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Economics as Science

Nancy Cartwright and John Bryan Davis

Economics as Science by Nancy Cartwright

The plan for this talk is to discuss, first, the question 'What is science?' I'm going to explain that the second question, 'Does economics fit the bill?', is hard to answer since we have no good answer to the first question. Then I shall turn to the question, 'Does economics' standing as a science give it special power?' Here, I shall point out that whether its knowledge constitutes science or not, economics does have esoteric knowledge that provides it with hidden sources of power.

My predecessor by several years at the London School of Economics, Karl Popper, thought he had the question 'What's a science?' solved. As you all know, scientific claims, he maintained, are falsifiable: 'I found that those of my friends who were admirers of Marx, Freud and Adler were impressed by […] their apparent explanatory power. These theories appeared able to

N. Cartwright (✉)
University of Durham, Durham, UK

J.B. Davis
Marquette University, Milwaukee, USA

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explain practically everything that happened within the fields to which they referred […] It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favor of those theories. It began to dawn on me that this apparent strength was in fact their weakness.'[1]

Here is an example that would have been dear to Popper's heart, just the kind he gives himself. The Rat Man, according to Freud, had an unconscious desire to hurt his father. This could, of course, result in him being nasty to his father. But it could also quite unexpectedly result in his being nice to his father through various Freudian mechanisms we all know about. So, the hypothesis is consistent with incompatible bits of data.

The first trouble, if we adopt Popper's criterion, is that it lets in too much. The claim that I am not sitting at my desk in Durham or in UCSD at this very moment is falsifiable, but it is certainly not science. You would need to add a whole lot more restriction to zero in on science and it is then the 'whole lot more' that does the bulk of the job. Worse, we have had considerable trouble figuring what the whole lot more is.

The second trouble is that it rules out too much. Physics has exactly the same problem as Freudian theory. The very same hypothesis about a situation can imply very different observations. Consider, for example, 'This ionised thallium has undergone beta decay' as the hypothesis. This implies two observations that are incompatible with each other: (1) that the ionised thallium has been replaced by fully ionised lead, with a continuum-state electron and anti-neutrino emitted; and (2) that it has been replaced by hydrogen-like lead with an anti-neutrino emitted. The physics solution to this is the obvious one we all know, that which observations are implied depends on what other empirical facts are taken to obtain in the situation. But that was exactly Freud's solution too! (Of course, one can then begin to puzzle out whether you then put some constraints on these other auxiliary assumptions. That attempt, too, has met with little success.)

The long and short of it is that we have not made much headway in saying what is science after 60 years of serious work in the philosophy of science. And not only in the philosophy of science but elsewhere: people are very concerned about climate change deniers, whether, when they

[1] Popper's 'Conjectures and Refutations', originally published 1963 (2013).

produce arguments, the arguments are really proper science; people in the USA are concerned about whether you can teach creationism in the public schools as a science along with evolution; the US Supreme Court's *Daubert* ruling worries about what can count as scientific expertise; and so on. None of these have come up with any satisfactory criterion to demarcate science from non-science. This past autumn, the Philosophy of Science Association, at their big international biannual meeting, after many, many years of the issue lying dormant, had a session on 'What is the scientific method?' Nothing came of it.

So, it is a little hard to answer the question 'Is economics a science?' My answer is that I do not have a clue.

I take it I am supposed to address whether an economist has particular power as a scientist rather than, for instance, as an adviser to public policy, running the Bank of England and so on. The first thing to note is that the power stemming from economics as science surely depends not entirely on the truth of the claim that it is a science, but on the perception—and there is a wide perception—that economics is a science and, as was mentioned earlier, a particularly good science because it is (at least, is thought to be) objective. It cannot be denied that (by contrast with sociology or anthropology, for instance) economics gets special kudos in policy areas and with the public because it is thought to be, it purports to be, it is widely believed to be objective and part of the reason for that is that it is quantitative. Quantitative is thought to be particularly objective. I will not go into this much—you are probably familiar with Ted Porter's *Trust in Numbers* and Michael Power's *The Audit Society*, both of which describe both the history and some sociology of how we have been converted to the idea that if something is quantitative it is objective, and if it is not quantitative it is most in danger of not being objective.

I would like to go on to talk a little about hidden sources of power. Economics as a science, because it produces knowledge and knowledge claims, has sources of power that many of you might recognise but certainly are not publicly recognised. The two I want to talk about are looping effects and then the hidden power that comes through the design of measures and models.

Looping effects have a number of other names: performativity, reflexivity—the word that George Soros likes so much—self-fulfilling prophesies.

I will just give you as an example one case—the Black-Sholes model—that you are surely familiar with, studied by Donald MacKenzie, the sociologist at Edinburgh. Here is how MacKenzie and Millo describe it: 'Option pricing theory […] succeeded empirically not because it discovered pre-existing price patterns but because markets changed in ways that made its assumptions more accurate and because the theory was used in arbitrage […] Option pricing theory […] did not simply describe a pre-existing world, but helped create a world of which the theory was a truer reflection.'[2] So, MacKenzie and Millo conclude, 'In so doing they altered patterns of pricing in a way that increased the validity of the model's predictions.'

So, that is one source of power, where you have these looping effects.[3] This is a very explicit case where there is a clear causal chain that MacKenzie traces. The other, of course, is Michel Foucault's theme that anyone who is able to create a new category or new concept that comes to be prominent can have a hidden source of power: as the concept becomes dominant, people begin to use it. They identify themselves to be in the category and begin to act accordingly and they identify others as in the category and treat them in the ways deemed appropriate. Like 'the involuntarily unemployed', the old example of the 'deserving poor' and so forth. That is one source of power that is not always so obvious to people outside economics.

Another hidden source of power over people's lives that economics has is in *the design of measures and of models*. I am going to talk about measures first, illustrating with cases from Tony (A.B.) Atkinson. These are places where having economic knowledge really matters; you would not know what you were doing if you did not have this economic knowledge. I teach this material when we talk about whether or not economics is objective in the sense of being value-free. These are all places where making certain decisions, based reliably on knowledge that you have as an economist and reasonably reliable predictions about how the measures will be used, will fairly predictably harm some groups of people and benefit others. There is often no scientific reason to make the decision one way rather than another. So you can, consciously or not, use your

[2] MacKenzie and Millo (2003).

[3] But as Michel Foucault argues, this source of power is not confined to economics but works for any science whose concepts get a grip on the way members of society and its institutions see themselves and others.

economic knowledge to benefit one group or another. This is an easy place for the intrusion of values—and it tends to be hidden. It depends on special economic knowledge that most people do not have and thus cannot see what difference it makes whether the measure is designed one way or another. Yet, given groups will be benefited and harmed, given the natural uses that we know will be made of the measures.

Here is just one issue Atkinson raises when you are thinking about designing a poverty measure. People can get the idea of the difference between an absolute and a relative measure; you can sometimes even get people to think, if it is a relative measure, about what they would like the poverty line to be relative to, like two-thirds of the median income. But if you start asking about whether it should be the mean, the median or the mode, you have lost most people. In Atkinson's book on poverty measures,[4] you find in chapter after chapter places where it makes a big difference to the poverty numbers and poverty ranking of different states and nations depending on how you design the measure in the detail—whether you choose relative versus absolute, mean versus median, whether you measure expenditure versus income, whether you treat households versus families, whether you use equivalent scales, numbers versus gaps. For many purposes, Atkinson favours measuring a poverty gap, which is how deeply below the poverty line individuals are as opposed to just counting the numbers. If we just count the numbers to measure poverty, then if you want to be seen to reduce poverty, it is a good strategy to take the people at the top and push them over.

Here is one really easy example of how important the details can be. The Indian Statistical Institute used to ask people how much rice they had consumed over the previous 30 days as part of their poverty measure. In response to criticisms that 30 days is too long a period and people do not remember how much rice they have consumed over the last 30 days, India changed their time period to seven days, a period that many other countries use. The technical change cut the Indian national poverty rate by half. By redesigning the measure, 175 million Indians suddenly escaped poverty.[5] Those are the kind of issues that come up in the

[4] Atkinson, 1998, Poverty in Europe, Wiley-Blackwell http://eu.wiley.com/WileyCDA/WileyTitle/productCd-0631209093.html

[5] Deaton (2001) p. 139.

design of measures. Atkinson also raises a variety of similar issues about the design of EU measures for social exclusion.

Let us look next at the hidden power that economics has in its ability to settle on *modelling assumptions*, both in the choice of model type and also in the choice of details within the model. First, consider the choice of the model type. Here is another case I take from Atkinson. He draws our attention to the fact that the commonly used representative agent models conceal issues of distribution. This is somewhat like Norbert Häring's point, where he argued that economic theory changed as the interests of the well-off changed. How it changed, in almost all of his cases, was by burying issues. Certain issues were no longer salient. They were not expressible in the model so they became hidden. It is not that you cannot talk about them, but you cannot talk about them when you are doing 'proper' economics within the model. Or consider this: most models assume the aim is to maximise expected utility. Of course, you can make utility the most abstract notion possible, but still there is a difference between looking for a course of action that maximises expected utility and one that maximises something like the substantial freedom of Amartya Sen's capability approach to just distribution.

On assumptions within a model, choice of parameters is a famous case. Nicholas Stern got in a great deal of trouble about the choice of parameters in the *Stern Review of the Economics of Climate Change*. The *Review* begins with a maximise-expected-utilities model. Interestingly, it is a representative agent model: there is one representative from each generation and Stern admits that the *Review* thus does not really take on issues about the distribution of responsibilities and benefits within any generation, so he does not really talk about who pays; for example, rich countries or poor countries. That is concealed in the representative agent model, but Stern is upfront that he is doing that.

The question that raised controversy is how much weight we should assign to each generation. If you look at the sum of expected utilities in such a model, you have to include a weight for each representative agent. Economists are all used to putting discount factors for the future into equations, but you have to think about what this discount factor for the future means in this equation. There is a variety of reasons for discounting the future. For instance, future generations might not be there, so you might want to count future generations a little less. Or, it could be a very poor way of putting uncertainty into the model, since this is not where a hedge against the uncertainly of our predictions belongs. When you put a weight in the *Stern Review* model you are weighting how much utility that generation matters in the proposed policy. The discount factors really matter here and what is interesting to me is that, once you have chosen an expected-utilities framework, you cannot avoid this question. You can fail to write a weight down there but that then that means you are weighting everybody equally, or you can discount some generations relative to others, but you cannot avoid the issue. Simply by virtue of using the expected-utilities framework in this case, you are forcing some ethical decisions to be made.

The reason I bring this up in this context is not really to point out the ethics of it so much as to point out that, if you look at the *Stern Review*, you have to be fairly sophisticated to see what is going on there. It takes a good understanding of what the modelling means to see that using an expected-utilities framework unavoidably raises this issue about how future generations are treated, and to understand and evaluate the different claims in the debate about the exact form of the discount factors.

Just to review: the promulgation of economic claims, I have reminded you, can change the world. Economics can even do this by making the world adjust to fit its otherwise probably false models. Moreover, details matter in measures and models. They affect policy and who benefits and who loses. The point is that it takes real economic knowledge to understand how these effects occur in both those kinds of cases. So, does economics have power because it is a science, because of those special kinds of knowledge that economics has? The answer is *yes*.

Economics as Science by John Bryan Davis

I will begin by identifying myself a little. I was trained originally in analytic philosophy, not at Oxford but in the Oxford style. Then, I was trained in economics, primarily history of economics. I am co-editor of the *Journal of Economic Methodology*, and I chaired and taught in a History and Philosophy of Economics programme for 10 years at the University of

Amsterdam, where the programme focus was the History of Economics from 1980 to the Present. I was, and am still, especially interested in the evolution of mainstream economics. A principal argument that I have made is that all the main new movements in mainstream economics are sourced from outside economics—behavioural economics, for example, from psychology. I was interested in what this meant for the state of economics. Sometimes, I am charged with arguing economics exhibits 'mainstream pluralism'. I will talk here about mainstream economics at this stage of its development as essentially a performative science. I want to emphasise the relation of economics to inequality and social stratification.

I think it is fair to say that we live in a world that is becoming increasingly unequal. It is also being institutionalised as such, and this works through structures that enhance and reinforce social stratification. I have worked with recent economics stratification theory as a foundation for self-reinforcing inequality and stratification processes that result from structures that systematically privilege higher and de-privilege lower socio-economic strata. Where is the science of economics in all this? The economics profession's own stratification processes involve replacement of its traditional independent reflexive practices for the evaluation and assessment of economics research with a stratification-reinforcing journal-ranking system that perpetuates status quo economics, limits innovation in economics, and thus serves social stratification.

The effect of this process in economics, I suggest, is that scientific behaviour in mainstream economics is increasingly replaced by bureaucratic behaviour and economics increasingly functions as what I will describe as a performative science in the sense of a science that always sees the world in its own image. I suggest that mainstream economics then risks becoming a 'bubble-science', one that is vulnerable to collapse like alchemy and other failed sciences of the past, and, as such, a potential contributor to economic crises. Let me explain this in terms of the change in reflexive practice in economics.

What was previously the traditional form of reflexive practice in economics? In the past, *economic methodology* and the *history and philosophy of economics* were economics' reflexive domains; in effect, its principal forms of scientific self-consciousness. Like other sciences, economics relies on a theory-evidence relationship. *Economic methodology* explains the theory-evidence relationship as a reflexive relationship. Theory depends on evidence and what counts as evidence is influenced by theory. Yet, because the economy itself evolves, there must always be new evidence so, for economic methodology, theory is always evolving and there must always be new theory. The *history and philosophy of economics* then explains economics' status as a science relative to the adequacy of its methodological practice and, in particular, according to its ability to evolve as a science.

What is economics' new reflexive practice? Methodology and the history and philosophy of economics are now largely marginalised in the economics profession. Whereas those reflexive domains were the means by which research quality and economics' performance as a science was ultimately judged, research quality is now judged largely through journal-ranking systems. Comments have been made in the discussion here about the importance of institutions and apparatuses like the Research Assessment Exercise in the UK in sustaining journal-ranking systems. These institutions and apparatuses are status-quo-biased, and reinforce social and theoretical stratification in the profession. Together, they reflect the famous Matthew effect: the rich get richer and the poor get poorer (from St Matthew), as described by sociologist Robert Merton.

In the overall dynamic, research from top institutions only goes to top journals, top journals only publish research from top institutions, and so top journals remain top journals and top institutions remain top institutions. I think that is now the main reflexive structure in economics. It has come about because, in the last 25–30 years, the journal-ranking system has been put forcefully into place for judging how people are promoted, how their research is evaluated, and basically how the profession works.

Looking over this time period from the perspective of economic methodology and the history and philosophy of economics, the main development was the elimination of the history (and philosophy) of economics from most economics departments. At the same time, the main generalist journals in economics ceased to publish history and philosophy of economics research, so that most economists ceased to be exposed to it and increasingly regarded it as irrelevant to the practice of economics. That meant that the way in which economics practises or operates the theory-evidence relationship is no longer an issue of concern in the economics profession. Where does that then leave economic methodology in the economics profession? The history and philosophy of economics judge the adequacy of the profession's economic methodology. Minus those fields' influence, most economists now confuse economic methodology and economic method. The former is the epistemology of economics; the latter concerns the tools of economics, especially econometric method, mathematical modelling, and increasingly experimental method. When method replaces methodology, these tools cease to be evaluated in regard to how well they contribute to knowledge. This means evidence is more and more taken at face value, since there is little reflection on what counts as evidence. I suggest the consequence of this development is that economics is becoming a performative science.

A performative science is one that actively seeks to remake the world—I emphasise 'seeks' because it cannot ultimately be successful—in its own image through policy and institutional design changes that incentivise behaviour to fit the theory. The MacKenzie research that has been discussed here is quite good on performativity in connection with the efficient markets hypothesis. Nudge behavioural economics is another example. Its policy recommendation is to alter social structures that incentivize people to behave as rational agents. Mechanism design theory may be even more important, because it aims to design entire market systems in such a way that people must behave as rational choice theory requires in order to be successful. What these initiatives thus do is seek to make the world, or 'perform' it, as standard theory sees it. I see the development of these approaches in mainstream economics as a natural outcome of the marginalisation of economic methodology (and the collapse of methodology into method). Without reflection on the epistemology of economics, economists become insensitive to the nature of the theory-evidence relationship and their role in determining it. Then, they are vulnerable to seeing the world in the image of their own research.

How does this all fit together with the recent emergence of journal-ranking systems as the main means of evaluating research in economics? If you do mainstream research, it is readily identified as such, and so it possesses a self-validating character. In reflexivity terms, mainstream research then functions like a self-fulfilling prophecy. If you do mainstream research, since journal rankings identify this as good research, your research fulfils the requirement of being good research. The opposite is the case with heterodox or non-standard economics. It is a self-defeating prophecy. By being identified as such according to the journal-ranking system, it must go to non-top journals. Since non-top journals only publish lesser quality research, heterodox or non-standard research must be lesser quality research.

So, we have, as one of the outcomes of mainstream economics evolution as a performative science, that it differentiates research practices according to where they originate in a stratified profession. This means many substantive topics are off the table for the mainstream of the profession, not only non-standard research, but such matters as the role of normative values in economics. Another way to put this is to say that economics is becoming an increasingly self-referential science.

I ask, then, is mainstream economics at risk of becoming a bubble science? A science that systematically rebuilds the world and its scientific practice in its own image is one that is likely to fail to explain a changing world. The failure of economics to anticipate and, after the fact, explain the financial crisis fits this picture. A bubble science, then, is one that will suffer significant stranded theoretical asset write-downs. We know from the history of science that this has occurred regularly. There have been many bubble sciences. Marxist economics was mentioned. Is neoclassical economics, its cold war compatriot that played a comparable ideological role, sitting at the end of a similar historical evolution?

It is interesting that mainstream economics seems to have become increasingly performative in a period when other sciences have gained greater influence within economics. I have written fairly extensively about the new movements deriving from other sciences in economics: complexity theory, behavioural economics, experimental strategies, and neuroeconomics. They have all originated from outside of neoclassical economics. Thus, they bring in deep reasoning from other sciences, 'contaminants' by the standards of neoclassical theory, and so we now have an economics ecosystem that is more diffuse and unclear in its overall character. I ask: is there a new reflexivity operating internal to economics generating new methodological and epistemological issues which runs counter to the mainstream's performative ambitions? Might this possible development again require a history and philosophy of economics able to judge economics' recent trajectory relative to its past development? A history and philosophy of economics that takes the present as history?

As a closing remark, let me comment briefly on how mainstream economics might adjust to these other-science influences. One thing that might happen is that key components of standard thinking get replaced piecemeal by new theory components that reflect other-science influences yet still comport with the main thrust of mainstream economics. I take as my example the theory of labour compensation. The standard view is that labour is paid its marginal product. Going back to the 1980s, when game theory and behavioural economics began to influence economics, the Chicago School developed an alternative view of labour compensation called 'tournament theory'. You are no longer rewarded according to your marginal contribution but, rather, according to your success in a lottery among many equally qualified people. Successful individuals then gain employment and income, and are set apart in terms of rank and position appropriate to a stratified world. Lazear and others have shown how labour markets are efficient under this system. So, the old neoclassical marginal reward analysis is put aside, but a mainstream competitive, efficiency-based account is preserved.

Interestingly, an economics that evolved in this way would be less bubble-like because it captures the real world phenomena of social stratification. It does so, on the view suggested here, because it accommodates other-science influences, albeit within its own traditional framework of competition and efficiency. I leave further reflection on this case to other occasions. What seems fair to conclude here, however, is that this kind of evolution of economics works quite well in a world in which a bureaucratic journal-ranking system explains how the science of economics operates.

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