Economics in the lab: Completeness vs. testability

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Abstract   Two important arguments in the methodological literature on experimental economics rely on the specification of a domain for economic theory. The first one is used by some experimenters in their skirmishes with economic theorists, and moves from the assumption that theories have (or ought to have) their domain of application written in their assumptions. The other one is used to play down the relevance of certain unwelcome experimental results, and moves from the symmetric assumption that the domain of economic theory is more limited than a literal reading of its assumptions would suggest. Of course, only one of them can be right. In this paper I criticise the former, and outline some well-known arguments that strongly point in the direction of the incompleteness of economic theory. Some remarks on the role of methodological arguments conclude the paper.

Keywords: experiments, theory, methodology

1 THE ARGUMENT FROM COMPLETENESS

Some economists are disturbed by the robust anomalies of standard economic theory that have been discovered and replicated in the lab. An obvious way of defending the theory is by questioning the design of these experiments. Typically, these economists argue that the experimental conditions are too unrealistic, or somehow not 'right' for theory testing, because some key conditions and factors that make the theory work in its intended domain of application (the real-world economies whose behaviour the theory is ultimately intended to understand/predict) are not instantiated in the laboratory. Some experimenters have developed a quick and effective way to deal with this charge. Here is a typical instance of this counter-argument, in the writings of Vernon Smith:

> If [an experiment's] purpose is to test a theory, then it is legitimate to ask whether the elements of alleged 'unrealism' in the experiment are parameters in the theory. If they are not parameters of the theory, then the criticism of 'unrealism' applies equally to the theory and the experiment. If there are field data to support the criticism, then of course it is

important to [modify] the theory to include the phenomena in question, and this will affect the design of the relevant experiments (Smith 1982: 268).

I merely test the theories you have devised, the experimenter says to the theorist. If the experiment is unrealistic, so is the theory it tests. Hence the problem lies with the theory, not the experiment designed to test it (see also Wilde 1980: 143; Plott 1991: 902–5). This *tu quoque* argument is supposed to shut up conservative theorists, and it works to a certain extent. But it works by imposing some very strong requirements on scientific theories, in particular the requirement that a theory's domain be written explicitly in its assumptions. Although this argument may have fulfilled an important function in defending experimental economics from the sceptics, it ultimately relies on a mistaken conception of the nature of theories, experiments, and the relation between them. Now that experimental economics has become generally accepted within the profession, it is time to dispose of this rhetorical device and replace it with a more adequate methodological account.

2 THE PROBLEM WITH COMPLETENESS

Why should a scientific theory carry its own domain written in its assumptions? The answer to this question depends crucially – and unsurprisingly – on what we take a theory to be. Unfortunately the term 'theory' is used both in science and in everyday life somehow liberally, to include anything from a vague speculation unsupported by empirical evidence, to very precise, highly explanatory principles like the laws of Quantum Mechanics. Since such sloppiness obscures important methodological issues (Hacking 1983), it is best to use the term *theory* only to refer to precisely defined, possibly formalised, highly integrated sets of principles endowed with potential explanatory power. For anything else, we can use the weaker term '*hypothesis*'. This is only mildly revisionary with respect to scientific language, for it reflects more or less the way in which economists speak of theories, when they want to be precise. (To put it another way, you do not get a rough informal guess or a low-level empirical generalisation published in the *Journal of Economic Theory*.)

In the philosophical literature, the idea that theories should specify their domain of application is traditionally linked with the search for laws of nature. If theories are sets of statements including at least one law, and laws aim at describing deterministic or probabilistic associations between events, then it is natural to require that the conditions of applicability of the theory be written in the antecedents of the laws. I shall call it the requirement of *completeness*. Consider the simple example: 'For every x, if x has property P, then x has property Q'. The observation of an x with P but not Q shows

that such a statement is not a true law of nature. The natural move is to try to figure out what difference, if any, there is between $P\&Q$ and $P\&{\sim}Q$ cases. Suppose the latter are associated with a third property R, whereas the former are not. By adding an extra condition we can try to restore the law-likeliness of the statement as follows: 'For every x, if x has properties P and ${\sim}R$, then x has property Q'. A universal statement with incomplete antecedent or which does not specify a set of sufficient conditions for the consequent to be instantiated is simply not a genuine regularity law.

The identification of the goal of science with the search for laws of association is usually justified on highly philosophical, even metaphysical grounds.[1] However, economists are also interested in theories for practical purposes: they want to use them to *predict* future events or phenomena, and to *intervene* in the economy (to bring new phenomena about, or disrupt or prevent them from happening). From this viewpoint, the completeness requirement has some obvious practical virtues too. A complete theory (that is, a theory specifying its own domain of application) is applicable almost mechanically: you just need to figure out in each case whether the conditions stated in the assumptions are instantiated in the real world. Moreover, it is easily testable: again, all we need to do is check that the 'right' circumstances are in place. (Especially this latter virtue is very attractive for an experimental economist interested in theory testing.)

But very few economic theories live up to the completeness requirement. Let us take a simple example: the transitivity principle of utility theory. The principle in its bare-bone version says that

$$[(x\succ_i y)\&(y\succ_i z)]\rightarrow(x\succ_i z).^2$$

Once interpreted as a universal statement, it translates into the claim that 'For all human beings i and all states of affairs x, y, … , z, if i prefers x to y and y to z, then i prefers x to z'. But in fact in certain experimental conditions – such as the famous case of preference reversals (Lichtenstein and Slovic 1971) – the principle seems to break down.[3] Taking the completeness requirement seriously implies that the principle should be amended so as to make sure that the conditions in its antecedent ('for all …, if … ') are truly sufficient for the consequent to be instantiated. In fact some economists suggest that factors such as learning (by repetition of the experimental task) and arbitrage can restore the transitivity of preferences (e.g. Berg *et al.* 1985, Chu and Chu 1990). But there is as yet no generally accepted theory of how to model the effect of repetition and arbitrage on the transitivity of preferences. According to the completeness requirement, more energy should be invested in the task of formally incorporating such mechanisms into economic theory, for the domain of a theory must be written in the antecedent of its laws.[4]

But the fact that they do not expand utility theory to include arbitrage and repetition, should not be an embarrassment to economists. Take a

physical law like $F=G(m_1m_2/r^2)$. It does *not* state that 'if two bodies have masses m_1 and m_2, and lie at a distance r, then the force of attraction between them is directly proportional to the product of their masses and inversely proportional to the square of the distance'. It states that 'if two bodies have masses m_1 and m_2, lie at a distance r, *and no other force than gravitation intervenes*, then … etc. etc.' The standard way of dealing with this problem is to include a *ceteris paribus* (*cp*) clause in the antecedent of laws: $(P\&cp){\rightarrow}Q$ – 'if P then Q, other things being equal' (Cartwright 1983; Hausman 1992). But then notice that to a certain extent the domain of the theory is left vaguely specified. The ceteris paribus clause is a 'catch-all' proviso; although certain factors in its domain may be known (electro-magnetic effects for the law of gravitation; arbitrage and repetition for the transitivity principle, etc.), others are left unspecified.

A supporter of the completeness requirement of course does not have to give up so easily. The classic rejoinder is that 'laws' such as 'ceteris paribus, for all bodies $F=G(m_1m_2/r^2)$' are unsatisfactory, or that the *ceteris paribus* clause merely points to a problem that ought to be solved by means of a better theory. One should 'unpack' the clause, in other words, and include explicitly all the relevant factors in the assumptions of the theory. One must move from $(P\&cp){\rightarrow}Q$ to $(P_1\&P_2\&P_3\& \dots P_n){\rightarrow}Q$, in other words.

But the prospect of modelling *all* relevant causal factors in the antecedent of theories/laws does not seem to be very promising for sciences like economics. Economic concepts refer to entities and phenomena that are evidently non-fundamental, that typically *supervene* on other entities and phenomena that are customarily studied by other disciplines. The conditions for the law of demand to hold, for instance, include the existence of human beings with certain preferences/beliefs, but also with certain cerebral functions, which in turn depend on chemical laws, which depend on physical laws, etc. It seems unlikely that we can model all this stuff in an economic theory because: (1) we only have a vague idea of how the different levels (physical, chemical, biological, psychological) are related to one another; (2) in order to model all the relevant conditions for the applicability of the law of demand we should go much beyond the linguistic resources of standard economic theory; (3) if we knew how to do that, the resulting theory would probably turn out to be terribly complicated; and (4) the theory would not be of much use, because eventually we want variables that we can control for policy purposes, and these usually lie at the level of analysis of standard economic theory.

If such arguments do not sound immediately convincing, it partly has to do with the fact that we are so used to think in reductionistic terms that we tend to ignore or underestimate the practical difficulties with which even the most basic reductions are achieved. (Contrary to common scientific pro-paganda, for example, even allegedly unproblematic reductions like those from chemistry to physics, or from biochemistry to chemistry, are incredibly

messy and often just not feasible.) Consider moreover that science in its historical development does *not* display a progressive reduction in the number and variety of established theories. Quite the contrary: new disciplines and sub-disciplines are constantly created, and phenomena are explained by means of an increasing number of theories and models, at different levels of specification. Science becomes increasingly varied and specialised, instead of unified under more and more fundamental theories (Suppes 1984).

Of course, *some* important steps in the progress of science have been achieved by enlarging the domain of a theory and digging 'deeper' in the microstructure of reality. The classic examples are the unified explanation of light and magnetism by means of electromagnetic theory; or the explanation of heat and pressure by means of statistical mechanics. As far as experimental economics is concerned, some promise seems to lie in the partnership with evolutionary theory and neurobiology. But it is important to realise exactly what these examples can and cannot prove. As a defence of the completeness requirement, these examples are misplaced: no one means to rule out or criticise a priori programmes like evolutionary economics or neuroeconomics. These programmes may or may not work, and we are presently in no position to make serious predictions about this. The point is that their success should not be made a *precondition* for doing 'proper' economic science (Dupré 1993; Cartwright 1999). Experimental economists *are* doing proper economics right now, and can keep doing it without incorporating the conditions that specify a theory's application within the theory itself. In fact most science goes on like that, the integration with 'lower-level' or neighbour theories being typically only partial and rough.

3 THEORIES AND MODELS

The arguments based on completeness and sufficiency rest on a basic misconception about the role of theories and models in science. In order to understand the role of theories we need to change perspective quite radically, and learn to see them as *tools* that help overcoming our limited cognitive capacities. Ronald Giere (2002) has recently proposed an illuminating analogy: theories are like cognitive scaffoldings supporting our representations and inferences. As such, they must not be too complicated: a theoretical model is usually a simplified and artificially isolated system that ignores or idealises dramatically what goes on in the real world.

Consider the following claim by Robert Lucas:

One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be prohibitively expensive to experiment with

> in actual economies can be tested out at much lower cost (Lucas 1982:
> 271).

The key expressions in this quotation are 'artificial economic systems', and
'to experiment with'. A model is an entity that can be manipulated, or
experimented upon, to put it in Lucas' terms. You *do things* with models,
you don't just contemplate them or put them in correspondence with reality.
Often the manipulation of models is performed by making changes in the
assumptions that describe some of its features, and by demonstrating that
certain consequences follow from certain changes.[5]

   This account fits well with the view that most scientific theories or models
aim at capturing *causal relations*. And causal relations are typically incom-
plete: when we say that 'hammering the metal bar makes it bend', or that
'reducing the interest rate stimulates investments' we do not mean that it
does do so in all cases and under all circumstances. A model provides a
concrete example of a situation in which the causal relation *can* hold, and
does so by ignoring all sorts of complications ('disturbing' or 'background
factors') that may cause the relation to break down. Each model implicitly
provides the *ceteris paribus* conditions of the relations that are built into it,
to put it another way: whatever is *not* included in the model, is in the *ceteris
paribus* clause (Cartwright 1999). Knowledge of causation then does not
require knowledge of all the background and boundary conditions that are
sufficient (jointly with the main cause) to make the effect happen. (Other-
wise, given the large number of possible interfering factors and preventa-
tives, we would never be able to claim that we know the causes of anything,
cf. also Woodward 2000.) The conditions may be numerous and vary from
case to case, and as such belong to the domain of applicability of a causal
claim.

   But then model-based knowledge must be applied intelligently, making all
the adjustments that are required from case to case. The possession of the
extra-theoretical knowledge required to put a theory at work is exactly what
distinguishes a good scientist from a student who has just read a scientific
textbook. A bad student thinks that she can infer the domain of applica-
bility from the theory itself, but in fact has only got a very vague clue of how
to use it in concrete cases. A lot of training and experience is required in
order to apply scientific theories.[6]

   Thus, for example, by learning game theory models you cannot figure out
exactly what their intended applications are. What you are given is a
handful of standard textbook applications as examples of the sort of things
the models are likely to be applicable to. The exact domain of application is
not defined in the textbook, because is part of the extra-theoretical hypo-
theses that are tested in applied science all the time. As Thomas Kuhn
taught us, in 'normal science' students learn modelling techniques and are
told that some models can be applied to some paradigmatic cases. But it is

implicit that the intended domain may vary, even though the models remain the same. This leaves the issue of the domain of scientific theories partly open. Which is just as it should be, in my view.

4 COMPLETENESS VS. TESTABILITY

I have argued elsewhere that experiments are similar to models in that they are also *tools* that we use in order to understand reality. As such, they can be more or less abstract, more or less similar to the 'target systems' (or 'real-world economies') we are ultimately interested in understanding. But there is no reason why they should be as abstract as theoretical models. Quite the contrary: we are often interested in adding components in the experiment that cannot be formally modelled in our theories. Science is a play with at least three characters: models, experiments, and their target (Guala 1998, 2005). Although we ultimately aim at understanding (and controlling, and predicting) the behaviour of target systems (real-world economies), the knowledge we are aiming at does not have to be entirely theoretical in character.

 The accent on the completeness of theories is misplaced and hides another, more important issue. The disturbing aspect of many critiques of experimental economics is that they are non-specific, and as such non-testable. The worry is that theoretical economists may try to insulate their theories from the empirical evidence by continuously claiming that 'something' is missing from the experiment, but without specifying exactly what. If this is their real concern, I certainly side with the experimenters here. But this does not imply that we should enforce the completeness requirement on scientific theories. Indeed, a much weaker methodological requirement will suffice. The idea, in a nutshell, is that although the possible factors that may limit the applicability of a theory or model do not have to be necessarily included in the theory itself, they can be used to defend a model from experimental anomalies *only if* they are defined in a precise enough way to make them *amenable to empirical testing*.[7] The rationale for this weaker requirement is well illustrated by Chris Starmer:

> While potentially valid criticisms of a given experimental design, [the] objections … which point to a specific limitation of the experimental setting do not seem to tell against experimentation *per se* since the experimenter can mount a ready response to each such objection. For example, if the hypothesis is that 'the free rider theory failed [in a given public goods experiment] because the incentives were too small', then run a new experiment with bigger incentives. If it is suspected that communication between subjects enabled them to 'beat' the free rider problem, design a new experiment that makes communication more difficult. So long as the theory defender identifies some specific aspect of

the design that renders it unsatisfactory as a test of the target hypothesis, it seems reasonable to think that a new experiment could be run that could 'correct' the limitation of the earlier test. Hence, criticisms that point to specific reasons as to why an experiment is not a satisfactory test of a hypothesis do not tend to undermine experimenting; they suggest new problems that can be investigated experimentally; they enrich the experimental agenda (Starmer 1999: 9).

This proposal is in line with some general criteria that we should impose on *all* inductive inferences. Scientific inferences from the evidence are always ampliative and hence fallible. As such, one should not be impressed by the mere *possibility* of making a mistake. Consider a simple inference from the fact that X and Y are statistically correlated, to the claim that X is a cause of Y. Is it not possible that a third factor Z that we have not controlled for is in fact a common cause of both X and Y? Yes, in principle. But in order to challenge a *specific* causal inference, one must point out exactly why the result may not be valid, and by doing so implicitly provide a new *hypothesis* (recall my earlier distinction between experimental hypotheses and formal theories) that is amenable to empirical testing. Similarly, the mere possibility that an experiment may lack some relevant 'real-world' features is not enough: we need probability in science. One cannot even start to figure out what the relevant probabilities are unless some reason for the lack of validity is indicated (cf. Guala 2005, chs 6–7).

Notice also that to give up on the completeness requirement does not have to be interpreted as a concession to instrumentalism. That a model is applicable *here* but not *there* is puzzling and needs to be accounted for in some way; one does *not* face a dilemma between a complacent instrumentalist attitude towards the truth of economic theories (à la Friedman 1953) and the requirement that domain restrictions be formally modelled in the theory itself (see also Starmer 1999: 18).

5 THE DISCOVERED PREFERENCES HYPOTHESIS

I have not said anything yet about the second argument for which the domain issue is important. I have done it on purpose, because the previous discussion should help us to put it in context. Different versions of this argument have been recently proposed by Charles Plott (1996) and Ken Binmore (1999); I will follow a recently established convention and refer to both versions by means of Plott's own label, the *'discovered preferences hypothesis'*.[8] The hypothesis states that people do not have, *in general*, stable and consistent preferences along the lines postulated by standard economic theory. That is why it is relatively easy to construct experiments in which the theory is violated by at least a significant minority of experimental subjects. According to the hypothesis, however, the theory does apply with a

remarkable degree of accuracy to behaviour in some conditions, where individuals have had a chance to form stable and consistent preferences about the objects of choice. (The preferences have thus been 'discovered', somehow metaphorically.)

The discovered preferences idea is based on the apparent similarity of several phenomena investigated by experimental economists, in contexts as varied as double oral auctions, public goods and ultimatum games, and individual decision making under risk. In several such contexts experimenters have observed a progressive convergence of the empirical data towards the theoretical prediction, under some experimental conditions. The conditions or mechanisms that facilitate the formation of stable and consistent preferences (and hence make the application of standard economic models possible) include adequate incentives, repetition, feedback, the transparency and simplicity of the task, and in general the possibility of learning.

Notice first of all that the hypothesis is entirely compatible with the analysis above. It recognises that models do not carry their domain of application written in their assumptions, and that their applicability is usually an extra-theoretical matter. This does not mean that the discovered preferences hypothesis is *right*, of course, only that it is *legitimate* – that it does not violate any reasonable rule of scientific method. In fact, the correctness of the Plott-Binmore proposal with respect to some specific experimental phenomena has already been questioned (cf. Cubitt *et al.* 2001).

But all this goes somewhat beyond the topic of this paper, which does not aim at settling substantial empirical issues. I want to finish with some general remarks on the spirit of the arguments discussed in this paper. Methodological discussions are sometimes used to transform what is at heart an empirical issue into a philosophical one to be solved by mere stipulation. This is obvious in the case of the arguments from completeness, but applies also to the discovered preferences hypothesis. The hypothesis is sometimes used to argue that the experiments which do not reproduce the conditions that tend to make the standard models applicable, somehow lack validity or interest. This suggestion is particularly evident in Binmore's writings, from which the following quote has been taken:

> I will happily undertake to refute chemistry if you give me leave to mix my reagents in dirty test tubes. Equally, if you undertake to prove in the laboratory that young stockbrokers cannot learn their trade by denying my subjects access to the conventional wisdom that the stockbroking profession has built up over many years of interactive trial-and-error learning.
>
> Just as we need to use clean test tubes in chemistry experiments, so we need to get the laboratory conditions right when testing economic theory.
>
> … I know that denying the predictive power of economics in the laboratory except under such conditions implies that we must also deny

the predictive power of economics *in the field* when such conditions are not satisfied. But have we not got ourselves into enough trouble already by claiming vastly more than we can deliver? I am certainly tired at having fun poked at me by marketing experts for supposedly believing that economic consumer theory is relevant to the behavior of customers buying low-cost items under supermarket conditions (Binmore 1999: F17)

There is some bold honesty in these paragraphs, especially in the acknowledgment that the domain of applicability of standard economic theory seems to be much more limited than expected. But the 'we all know that it can't work in such conditions' attitude obscures an important point: the domain of economic theory is constantly redefined and worked out after a lot of empirical effort. The work of those who have devoted their careers to observing violations of the theory under a range of different 'background' conditions has been and still is extremely useful and relevant. This research (Kahneman and Tversky's work is the explicit target of Binmore's dismissing remarks) shows what the limitations and the real domain of application of the theory are. Of course the anomalies do not *falsify* the theory – as some enemies of the standard theory would like – for the simple reason that falsificationism is not the methodology of science (of *any* science, let alone experimental economics).

Perhaps Ken Binmore and other social scientists deeply committed to evolutionary game theory dislike this empiricist attitude. The limitations to be imposed on standard economic models in their view are motivated by theoretical considerations, rather than by the available empirical evidence. Since the disagreement here reflects a deep divide regarding the methods and the standards of scientific proof, this seems a good point to stop (but see Sugden 2001). Where I stand on this issue should be evident by now: experiments are a powerful method for the discovery of where the theory works. It seems more sensible to leave it to the experimenters to try and discover the empirical limitations of economic models, than to rule out entire classes of experiments as irrelevant from the start.

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NOTES

1 The typical motivation behind this account is deeply empiricist in character: events are all there is. If things 'just happen' and are not connected by causal or necessary links, then the job of the scientist is simply to summarise events by subsuming them under general categories, and show how these categories do (and do not) overlap.

2 I am using this example because the transitivity principle can be straightforwardly interpreted as a law of association. As a matter of fact I do not believe that this case is representative of the majority of the theoretical principles used in economics, which are more naturally interpreted as causal claims than as regularity laws. More on this below (but see also Cartwright 1989, Hausman 1989, Hoover 2001).

3 The interpretation of preference reversals as a violation of transitivity is one among many proposed in the literature (Lichtenstein and Slovic famously deny the existence of a stable preference structure, for example) but is probably the most popular one in the economics literature. I cite it merely for illustrative purposes anyway.

4 As Bob Sugden pointed out (in correspondence) it is not difficult to think of ways of modelling the effects of repetition and learning. The fact that economists do not give it much importance confirms my point about the unproblematic incompleteness of economic theory (see below).

5 For an account of models that stresses this manipulability, cf. Morrison and Morgan (1999).

6 This point has been forcefully made by Collins (1985); on the extra-theoretical 'stories' that economists use in order to put models at work, see Morgan (2001).

7 The conflation of completeness and testability has a long history in the debate on ceteris paribus clauses in economics. See the discussion in Hausman (1992, ch. 8).

8 See Cubitt *et al.* (2001). Binmore does not like this convention because it suggests the existence of some pre-existing psychological entity that is discovered, whereas in his view experimental subjects simply learn to be consistent with repetition.

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