, just as Popper's attempt to find method in science has not held up to empirical scrutiny, it has also failed to hold its own in philosophy. More robust are, for example, the work of Paul Feyerabend and Imre Lakatos.

6. Even though both were well trained in math and were statisticians in their early careers, neither Keynes nor Friedman employed formal statistical tests in their core work, nor did they devote effort to trying to translate their ideas into equations.

7. The economy of the 1600s centered on agriculture, and in general life depended on the seasons in a way we can only partially fathom today. It is easy to forget, too, that with the intermingling of church and state, fixing the calendar was also important to government and politics, as for example in the proper determination of the date of Easter.

8. Most such enterprises founder – great economists are great in part because they are entrepreneurs in the normal sense of that word, able to found and keep new entities going.

9. I have witnessed economists reduced to tears in front of their colleagues under the no-holds-barred assault of particularly nasty junior faculty members bent on showing off their own cleverness upon discovering a weakness

in a presentation. Where I went to grad school that was viewed as an unfortunate incident, and senior faculty tried to make amends. But some programs take pride in fostering a combative atmosphere.

10. For economists, the dress code is a blue blazer with beige trousers – I’ve even seen a skit mocking this at a convention.

11. From this perspective, literary analysis is also a science. An exemplar might be scanning a poem, to see whether it follows one of the standard typologies (“sonnet” of the Spenserian form).

12. This is not true of German-style education, which would include Korea, Japan and many other countries in Europe and elsewhere. These are very traditional apprenticeship systems in which a graduate student studies under a single professor, and can be correspondingly narrow in their basic training. I do not know the British system, but believe it is closer to that of the US than of the continent.

13. A concrete measure is to compare bibliographies of papers on the same topic – that of an economist writing on trust (me!) can turn out to have no items in common with one by someone in organizational behavior (e.g., various papers by Sim Sitkin, the editor of a volume in which a paper of mine appeared).