

## Economic Methodology Letters

The following is an extended exchange of letters/emails between two individuals: Poppy and Dan. Dan is an economics student; Poppy is a student of the physical sciences with a strong curiosity and a wide-ranging circle of friends. The letters were sent over a seven week period before Easter. They have been edited to exclude personal information.

The letters are a good guide to the main points in my lectures (excluding the last four). Two notes of caution. First, they leave out a good deal of detail and examples. Second, the arguments are not outlined in a rigorous or comprehensive manner, but instead reflect a more conversational style. However, a conversation is a useful way of presenting philosophical arguments, and has a long tradition in that discipline.

Simon Wren-Lewis  
April 2005

Week 1

Dear Dan

I came across a book entitled 'The Death of Economics', written by Paul Ormerod, who is also an economist. (Published by Wiley in 1994.) He has some very critical things to say about economists. Chapter 1 starts (I have edited this slightly):

*'The World Economy is in Crisis. Unemployment in Western Europe rises towards the 20 million mark. America faces the deep seated problems of the twin deficits, the federal budget and the balance of trade.*

...

*The orthodoxy of economics, trapped in an idealised, mechanistic view of the world, is powerless to assist.*

...

*Teams of economists descended on the former soviet union, proclaiming not just the virtues but the absolute necessity of moving to a free market system as rapidly as possible. But despite governments in the former soviet bloc doing everything they were told, their economic situation worsens.*

...

*Yet to the true believers, within the profession itself, the ability of economics to understand the world has never been greater. Indeed, in terms of influence in the world the standing of the profession appears high.*

Later on in the chapter he talks about the teaching of economics.

*Substantial and impressive textbooks exist, both in micro and macro economics, consisting in the main of the mathematical technique of differential calculus applied to linear systems.*

*It cannot be stated too often that very little of the content of such textbooks is known to be true, in the sense that many of the statements in textbooks on, say, engineering are known to be true: formula for building bridges exist, and when these formula are applied in practice, bridges in general remain upright. The same does not apply to economics, and yet the confidence of the true believers has grow'd and grow'd like Topsy.*

The book is over ten years old, so some of the topical references are a bit dated, but the points about the former Soviet Union and the US seem valid enough. He seems particularly annoyed about how arrogant economists are, despite their inability to solve key economic problems.

Have you come across this book? Is what he says about the truth of statements in economics textbooks correct?

Dear Poppy

Thanks for this. I have not seen the book before, but I've heard similar remarks from some of my non-economist friends.

I could discuss some of the specific problems that the book mentions, but I guess what I object to most is the implication that somehow economics is

divorced from facts. He talks about economic theory not being 'known to be true', in contrast to theories in the physical sciences like engineering. In my view economics is just like any other science in the following sense. We start of with some unexplained facts. We develop a theory to explain these facts. We explore the implications of that theory. If the complete theory fits the facts (not just the unexplained facts, but all other facts related to the theory), the theory is true. If it does not, it is false, and we go back to the drawing board.

Where economics differs from, say, engineering is that economic theories are more inexact. There is more uncertainty in economics partly because people are more unpredictable than physical systems, and partly because we cannot do experiments.

Dear Dan

No need to be so defensive! I was not questioning whether economics is a science – it was more the implication from the book that economists seem quite sure about their theories, despite the additional uncertainty you mention.

I also think you do not need to be so defensive for another reason. I did a Philosophy of Science course last year, and we saw there two reasons to question the idea that in the physical sciences there are objective facts that establish the truth of theories. First, in general facts are not completely theory independent. We use theory to organise the vast amount of information that we constantly receive. (This was covered in the first three chapters of our textbook: 'What is this thing called science' by Chalmers – its very clear, I would recommend it.) Second, we cannot use facts to prove a theory in the same way that, say, we can prove  $1+1=2$ .

This second point is called the 'Problem of Induction', and was discussed by David Hume (1711-1776), who I think also did some economics. Hume asked whether we could be sure that the sun will rise every morning. We have a theory that says it should, but we cannot guarantee that the theory will continue to work.

The example I remember is due to Bertrand Russell. Imagine a chicken, who was fed by the farmer every morning. The chicken would develop a theory that, when the farmer arrived each morning, it would get fed. The theory worked fine, until the farmer arrived one morning, and killed the chicken.

Compare this to the following statements:

1. All economists are arrogant
2. Dan is an economist
3. Dan is arrogant

Now we know, with certainty, that if 1 and 2 are correct, then 3 must be correct. This is called a deductive proof. 3 must follow from 1 and 2 – it could not be otherwise by the laws of logic.

John Stuart Mill (1806-1873), another philosopher who also did economics, described a practical and a logical problem of induction. Take the sun rising example again. Now we could imagine that some black hole, which we hadn't detected (they cannot be seen of course), collided with the sun – so the sun did not rise the next morning. That's a practical problem of induction – we just missed out on some information, but the laws of physics stayed the

same. The theoretical problem of induction – and the one Hume had in mind - is that the laws of physics as we now understand them might turn out to be wrong, in such a way that the sun does not rise next morning.

So to say that facts prove a theory is not quite as straightforward as it appears.

Week 2

Dear Poppy

Sorry to be defensive! Thanks for the info on deductive and inductive proof. I feel that economists wouldn't make the mistake that Russell's chicken made, but maybe I'll come back to that later.

I realised that facts interact with theories in two ways, one of which was missing from my previous letter. Theories make basic assumptions about the real world, which are then used as building blocks for the (deductive) development of theories. So we could ask (inductively) whether the assumptions of theories are true, as well as their predictions.

In fact, economists are always getting into trouble with the unrealism of their assumptions. This was the starting point for the only bit of methodology I know in economics, due to Milton Friedman. He argued that economists should not worry about realism of assumptions, and should instead just focus on predictive ability. However, some others argue that Friedman ignored his own methodology when it came to his work on the Phillips curve. Perhaps that's why economists tend not to worry about methodological issues.

Dear Dan

Tell me more about what Friedman said, and this Phillips curve stuff.

Dear Poppy

OK. Friedman was responding to a paper by Danl in Hitch. They surveyed managers of firms, asking them how they set prices. They used the results to cast doubt on the economists assumption that firms set prices to maximise profits.

Friedman drew an analogy with snooker players. He said that the best way to model a snooker game was to use the laws of physics: conservation of momentum and all that. However, if you asked snooker players if they knew these laws, they would probably say know. But they played snooker as if they did.

Friedman argued that the assumption of profit maximisation, or any assumption in economists, was fine if it led to good predictions. You didn't need to worry about the realism of those assumptions, as long as the theory was good at predicting.

The stuff on the Phillips curve is a bit more technical. A.W.H. Phillips (1914-1975) was the first to notice a relationship between wage inflation and unemployment. Later economists generalised this to an equation of the form

$$\Delta w_t = \beta E[\Delta p_t] - \alpha(U_t - NR)$$

where  $w_t$  are nominal wages at time  $t$  (logged),  $p_t$  are nominal prices (logged),  $E[x_t]$  are expectations about  $x_t$ ,  $U_t$  is unemployment,  $NR$  is some constant (called the natural rate), and  $\alpha$  and  $\beta$  are positive parameters. Thus if price inflation went up, or unemployment went down, wage inflation would rise. Typically  $\beta$  was estimated to be less than one.

Now if prices were a simple fixed mark up on wages costs, so  $\Delta p = \Delta w$ , and we have perfect foresight in the long run (so  $E[x]=x$ ), then this implied

$$\Delta p(1-\beta) = -\alpha(U-NR)$$

giving a long run trade-off between inflation and unemployment.

Friedman argued that, in the long run,  $\beta$  had to be one. However, he argued this not because that assumption predicted better, but because it was implausible to assume otherwise. If  $\beta < 1$ , this implied workers suffered from 'money illusion': that they did not realise that inflation would erode the purchasing power of their wages. It was much more realistic to assume that workers would adjust their expected *real* wage to the level of unemployment, so in the long run  $\beta=1$ . In that case there was no long run trade-off between unemployment and inflation, and unemployment would always tend to return to the natural rate.

Some economists have suggested that here Friedman was arguing against his own methodology, using the realism of assumptions to criticise and change a theory.

Dear Dan

Interesting. I agree that Friedman's emphasis on prediction (this is called instrumentalism in the philosophy of science) seems questionable. Imagine two theories, both of which predicted equally well, but one of which made realistic assumptions and another made unrealistic assumptions. Would we really be indifferent between the two?

Thinking about realism of assumptions reminded me about something else I learnt in my Philosophy of Science course. A philosopher called Karl Popper (1902-1994) wanted to distinguish between 'proper sciences' (physics etc) and 'non-sciences' like (he suggested) astrology, Marxism and the theories of Freud.

Popper first went back to the problem of induction. He agreed with Hume that facts that agreed with the theory could not prove the theory was true, in the same way that we could prove  $1+1=2$ . However, a fact that disagreed with the theory clearly showed that theory was false. For example, seeing one black swan could refute a theory that said all swans were white.

Thus, theories were potentially falsifiable. Popper argued that the problem with theories like astrology and Freudian psychoanalysis is not that they don't fit the facts. The problem was that they could never be disproved. A proper science, he argued, was one that had theories that were potentially falsifiable. This is called his demarcation theory.

According to this criterion, the reason astrology is not a science, is not because its underlying assumptions (that the position of the planets control personality) are implausible, but because its predictions can never be proved

wrong. So, for economics to be a science, it needs to make falsifiable statements. How does economics rate on these criteria?

Dear Poppy

Lots of questions and reactions to your last letter. I can see the attraction of falsificationalism, but the counterpart seems to be that observations that confirm a theory mean nothing. Can this be right? Also I doubt whether one contrary observation could completely destroy a theory – theorists could always argue that the observation was faulty, or some other condition of the theory was not met.

Also, thinking about astrology, I'm not sure I agree. The main reason I think astrology is nonsense is that its basic premise is implausible. The fact that it was believed a thousand years ago and not now must have something to do with the development of science, and that we now know about what planets are and what they do.

I'm also a bit worried about how his demarcation theory would work on economics, but I'll come back to you on this when I've thought more about it.

Dear Dan

Your right about theories protecting themselves. According to the Duhem-Quine thesis, all theories involve a large number of auxiliary hypotheses. A contrary observation could be due to the basic theory being wrong, or one of its auxiliary hypotheses being wrong. You could always protect a theory by changing one of the auxiliary hypotheses.

However, Chalmers describes 'sophisticated falsificationalism' as the requirement that any change to an auxiliary assumption should also be falsifiable. Consider two examples.

Apparently in Aristotelian theory all celestial bodies are perfect spheres. Galileo saw through his telescope that the moon had mountains and craters. The response of one Aristotelian was to protect the basic theory by saying that the moon's craters are covered by an invisible substance that makes the moon a perfect sphere. The problem with this ad hoc modification is it cannot be falsified.

Contrast this with the discovery that the orbit of Uranus did not obey Newton's laws. Two scientists conjectured that this problem could be solved if there existed another planet at a certain place in the sky. That was a falsifiable statement. People looked, and discovered Neptune!

Week 3

Dear Poppy

I now understand why I had difficulty in applying falsificationalism to economics. Falsificationalism is powerful when applied to universal statements, like all swans are white. But most theories in economics are not like that. Instead, economics describes relationships which tend to hold, but which sometimes may not hold due to complicating factors. I think Mill

described economics as describing tendencies, others have talked about economics as an inexact science.

The moment we move away from universal statements, then the asymmetry between confirmation and rejection disappears. If our theory was '95% of swans are white', then observations of white swans matter. In one sense they do not matter as much as seeing black swans, but this is a well known proposition of Bayes theorem. (The impact of an observation on our beliefs depends on our prior beliefs about the likelihood of that event occurring.)

The modern view of econometrics is that all economic theories are inherently probabilistic. We only have a sample of observations, and in testing hypotheses we can make Type 1 errors (rejecting a true hypothesis) or Type 2 errors (accepting a false hypothesis). As we can never eliminate type 1 errors, we can never be absolutely sure that contrary observations completely reject a theory.

However, while this means we do not have a complete contrast between confirmation and rejection, we can still test theories against the data, and make some statistical statements about the likelihood of the theory being true or false. So I guess the demarcation theory could still survive, if we replace 'falsifiable' by 'testable'.

Dear Dan

This is really interesting – when I did my course I guess we did mostly think about sciences like physics, where it is possible to construct universal theories and test them using experiments where we control for complicating factors (although there is always some noise).

One element missing from our talk of falsifiability and testability so far has been the existence or otherwise of an alternative theory. This is a point stressed by two philosophers of science, Thomas Kuhn (1922-1996) and Imre Lakatos (1922-1974). They use historical studies of scientific change to show that theories may continue to be used even when contrary evidence arises, but where there is no alternative theory available to explain these anomalies. For example, it was known since 1687 that the orbit of Mercury did not precisely follow the path suggested by Newton's laws, long before Einstein explained this through general relativity.

Popper tends to paint a picture of scientists as heroic figures, bravely constructing falsifiable theories. Both Lakatos and Kuhn suggest a less glamorous picture. In what Kuhn calls 'normal science', work takes place with an existing 'paradigm', extending and refining an existing theory with no thought being given to whether the basics of the paradigm are correct. Scientists are normally 'theory users' not 'theory makers'. Lakatos calls paradigms 'research programmes'. He talks about the 'hard core' of a programme (those theories that represent the foundation of that programme, and which will not be questioned), and a 'protective belt' of auxiliary hypotheses (which can be changed to fit the data). Lakatos also talks about positive and negative heuristics: a negative heuristic is how a research programme dismisses contrary evidence, while a positive heuristic is how the theory adapts to attempt to explain anomalies.

These ideas would certainly help explain why astrology was so widely used before science as we know it developed, and why it is not taken seriously now. It survived as long as there was not an alternative way of understanding events.

Dear Poppy

I can see how Lakatos's ideas might apply to economics. Because economics is an inexact science, then a single contrary (to theory) observation can be put down to this inexactness, and I guess this could be described as a negative heuristic. A more positive heuristic would be the search for potential 'market imperfections', and an examination of how these imperfections change theoretical results.

I have also tried to relate these ideas about how theories change to economics. The example that seems to fit best is the Keynesian revolution in macroeconomics. Before Keynes, macroeconomics was just microeconomics scaled up. These theories found it difficult to explain involuntary unemployment in recessions, and particularly the Great Depression of the 1930s, but they did so by asserting that something (e.g. trade unions) was preventing prices falling to clear the market.

The content of Keynes's 'General Theory' was very different. It started with aggregate relationships, and stressed the importance of effective demand in situations where prices did not clear markets. Crucially, it could explain involuntary unemployment, and suggested how government policy could reduce it.

However, the story does not seem to fit the frameworks suggested by Kuhn and Lakatos exactly for two reasons. First, I suspect Keynesian economics initially displaced its classical predecessor not so much because it predicted better, but because it offered a policy a way of overcoming a problem. (According to classical theory, any attempt by government to spend its way out of a recession would lead to a matching reduction in private spending: complete crowding out.) Of course, such normative issues are not such an essential part of the physical sciences. Second, there have been two (successful) attempts at synthesising the Keynesian and Classical approaches, yet this synthesis seems to be missing from the accounts you gave.

I also have the following worry. If paradigms or research programmes have such good ways of protecting themselves against awkward facts, when scientific revolutions occur, how do we know whether the right side won!

Dear Dan

On your last question, this is where Lakatos and Kuhn differ. Both stressed that paradigms or research programmes often disagreed about the evidence as well as theory. For example, followers of Aristotle questioned whether observations through a telescope should be counted as superior to observations with the naked eye. But Lakatos wanted to have some *objective* criteria of progressive theory change. The tool he used to do this was the 'novel fact'.

Its easiest to give an example. Einstein's general theory of relativity predicted that light should bend around the Sun. After the theory was published, scientists used an eclipse to observe light from stars being bent by the Sun, and confirmed Einstein's theory. So Einstein's general theory was progressive because it explained this novel fact.

Dear Poppy

I'm not convinced by this novel fact idea. Certainly Einstein's successful prediction is impressive, but why should we be more impressed by this than his explanations for Mercury's orbit, just because the latter was known about beforehand?

I'm reminded about an idea one well known UK econometrician had – that because prediction was more impressive than explanation, the researcher should temporary throw away recent data, and build a model using the rest. At the very end, they should use their model to predict the observations they through away.

Suppose one researcher did this, while another used all the data to build their model. Inevitably, the researcher using all the data would produce a model with the better overall fit. Should we reject this model and go for the other, just because it was used it to predict some of the data?

Also, going back to the Keynesian revolution, it is difficult to think of any novel fact there.

Dear Dan

I agree with you about novel facts. What counts as novelty seems to depend on the historical context, which gives the idea a subjective relativity that Lakatos wanted to avoid. I'm also worried about some of his other ideas. Is the hard core/protective belt distinction a binary divide, or a matter of degree? I also wonder if we need the idea of novel facts: could it simply be that the progressive theory explains more than the theory it replaces. Finally, the focus on theory change may be misleading, as important developments can occur within a paradigm or research programme.

Week 4

Dear Poppy

I've been thinking about the question you posed right at the beginning, about how economists can seem so sure of their theories when they appear to be relatively unsuccessful in some cases. I wonder whether there might be another source of knowledge for economists (and social scientists generally) besides conventional data.

Basic microeconomic demand theory is built up from very simple axioms about behaviour. These axioms, generally described as rationality, involve completeness (any choice can be ordered) and transitivity (if  $x$  is preferred to  $y$ , and  $y$  to  $z$ , then  $x$  is preferred to  $z$ ). If we add continuity, then we can write down a utility function. Assume that people maximise utility subject to a budget constraint, and we get the theory of demand.

Lionel Robbins, writing in 1935, thought that these assumptions could be confirmed by simple observation. He wrote 'The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates....The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so....These are not postulates the existence of whose counterpart in reality admits to extensive dispute once their nature is realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious.'

However, we could confirm these assumptions in an even more basic way, by asking whether they are valid for ourselves. Mill wrote "The desires of man, and the nature of the conduct to which they prompt him, are within the reach of our observation. We can also observe what are the objects which excite those desires. The materials of this knowledge every one can principally collect within himself"

So perhaps these basic axioms of theory can be confirmed by introspection. If so, then any developments of this theory are bound to have some validity. In this sense social scientists have an advantage over the physical sciences, which can perhaps compensate from the inability to do experiments.

Remember Russell's chicken. If the chicken had been an economist, it would have asked 'what's in this for the farmer', and might not have made the same mistake!

Dear Dan

I am not impressed by this idea of introspection. First, how can we be sure that we are not fooling ourselves about how rational we are? Second, how do we know that others are like ourselves?

On the other hand, the components of rationality you mention do seem to be pretty uncontroversial. But surely there must be much more to economics than this.

Dear Poppy

I understand what you say about introspection, but given how basic these rationality axioms are, the assumption that human beings are alike in this respect may be reasonable. (I think an Austrian economist, Mises, argued that self-interest was so basic to human nature that it had to be logically true, but I'm not sure I would go that far.) And remember that the results of conventional testing of economic theories are always subject to error (and the theories hardly ever involve universal statements), so the alternative to introspection is hardly perfect.

Of course there is more to economics than rationality, but I would definitely place rationality in the 'hard core' of the discipline. An economic methodologist, Hausman, defines economics as studying the consequences of rational greed. Economists tend to be very intolerant of models where there are unexploited profit opportunities. They believe individuals will take any

opportunity to increase their utility unless there are good reasons why this is difficult or costly.

A good example is the concept of rational expectations. Expectations are central to much economic decision-making. In macroeconomics in the 1950s and 1960s economists used simple rules to model expectations formation. The most common was adaptive expectations, which says

$$E[X_t] = E[X_{t-1}] + a(X_{t-1} - E[X_{t-1}])$$

where  $E[X_t]$  is the expectation of variable  $X$  at time  $t$ , and  $a$  is a positive parameter below one. According to this rule, expectations are revised by a proportion 'a' of last period's expectations error. This rule was used to model inflation expectations in the Phillips curve I mentioned in an earlier letter.

In the 1970s a number of economists became very critical of these simple rules. They argued that applying these rules meant that in many cases agents were ignoring information, and were worse off as a result. They advocated instead the concept of rational expectations, where agents used all available and relevant information in an optimal way to form their expectations. Rational expectations had been first suggested in 1960 by an economist called Muth, who wrote: "I should like to suggest that expectations, since they are informed predictions of future events, are essentially the same as the predictions of the relevant economic theory" His key argument was that "If the predictions of [economic] theory were substantially better than the expectations of firms, then there would be opportunities for the insider to profit from this knowledge."

After much debate, rational expectations became the standard assumption in economics. My reading of this debate was that rational expectations won the day not because it was better at predicting or explaining events than adaptive expectations, but because it was consistent with the basic concept of rational economic man.

Dear Dan

Rational expectations sounds a bit like an assumption of perfect knowledge that economists seem to be happy to make. I can see why rationality might be in the 'hard-core' of economics, but surely there must be other assumptions in there, once we move beyond simple demand theory?

Still not convinced on introspection.

Dear Poppy

Rational expectations is different from perfect knowledge (also called perfect foresight), because agents are allowed to make (possibly large) expectations errors. The key point is that these errors could not have been avoided or foreseen by using economic theory applied with best statistical practice. Applying best statistical practice will mean that the errors produced by rational expectations will have a zero mean and not be serially correlated over time.

You ask whether there other hard-core elements to economics besides rationality? There are few obvious candidates. Theories of trade (including

international trade) add assumptions about markets to basic consumer demand theory, but these assumptions are flexible: there are theories based on perfect competition, but also theories based on imperfect competition. The standard theory of the firm is very like consumer demand theory: firms maximise profits rather than individuals maximising utility, subject to a production function (and maybe a demand curve) rather than a budget constraint. From this we can get a labour demand curve, and once we allow leisure as a good we can get a labour supply curve, and therefore the basics of labour economics. Even the assumption of profit maximisation by firms can be justified as maximising the interests (utility) of the firm's shareholders.

Standard demand theory gives us how consumers choose between apples and pears, but a small extension to the rationality axioms gets us a theory of how agents choose between safe and risky assets, which is the foundation of financial economics. If we make the choice between consumption in different time periods, and assume that agents can freely borrow or lend, then we get a theory of saving, which is the basis of modern macro that focuses on intertemporal choice.

On justifying rationality, there is another argument that economists find persuasive. This is that agents that are not rational can be taken advantage of by those that are. Take transitivity for example. Suppose we have someone whose preferences are not transitive, so they prefer x to y, y to z, but prefer z to x. We could then offer to sell this person y for z, at a small fee, and they would accept because they would be better off. We could do the same for x and y. Finally, we could offer (for a small fee) to sell them back z for x, and they would again accept, because they prefer z to x. They would be back where they started, except for the fees that they had paid me. This is called a 'money pump' in economics.

So non-rational people would either learn that they would be better off being rational, or in some evolutionary way rational people would dominate them.

Dear Dan

You mentioned that finance theory was based on a simple extension to rationality. I was talking to a psychologist friend of mine, and he said that the extra assumption you need – independence – had been shown not to hold in experiments! He gave me this example. Suppose there was a lottery with the following pay-offs.

Choice	1%	10%	89%
A	100K	100K	100K
B	0	500K	100K
C	100K	100K	0
D	0	500K	0

The chances of winning are across the top. The axiom of independence says that the choice between A and B should be identical to the choice between C and D, because the two sets differ by a common element (the 89% outcome).

However, when this experiment is done, a significant number of people choose A over B, but choose D over C. This is apparently called Allias's

paradox. So if this axiom does not hold, why do economists still assume it? My psychologist friend says there are other examples where experiments contradict assumptions made by economists. And why don't economists use experiments of this type more often?

Dear Poppy

You shouldn't be mixing with psychologists! Actually experimental economics is a big growth area. Economists used to believe that experiments could not duplicate real life situations (for example, pay-offs in real life are normally much bigger than in these experiments), but now they are less dogmatic about this. Indeed, economists became famous when they helped Gordon Brown get billions in the auction for 3G mobile phone licenses, and they used experiments to test out their auction scheme.

Your friend is right about Allias's paradox. Economists have developed some theories to take account of it. But I guess that they believe that in most situations the paradox will not be that important, and so it can be ignored. Remember that economics is an inexact science: there will always be complicating factors that mean its results do not hold exactly.

Week 5

Dear Dan

OK, so I guess it's down to how important these deviations from rationality found in experiments are in real life situations, and that may vary on a case by case basis. But going back to another example you gave, on intertemporal choice. There you slipped in an assumption that agents were free to borrow or lend. Well lending is all right, but if I go to the bank asking to borrow a large sum of money, I know what the answer will be. Surely this matters in real life?

Dear Poppy

Absolutely, and this is just the kind of thing economists are interested in. There is a very famous paper in economics called 'The Market for Lemons', and its author George Akerlof won the Nobel prize for it. The paper looked at the used car market, and asked why almost new cars sold at a heavy discount compared to new cars. He said the reason was asymmetric information between buyers and sellers.

The title of the paper refers to the US term for a car that is dodgy from the start – it is called a lemon. I guess it's just not put together properly, and this causes problems throughout its life. I think they used to be called 'Friday afternoon cars' in the UK. Now these lemons will be worth a lot less than a normal good car. Akerlof suggested that the owner of a lemon will know it's a lemon, but the buyer will not. All the buyer knows is that some proportion of almost new cars are lemons.

As a result of this knowledge, the buyer will want some discount, to reflect the chance that he might be buying a lemon. The seller of the lemon will be happy to give this discount, because they are still getting a good price for a lemon. However the seller of a perfectly good, almost new car will not be

happy to give the discount, because they know that their car is not a lemon. As a result, some sellers of good cars will withdraw from the market.

This makes the probability of buying a lemon greater, so the buyer will want a bigger discount to cover this risk. Again, the sellers of lemons are still getting a good deal, but the sellers of good cars are not, so some more will withdraw from the market. You could end up with a situation where *all* the sellers of good, almost new cars had withdrawn from the market.

Dear Dan

Whoa, hold on, I don't understand what is going on here. I thought we were talking about problems with borrowing, and you tell me about the used car market! To be honest I'm not very interested in used cars, but even if I was I'm not convinced. Do these lemons really exist in significant numbers? Do we know we have a lemon the moment we have bought it? And what about warranties?

Dear Poppy

I agree, and so does Akerlof. He wrote "The auto market is used as a finger exercise to illustrate and develop these thoughts. It should be emphasised that the market is chosen for its concreteness and ease in understanding rather than its importance or realism." This is how we economists think. We tell simple stories to get ideas across. The idea Akerlof wanted to get across was that asymmetric information could have serious implications for supply decisions, and therefore market price.

Now where his ideas have been pursued is in the market for loans. The bank manager knows some borrowers will pay back a loan, but others will default. So he needs to charge a premium on the interest rate to cover the costs of default. This premium will deter some borrowers, but is likely to have less of an effect on likely defaulters. This is just like Akerlof's used car market. So we get what economists call adverse selection – the people the bank manager wants to lend to are driven out of the market. In this situation it may be better for the bank to ration loans, rather than charge a high premium on the interest rate. Hence your difficulty borrowing from the bank!

Dear Dan

OK, now I see. But then why do macroeconomists assume no constraints on borrowing?

Dear Poppy

Well, they do and they don't. When they do, they get an interesting result, which is known as Ricardian Equivalence. This says that consumers will save all of any tax cut. The reason is as follows. Suppose people live forever, and can borrow and lend freely. In this situation, their spending plans will not just depend on their current income today, but their income throughout their life. Lets call this 'lifetime resources'. This, after all, is why people save

for retirement: they consume less now so that they can carry on consuming at a similar level when they retire.

Now a tax cut that is paid for by the government borrowing today will require tax increases at some point in the future, either to pay back the borrowing or at least the interest on the borrowing. People will factor in to their calculation of lifetime resources not just the tax cut, but these future tax increases. Ricardian Equivalence shows that the net effect is zero: lifetime resources are unchanged, and so people are no better off as a result of the tax cut. So they consume no more, but just save the tax cut so they can pay for the higher taxes in the future.

Now, before you ask, you might object about the assumption that people live forever. But economists have looked at a more realistic assumption, where people have finite lives. As we would expect, Ricardian Equivalence no longer holds exactly, because there is a chance that you will be no longer paying taxes when the tax increases come. But if we plug realistic numbers into this model, then we find the departures from Ricardian Equivalence are very small. So the assumption of finite lives does not matter much, and it does make the model simpler.

So is the assumption of freely available borrowing a similar type of assumption? The answer is no, not necessarily. But to understand why, you have to apply the original theory! If you are denied borrowing by the bank, then you will regard the tax cut as effectively a loan from the government. As a result, you are likely to spend all of the tax cut, just as you would have spent all of any money you wanted to borrow. So even if the number of borrowing constrained individuals in the economy is small, their effect on the aggregate may be bigger than their numbers suggest.

In both cases these assumptions, although important, cannot really be described as 'hard core', because economists have explored the implications of relaxing them.

Dear Dan

Yes, this is a nice example. It shows when assumptions made by economists matter and when they do not, even though they are always strictly untrue. So you can get away with untrue assumptions because economics is an inexact science, but only as long as the abandoning the assumption does not matter much.

But your example also tells me something more about how economists work, and as a result I'm beginning to believe in the importance you attach to building theory up from basic assumptions about individual behaviour. Nowhere in your letter did you talk about the evidence on tax cuts. Instead in this and the other examples you quote, you seem to be playing with models: relaxing this assumption or that, and looking at the consequences.

Some philosophers of science draw a distinction between models and theories. Models are the deductive/mathematical systems themselves. Theories come when these models are applied to reality, using induction. If experiment is difficult, and evidence is unreliable (because the subject is inexact), then you have to fall back on playing with models.

Dear Poppy

Yes, we economists like playing with models. Paul Krugman, a very eminent US economist, has written “Mainly, it [economics] is a menagerie of thought experiments--parables, if you like--that are intended to capture the logic of economic processes in a simplified way. In the end, of course, ideas must be tested against the facts. But even to know what facts are relevant, you must play with those ideas in hypothetical settings.”

As this quote suggests, evidence is important too. Econometrics is all about trying to interpret the evidence. And the evidence on Ricardian Equivalence does suggest that quite a bit of tax cuts are consumed. But I think you are right that evidence has much less influence on what economists do than you might expect from a physical science background.

This seems particularly true in macroeconomics at the moment, where there seems to be a disconnect between theoretical models based on microfoundations and the work of econometricians.

Week 6

Dear Dan

I was going to ask you about that. Some time ago you talked about the Keynesian revolution in macroeconomics. But more recently you have been giving me examples of macro based on microeconomics.

Dear Poppy

I guess in one sense you could say there was a counterrevolution. One of the problems with the original Keynesian revolution was that it was founded on the idea that prices did not move to immediately clear the market (the in term is ‘nominal inertia’), but it was never very clear in Keynesian theory why they did not move. In the 1960s and 1970s, when inflation took off, this became a serious problem. Milton Friedman came to the rescue with a story based on the Phillips curve I described earlier, combined with adaptive expectations. Remember how we could get an equation of the form

$$\Delta p_t = \beta E[\Delta p_t] - \alpha(U_t - NR)$$

if we assumed that prices were a fixed mark-up on costs. Friedman argued that  $\beta=1$ , so we could write

$$U_t = NR - (\Delta p_t - E[\Delta p_t])/\alpha$$

Now Keynesians needed to argue that unemployment would deviate from the natural rate for long periods of time (i.e. for years, as in a boom or recession). Friedman noted that this would occur if expectations were adaptive, because it would take time for expectations about inflation to converge on actual inflation following a shift in inflation. What is more, a persistent upward shift in inflation would lead to a persistent fall in real wages (because the shift would be persistently under predicted), which would raise labour demand, reduce

unemployment etc – so the story fitted together. It became the standard way of integrating Keynesian ideas with inflation.

Rational expectations, on the other hand, suggested that expectations errors were random – if they were not, agents were using data inefficiently. So rational expectations undercut this Keynesian story. A group called the New Classical economists argued that this was just one example of how Keynesian economics neglect good (microfounded) economic theory, and that as a result Keynesian economics should be abandoned.

The end result was less dramatic. Keynesians found alternative reasons for nominal inertia, and these were used *within microfounded models embodying rational expectations* to justify the importance of Keynesian type business cycles and demand management policies. In this sense the Keynesian revolution survived. However the ‘microfoundations project’, to reinterpret macroeconomics in terms of the actions of representative, rational individuals, began to dominate the subject.

This is why it's problematic to describe the original Keynesian revolution as a scientific revolution of the kind examined by Kuhn and Lakatos. From today's perspective, the original Keynesian revolution has had a relatively minor impact on theory, although the impact on practice (active demand managements by governments or central banks, the importance of national accounts data) is more permanent

Dear Dan

This story helps explain why you stress the role of microeconomic models in the methodology of the subject. But does the fact that there has been such a change in the way macroeconomics has been carried out over the last fifty years suggest that there is an alternative to this micro based methodology in economics. One, perhaps, where empirical evidence at the macro levels plays a greater role?

Dear Poppy

Perhaps. There is certainly a large body of work by econometricians using macro data. However, at about the same time as the microfoundations project, there were also moves by econometricians to use less theory in their models. As a result, the standard way that macroeconomic evidence is presented nowadays is in the form of unrestricted Vector Autoregressive Models (VARs), where the impact of economic theory is minimal.

The post-WWII macroeconomics was a kind of compromise between the two approaches. Theory was used to justify aggregate relationships, but in a more informal and eclectic way. However, if the data went against theory, then this was allowed to change the model's specification, even though this meant that the theoretical foundation of the model became unclear. This flexible attitude to theory is something that the microfoundations approach would not tolerate.

A recent write up of the forecasting model at the Bank of England describes a spectrum of approaches to macroeconomic modelbuilding:

Data

Theory

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VARs

SEMs

Microfounded

Here SEMs stands for 'structural econometric models', which are the mix of theory and data based restrictions that were the mainstay of the original Keynesian revolution. I guess such a spectrum makes little sense from a philosophy of science perspective, where theory is supposed to be consistent with the data. It begins to make sense after recognising the alternative source of empirical verification that lies behind microeconomics.

Week 7

Dear Dan

I think I would describe the microfoundations approach as methodological individualism: the idea that all macro relationships must be derivable from micro behaviour. I see no reason why this has to hold. For a start, once you have any kind of heterogeneity in the system, aggregation may be impossible to achieve. More generally, in any but the simplest systems, complexity could make models intractable.

In the physical sciences there are lots of examples where we cannot hope to model aggregates by modelling their individual components, but where aggregate models do work.

One other thought struck me. This focus on self-contained microeconomic models based on rational economic man leaves little room for other influences on human behaviour, or other social science disciplines.

Dear Poppy

I think I agree about the difficulties with a bottom up approach. I can already think of shortcuts or cheats that are used to get around this. One example is the demand for money. A non-microfounded approach would talk about the 'medium of exchange' role for money, and make aggregate money demand (M) a function of total nominal output (Y). It would also talk about the opportunity cost of holding money, and add the nominal interest rate as an argument (r). It might also talk about money as a store of value, and add wealth (W) to the right hand side. It would then estimate an equation of the form

$$M = M(Y, r, W)$$

The microfounded approach derives this equation from utility maximisation. However, to incorporate the medium of exchange role properly is very difficult (the 'cash in advance' approach). Instead it uses a trick, which is to add money (in real terms) directly into the utility function, as providing 'transaction services'. We end up with an equation a bit like the one above. But treating money in the same way as consumption goods or leisure is a bit of a cheat.

Your point about economics being self-contained rings true in one way. You do not see the same emphasis on a multi-disciplinary approach in economics as you do in other social science disciplines. However, economists have applied their techniques (maximisation subject to constraints, game theory) to other disciplines with some success: for example voting models in political science, or firm location in economic geography.

Dear Dan

Yes, a sociology friend of mine told me about one of these forays by economists: a guy called Becker I think, who suggested that polygamy might be a rational choice by women to have children by successful males. Becker also suggested that poor families had lots of children because they could not afford to invest in the quality of their children, so had to go for quantity. Perhaps a rather narrow minded view, but I can see how it fits in with the type of theorising you have told me about earlier.

It would seem to be difficult for other social science disciplines to maintain such a 'focused' view (to put it kindly). Take Weber's theory of the link between the protestant ethic and capitalism for example. Weber noted that Western Europe was not the most obvious place for industrialisation to occur. The great civilisations were in India, China and Turkey, and China was for some time more technologically advanced.

So why did industrialisation occur in Western Europe and not elsewhere. Weber suggested a link between capitalism and the protestant religion. All societies wanted to accumulate wealth, but it was normal to accumulate in order to spend. The protestant ethic combined an imperative to work with a modest (sometimes frugal) lifestyle, so accumulation of wealth had to go to investment. This, he suggested, was the key feature of capitalism.

Thus his theory combines economics with religion, in a way that its difficult see economists doing. I think this holistic approach is typical in non-economics social sciences.

Dear Poppy

I've certainly understood how economists do things better as a result of our exchange, and perhaps also the limitations to what economics can do. Certainly economists would have difficulty in assuming anything where individuals appear to act in ways that are not in their own self-interest. An example that comes to mind is compulsory seat belts.

Economists would have difficulty with the idea that individuals should be forced to wear seat belts for their own good, because individuals should be free to make their own assessment of their own costs and benefits. You might think, however, that seat belts would protect others. But some economists have argued the opposite. As wearing a seat belt reduces the personal cost of having an accident, it is likely to make drivers take greater risks! So the net effect is to increase the danger to others. The problem here, I think, is the assumption that people act in their own self interest, particularly when small probability risks are involved.

I also think economists have difficulty in cases where individuals respond to social or religious/ideological norms that do not appear to be in

their interests. Take the question of whether to undertake university education, for example. There is a well-developed economic theory of human capital, where the individual balances the cost of education (including missed earnings) against the prospect of higher earnings in the future. While this clearly is a factor, it cannot explain the class divide in university applications. There seems to be much more going on here than rational self-interest.

Right, I'm off on my Easter holidays.

Dear Dan

Have a good Easter. I've still got some puzzles I want answers to, so expect some more questions when you get back.