Struggling Over the Soul of Economics: Objectivity vs Expertise

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CHESS Working Paper No. 2013-01
Durham University
September 2013

CHESS

Centre for Humanities Engaging Science and Society

at Durham University

CHESS working paper (Online) ISSN 2053-2660

Struggling Over the Soul of Economics: Objectivity vs Expertise[†]

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[†] I gave a version of the paper as my Inaugural Lecture at Durham University on June 6, 2013. I want to thank the audience for the the discussion afterwards which helped to improve it.

We now have a situation where social and psychological theories of human thought and action have taken the place of [the individual's] thought and action itself . . . Not live human beings, but abstract models are consulted; not the target population decides, but the producers of the models. Intellectuals all over the world take it for granted that their models will be more intelligent, make better suggestions, have a better grasp of the reality of humans than these humans themselves
Feyerabend 1999: 263

When the crisis came, the serious limitations of existing economic and financial models immediately became apparent. Arbitrage broke down in many market segments, as markets froze and market participants were gripped by panic. Macro models failed to predict the crisis and seemed incapable of explaining what was happening to the economy in a convincing manner. As a policy-maker during the crisis, I found the available models of limited help. In fact, I would go further: in the face of the crisis, we felt abandoned by conventional tools.

In the absence of clear guidance from existing analytical frameworks, policy-makers had to place particular reliance on our experience. Judgement and experience inevitably played a key role.
Trichet 2010

1. Introduction

This is the golden age for philosophers of economics. When I first entered the subject in the late 1990s, the economics profession seemed all fine and dandy. In macro, a 'new synthesis' between new Keynesian and the new classical schools had just emerged, micro experienced an exciting revival fuelled by new findings in behavioural economics and micro econometrics, and economic policy subscribed to the Washington Consensus. Of course, outsiders such as heterogeneous economists and practitioners of history and philosophy of economics such as myself had our complaints but they were perceived as just that: economics dilettantes' grumbles.

This has changed over the years but most dramatically with the 2008- Financial Crisis. Suddenly we hear insiders to the profession make the same or similar complaints we had made, except couched in a more flamboyant language. We hear that 'the economics profession went astray because economists... mistook beauty... for truth' (Krugman 2009), that economists 'killed America's economy' because of unrealistic models (Stiglitz 2009), and that the Crisis has made clear a '*systemic failure of the economics profession*' as it had systematically disregarded key factors responsible for outcomes such as the Crisis (Colander, Goldberg et al. 2009; emphasis original) – all from highly prominent members of that very failing profession.

These are good times for making methodological remarks, then. The remarks in this paper concern the notion of objectivity and the role of experts in economics. I will argue that core methodological debates are at heart debates about the notion of objectivity and about how objective a science economics can and should be. I will then introduce an alternative notion and show that for economics to be objective in the new sense, expert judgements are likely to play a much more prominent role than they currently do (or, more precisely, ought to do following the traditional ideal). It is quite obvious, however, that making expert judgements more visible alone won't suffice. The right *kind* of expert judgement is needed. I'll therefore finish with some thoughts about what a better economic expertise might look like. To start us off, I'll tell the tale of a fascinating case of failing economics, which highlights just the aspects of economic analysis I will focus on throughout this paper.

2. A Fascinating (and, Embarrassing) Case

This short story begins with a 2010 paper in which two Harvard economists Carmen Reinhart and Kenneth Rogoff purported to show that a country's level of debt and GDP growth are negatively correlated (Reinhart and Rogoff 2010). Their evidence seemed to indicate an important non-linearity: 'the relationship between government debt and real GDP growth is weak for debt/GDP ratios below a threshold of 90 percent of GDP. Above 90 percent, median growth rates fall by one percent, and average growth falls considerably more' (*ibid*.: 1). 90 percent debt/GDP thus looked like a tipping point beyond which growth drops sharply.

In their paper, Reinhart and Rogoff were careful not to draw strong policy conclusions from their findings or even to read them causally. However, other statements they made lent themselves to causal interpretation ('In a series of academic papers with Carmen Reinhart... we find that very high debt levels of 90% of GDP are a long-term secular drag on economic growth that often lasts for two decades or more', Rogoff 2012; my emphasis), and they certainly regard the 90 percent threshold an important indicator for policy (*e.g.*, 'Our analysis, based on these cases and the 23 others we identify, suggests that the long term risks of high debt are real.', Reinhart and Rogoff, *op. cit*.: 23).

The timing of this research could hardly have been better. Many governments ran huge budget deficits to finance fiscal stimulus packages in the aftermath of the recent financial crisis. As a consequence, public debt/GDP ratios soared all over the world between 2008 and 2012: from 64.8 to 101.6 in the US, 44.5-89.8 in the UK, 66.2-93.1 in the Eurozone, 64.9-81.6 in Germany, 105.4-161.6 in Greece. Alas, not all countries could handle the increased levels of debt equally well: the US steered dangerously close to a fiscal cliff, several European countries such as Greece and Cyprus had to be bailed out by IMF and the EFSF, and, probably, the best is yet to come. IMF and EFSF grant financial assistance only after a 'country programme' with the requesting government has been agreed on, and these programmes invariably contain numerous austerity measures. The

Reinhart-Rogoff findings appear to justify austerity. Until an Amherst grad student cooked their goose.

Thomas Herndon tried to replicate the Reinhart-Rogoff findings as an exercise for a term paper in an econometrics class. Despite his attempts, no matter what he tried his results kept deviating from those published by the prominent economists. So he asked them for their spread sheet, received it and found the sources of discrepancy: a silly coding error, mysterious data exclusions and dubious methodological choices (Herndon, Ash et al. 2013). When corrected for the mistakes, growth remains slightly lower for countries with a debt/GDP ratio above 90 percent but the difference is not at all dramatic. I haven't seen any evidence to the effect that Reinhart-Rogoff deliberately tweaked results. Thus far, they have admitted to the coding error but defended other aspects of their study (Cook 2013). It is pretty clear, however, that the original analysis did not quite receive as much attention as it should have, especially considering the likely policy consequences of research of this kind.

I do not want to pass any judgement on this case here. What it does in my view is to highlight challenges with respect to three aspects of economic analysis: theoretical, methodological, and ethical. One should note, first, that Reinhart and Rogoff established their result on the basis of an empirical study alone, with no theory supporting or explaining the result (I mean 'theory' here in the broadest sense of either macro or micro economic abstract principles, historical and institutional background knowledge and so on). Second, Herndon *et al*. point out certain problems concerning selective data analysis, weighting and other methodological choices Reinhart and Rogoff made. But who is to say that the former are right and the latter wrong? On what methodological principles could we tell? Third, the case raises *ethical* challenges. One the one hand there are issues concerning scientific integrity. Even if no foul play was at work here, arguably, Reinhart and Rogoff did not take the appropriate care in making sure their evidence supports their claims. Or perhaps it is peer review that should have seen to that. Whoever made the mistake, something went wrong here. On the other hand, one might wonder whether research results that have a degree of political effectiveness such as these should come with warning labels. Once more, in the original paper Reinhart and Rogoff never talk about causation, just association. However, it must have been clear to them that in policy and public arenas subtleties such as this will be ignored. A later reply by the authors shows that their actual views on the issue are a lot more subtle than they were taken to be (Reinhart and Rogoff 2013). Perhaps they should have made that clear when they first published their results?

3. The Traditional Ideal of Objectivity in Economics: Abstract, Mechanical, Value-Free

There is a 'received view' in economics how good theory and good methodology ought to look like, and how economists ought to address ethical issues. This received view, I submit, stems from certain ideas regarding the notion of objectivity in science. Good science ought to be objective, and objectivity with respect to theory formation, methodology and ethics has a very specific meaning. In this section I will trace this three-pronged idea of objectivity in economics.

3.1 Objectivity in Theory Formation: Abstracting the Reality Behind the Phenomena

According to a widely held view concerning the aims of science, science ought to describe, and to some extent succeeds in describing, the objective reality behind the phenomena. Phenomena, that is, objects and events of scientific interest in the form in which they appear to us, are, or so this view goes, co-produced by reality and the idiosyncrasies of our particular points of view such as details of our perceptual apparatus, expectations formed by upbringing and culture, local and historical details of the conditions under which the observation was made and so on. As a consequence, phenomena in their full empirical concreteness are too complex for fruitful scientific theorising. One reason is that science aims to establish laws, that is, truths of high generality. As the precise details of an individual observation will never be exactly replicated, theorising cannot be about the results of observations. Therefore, science has to abstract as much as possible from the details pertaining to a scientific observer's standpoint and examine reality behind the phenomena. It is in the nature of this reality that it cannot be observed. However, scientists can theorise about reality and test alternative theoretical conceptions against the observations they make.

While this 'two-worlds view' (there is, first, a world of concrete ephemeral things with smells and warmth and colours and locations, and there is, second, a world of real things stripped off of most of these properties but which are more enduring and more frequently reappearing) has entered Western philosophy back in antiquity, it was probably Galileo whose science presents the first thoroughgoing and tremendously successful application of it. Galileo is responsible for the distinction between secondary and primary qualities, that is, qualities (characteristics) whose existence depends on us (such as odours and colours) and those that would continue to exist were all conscious beings wiped out from the universe (*The Assayer*, see Galilei 1623 [1960]). And Galileo is responsible for developing the method of analysis of synthesis, according to which phenomena are first broken into their simplest parts, the laws of these simple parts are established, and finally, the different laws of the various parts are combined to make a prediction about a new phenomenon (*cf.* Naylor 1990).

It took nearly two hundred years for the two-worlds view to find its way into economics. Adam Smith's economics (1776[2008]) was still deeply affected by his approach to moral philosophy, which required moral judgements about a phenomenon be made in consideration of all relevant detail (albeit from an impartial perspective, see Raphael 2007: Ch. 5). Only with David Ricardo (1817) did economics become an abstract science. Ricardo solved policy problems by building a simplified model of the situation within which the policy relevant question could be addressed unambiguously. Of course, the best known application of this approach is found in Ricardo's theory of the comparative advantage, which he demonstrated using a two-countries/two-goods model and used to advocate the abolishment of the Corn Laws.

Ricardian economics, and not Smithian economics, has won the day. There were of course a few periods of uproar. Richard Jones, a 19th century political economist and close friend and ally of John Herschel, William Whewell and Charles Babbage (see Snyder 2011), developed a view of political economy in which phenomena in their concrete embedding in the cultural and moral contexts of their time were the subject of theorising, in direct opposition to Ricardo's toy models (1831). But shortly after the publication of his book John Stuart Mill presented arguments to the effect that Jones' approach could not possibly be of use in political economy (1843/1844), arguments that many methodologists would still regard as compelling today.

The most famous controversy arose towards the end of the nineteenth century when Carl Menger, founder of the Austrian School of economics, attacked members of the German Historical School and specifically Gustav Schmoller for their methodological aberrations. Schmoller contended that the starting point for all economic investigations be found in the concrete and comparative study of economies in all their gory detail through history and statistics, much like Richard Jones had. He had no patience for Ricardian economics (Schmoller 1883: 978; quoted from Haller 2004: 8):

> Once abstract economics had achieved a great system, its source of power dried up because it volatilised its results too much into abstract schemes, which no longer had any connection to reality.

Menger, by contrast, regarded history and statistics as at best 'auxiliary sciences' and saw the best way to study economics in finding the laws that apply to the simplest elements through the use of reason. Once more the objectivists prevailed: the Historical School essentially disappeared with Schmoller, except for its effect on American Institutionalism, which has, after World War II, itself disappeared from the face of the earth (Yonay 1998).

From Mill and Menger we learn that it is not only 'abstract' and 'simplified', which characterise this idea of objectivity. There is also the view that economics describes (or theorises about) a privileged aspect of the social world. This privileged aspect, the 'economic aspect' is given by the proper understanding of the nature of economics, which is captured in a definition. Mill's definition, for instance, reads (Mill 1844):

> The science which traces the laws of such of the phenomena of society as arise from the combined operations of mankind for the production of wealth, in so far as those phenomena are not modified by the pursuit of any other object.

Menger does not define economics explicitly but it is clear from his writings that he regards (theoretical) economics as the science that traces the (strict) laws pertaining to the satisfaction of human needs. Theorising, then, proceeds not only by abstracting and simplifying some aspect of the social world but by focusing on a specific aspect. There are slight differences in the precise understanding of what this is among different economists, but all agree that there is such an 'economic aspect'.

A final battle I want to mention here is the 'Measurement Without Theory' debate of the late 1940s. Again we find proponents of an approach to economics that begins with concrete phenomena and studies them comparatively through history and statistics – in this case the National Bureau of Economic Research economists Arthur Burns and Wesley Mitchell – who were attacked by Tjalling Koopmans for not grounding their studies in 'economic theory' (1947). According to Koopmans, measurement and observation, causal inference and policy analysis all had to be guided by abstract economic theory in order to succeed. And again, most economists went with Koopmans.

The reason to present this potted history of methodological controversies here is twofold. On the one hand I want to show that certain criticisms that have been advanced against contemporary economics in the wake of the Financial Crisis are hardly new but rather have been made over and over again in the history of economics ever since economics left the embrace of philosophy after Adam Smith. On the other, the history shows that the issue is in essence one about objectivity. Proponents on one side of the debates defend an ideal of objectivity that regards claims about an abstract, simple economic reality behind the phenomena as objective whereas their opponents demand that the subject of economic investigations be these phenomena in their full complexity.

3.2 Objectivity in Methods: Creating Results By Mechanical Means

There is a second 'traditional' sense of objectivity that doesn't pertain to the *products* of science (such as scientific claims about an abstract reality behind the phenomena) but rather to its *procedures*. To say that science is objective in this second sense is to say that its methods are free from personal elements, that in principle anyone (as long as they have the required technical skills) who employs these methods would come to the same conclusions. In other words, science is objective to the extent that it uses mechanical methods instead of human judgement.

In the history of economic thought there have been numerous controversies whether economics be a deductive or an inductive science. Richard Jones, for instance, developed his own inductive, Baconian approach against the deductive, Ricardian mainstream, Mill's methodology of economics has been termed 'deductive *a priori*' (Hausman 1992), and Schmoller's inductivist (though for a critical view on that label, see Reiss 2000).

However, the categories 'deductive' versus 'inductive' are rather too coarse to get to the heart of the matter. For one thing, contestants on both sides (with the possible exception of Carl Menger) defended the view that an appropriate economic methodology makes use of a combination of inductive and deductive elements. Mill, for instance, only argued against the method of *specific experience*: since we will never find two situations (say, two economies) that are exactly alike except with respect to one factor, we cannot apply his method of difference to economic matters for causal inference (Mill 1844, Mill [1843] 1874). Nor do we need the method of difference. Since we already know the most basic principles of human behaviour (people seek wealth and avoid labour) and technology (the law of diminishing returns) *inductively from generalised experience*, we can apply these fundamental principles to a specific situation to make a prediction. Schmoller, similarly, thought that induction and deduction are inextricably linked and equally needed for scientific reasoning like two legs are necessary for walking (Schmoller 1920, vol. I: 110; quoted from Hutchison 1994: 279):

> What has been achieved is just as much the result of deductive as of inductive reasoning. Anyone who is thoroughly clear about the two procedures will never maintain that there are sciences explanatory of the real world which rest simply on one of them.

The issues that divide the camps therefore concern not so much induction or deduction on its own but rather how much of each and at what stages of the investigation and how much and what kind of experience is needed for a good inductive argument.

So what is a good inductive argument? That is the subject of today's most hotly disputed methodological issue in economics and many other social sciences. This debate has most aptly been referred to as the 'causal wars' and described as follows (Scriven 2008: 11):

> The causal wars are about what is to count as scientifically impeccable evidence of a causal connection, usually in the context of the evaluation of interventions into human affairs. The most recent battles are between those arguing that only the use of RCTs should be accepted as providing acceptable evidence (sometimes, the exotic regression discontinuity (RD) design is also allowed). The RCT *or randomly controlled trial*, is an experimental design involving at least two groups of subjects, the control group and the experimental group (a.k.a. study group, or treatment group), between which the subjects are distributed by a strictly random process (i.e., one with no exceptions), and which are not further identified or distinguished by any common factor besides the application of the experimental treatment to the experimental group.

The RCT is essentially a probabilistic version of Mill's method of difference. Proponents maintain that a good argument in favour of some result R must contain the premise '*R* was tested in a randomised trial' – in apparent defiance of Mill's views of the applicability of the method of difference in economics.

But the defiance of Mill is only apparent. In fact, deduction from first principles, Mill's method of difference, the ideal RCT and a number of other methods (such as instrumental variables and related method defended by proponents of 'Mostly Harmless Econometrics', Angrist and Pischke 2008) are all of a kind Nancy Cartwright calls 'clinchers' (*e.g.*, Cartwright 2007). All clinchers have in common that their conclusion is certain, given the assumptions (these methods can be said to 'clinch' their conclusions). The model is the deductive proof: if all the assumptions are true and no mistake was made in the derivation, the conclusion must be true.

What matters here is not so much that the conclusions are certain given the assumptions but rather that they are established 'mechanically', with as little subjective judgement as possible. In consequence, results are established independently from the team of economists doing the experiment or derivation and in a way that is transparent to everyone who bothers checking them. Especially transparency is often used as a selling point for RCTs (Cohen and Easterly 2009: 18):

> If politicians can be convinced of the benefits of a particular policy or program for their constituents, they may be willing to adopt it, particularly if the results are presented as rigorous and transparent.

Mechanical objectivity contrasts with a method of 'considered judgement' (*cf.* Elgin 1996). A *considered* judgement about a scientific hypothesis involves taking into account all the evidence relevant to the assessment of the hypothesis, which requires judgements about relevance, about the quality of the evidence, about weighing difference pieces of evidence, about the amount of evidence needed to accept or act on the hypothesis, about whether or not additional evidence should be sought in light of the cost of doing so and so on. Many of these judgements do not follow strict rules and are therefore 'subjective' in the eyes of some. This concerns especially certain value judgements that are required, for instance, to determine how much evidence is 'enough', and to which we will now turn.

3.3 Objectivity in Ethics: The Social Scientists Qua Scientist Makes No Value Judgements

There is a third 'traditional' sense of objectivity. The first two, which we may refer to as 'product objectivity' and 'process objectivity', respectively, can be found as much in natural as in social science. The third sense originates in social science and has to do with the fact that social science concerns human action and human action cannot be understood without evaluations.

This sense goes back to Max Weber and his widely cited essay '"Objectivity" in Social Science and Social Policy' (Weber [1904] 1949). In this essay Weber argued that the idea of an aperspectival social science was meaningless (72/81):

> There is no absolutely objective scientific analysis of... 'social phenomena' independent of special and 'one-sided' viewpoints according to which – expressly or tacitly, consciously or unconsciously – they are selected, analyzed and organized for expository purposes.
>
> All knowledge of cultural reality, as may be seen, is always knowledge from particular points of view.

The reason for this is twofold. First, reality is too complex to admit of full description and explanation. So we have to select. But, perhaps in contraposition to the natural sciences, we cannot just select those aspects of the phenomena that fall under laws and treat everything else as 'unintegrated residues' (73). This is because, second, in the social sciences we want to understand social phenomena in their individuality, that is, in their unique configurations that have significance for us.

Values thus solve a selection problem. They tell us what research questions we ought to address because they inform us about the cultural importance of social phenomena (76):

> Only a small portion of existing concrete reality is colored by our value-conditioned interest and it alone is significant to us. It is significant because it reveals relationships which are important to use due to their connection with our values.

It is important to note that Weber did not think that social and natural science were different in kind. Social science too examines the causes of phenomena of interest, and natural science too often seeks to explain natural phenomena in their individual constellations. The role of causal laws is different in the two fields, however. Whereas establishing a causal law is often an end in itself in the natural sciences, in the social sciences laws play an attenuated and accompanying role as mere means to explain cultural phenomena in their uniqueness.

Nevertheless, for Weber social science remained objective in some way. By determining that a given phenomenon is 'culturally significant' a researcher reflects on whether or not a practice is 'meaningful' or 'important', and not whether or not it is commendable: 'Prostitution is a cultural phenomenon just as much as religion or money' (81). An important implication of this view came to the fore in the so-called '*Werturteilsstreit*' (quarrel concerning value judgements) of the early 1900's. In this debate, Weber maintained against the 'socialists of the lectern' around Gustav Schmoller the position that social scientists qua scientists should not be directly involved in policy debates because it was not the aim of science to examine the appropriateness of ends. Given a policy goal, a social scientist could make recommendations about effective strategies to reach the goal; but social science was to be value-free in the sense of not taking a stance on the desirability of the goals themselves.

Schmoller, of course, demurred. To him the issue was one of making economics more ethical and at the same time making ethical debates more scientific. But it is important to note that he did not think that it would be a good idea to load economics with class or group or individual-specific interests. He believed, rather, that ethically sensitive social problems, especially those concerning economic justice, had demonstratively better and worse solutions and that economists were in a good position to help finding the better ones (Nau 2000). For him, there was therefore no conflict between acting as an economist qua economist and contributing substantively to political debates at the same time.

Many if not most contemporary economists stand firmly on Weber's side in this controversy. For a concise statement of the current mainstream view on the issue, consider Faruk Gul and Wolfgang Pesendorfer's widely cited paper (Gul and Pesendorfer 2008: 8):

> However, standard economics has no therapeutic ambition, i.e., it does not try to evaluate or improve the individual's objectives. Economics cannot distinguish between choices that maximize happiness, choices that reflect a sense of duty, or choices that are the response to some impulse. Moreover, standard economics takes no position on the question of which of those objectives the agent should pursue.

4. Why Objectivity?

This is not the place to resolve these debates. Just to take a stance, I do think that compelling arguments have been made on the side of those who reject the respective ideal of objectivity in all three cases. First, even if there were an abstract economic reality behind the phenomena, we would never know it because economic factors tend not to combine additively but rather interactively. Therefore, the 'law' that tells us what any given factor does in isolation from all others or when all others are held constant is not very informative about what that factor does when others are present or not constant. For policy purposes we should then study phenomena that are as close as possible to the policy situation we wish to change or bring about (Reiss 2008: Ch. 5). Second, there's a lot wrong with mechanical methods, and much has been said about this by people eminently more qualified than I am (Blalock 1991; Scriven 2008; Cartwright 2007; Deaton 2010). Some of the problems are: mechanical methods tend to be able to address only narrow sets of issues, a predicament which often results in changes to the research question from one we want to answer to one we can address using the method; RCTs in particular are costly and may affect the populations studied; in social science, blinding – one of the major benefits of RCT methodology – is seldom an option; average results may mask dramatic differences in effect size among sub-populations; etc. Third, few learned men and women would any longer agree with Milton Friedman's dictum that differences about values are 'differences about which men can ultimately only fight' (Friedman 1953), and would rather turn to Amartya Sen who argues that rational argument concerning ethics is possible (Sen 1970). Moreover, economists are often in a privileged position to contribute to ethical debates (Atkinson 2001).

Rather than going through these arguments again what I want to do here is ask why we want a science that is objective in the first place. The first, and in my view most plausible, candidate is truth. For scientific realists, truth is an important (or the most important or the only) aim of science and there is a case to be made that only objective science – in the three-pronged sense of objective – can lead us to the truth.

The best argued and most coherent view of economics along these lines stems from Carl Menger. He argued that the aim of theoretical economics was to establish the laws pertaining to the 'strict types' of the economic world, which could never be given in empirical circumstances because empirical types are always sullied by impurities (Menger 1963[1887]):

> For even natural [as opposed to social] phenomena in their "empirical reality" offer us neither strict types nor even strictly typical relationships. Real gold, real oxygen and hydrogen, real water—not to mention at all the complicated phenomena of the inorganic or even of the organic world—are in their full empirical reality neither of strictly typical nature, nor, given the above manner of looking at them, can exact laws even be observed concerning them.

In order to gather the truth, then, we have to depart from the phenomena and consider the strict types which are revealed to us by reasoning. These types and their strict laws, discovered ('mechanically') by deduction from first principles, describe the economic aspect of the phenomena of society. Ethics has no place in this vision of economics because it concerns the evaluation of states of affairs. But the economic laws are there whether or not we like them.

The problem with this vision, coherent as it might be, is two-fold, epistemic and practical. The epistemic problem is that there is no way to arbitrate between different views on the matter of 'economic truth'. Menger starts his *Principles* with a definition of (economic) goods in terms of human needs, a thing's ability to satisfy a need, the needy person's knowledge of this ability and her command over it (Menger 1950[1871]). This is not an innocuous definition of economic good. Menger's objectivist conception of needs differed from the other neoclassical economist's subjectivist conception of wants or preferences. Neoclassical economics ignored issues of information and knowledge until very recently. Nor is 'having command over a thing' something about which most neoclassical economists would worry a great deal, but then, on a Mengerian conception of economics, how could we tell who is right?

The practical problem is that predictions concerning empirical phenomena depend on our knowledge of the strict laws of all aspects of the phenomena. So even if we knew the economic aspect for sure (but see above), we'd have to know every other aspect plus the (meta-)law for combining aspects. This is clearly a tall order but also one that may face in-principle obstacles, *viz.*, when aspects do not combine in a neat lawful way.

The next candidate for why we want objectivity is that it may guarantee us empirical adequacy. But this is not the case. As pointed out above, there is no guarantee that economic factors add linearly. To the contrary, there is much evidence to the effect that what a factor does depend on the constellation of all other factors. Therefore, the method of analysis and synthesis described above in Section 3.1 is unlikely to be successful. It certainly doesn't guarantee success. An RCT is, in a sense, a 'holistic' method. If, say, we want to learn whether handing out bed nets for free, for a symbolic price or at cost is the most effective strategy to reduce malaria infections, we implement these strategies in affected population's otherwise unaltered habitat. We do not seek to establish what bed nets do to malaria infections in isolation from all other factors. But the RCT guarantees its result only under highly restrictive assumptions which, in most applications cannot be known to have been met. And there certainly have been many failures of policies established on the basis of RCTs.

Arthur Fine argues that we want objectivity, specifically procedural objectivity, in science because we want to trust scientists and their results: 'Where the process of inquiry has certain built-in procedural features ("safeguards", we sometimes call them) we are inclined to trust it more than we would a procedure that fails to have those features' (Fine 1998: 17). It has been argued, for instance, that RCTs in medicine, while certainly no guarantor of the best outcomes, were adopted by the U.S. Food and Drugs Administration (FDA) to different degrees during the 1960s and 1970s in order to regain public trust in its decisions about treatments, which it had lost due to the thalidomide and other scandals (Reiss and Teira 2013; Teira 2010). To rid science of values can, similarly, be defended by appeals to attempts to ensure the trustworthiness of science. If social and moral values are personal, and this assumption is standardly made by economists (recall Lionel Robbins' 'If we disagree about ends, it is a case of thy blood or mine – or live and let live... But, if we disagree about means, then scientific analysis can often help us resolve our differences', Robbins 1932: 150), one will have a hard time trusting in claims advanced by someone whose values one does not share. One might of course disagree on factual claims too but these disagreements, or so the story goes, can be settled on the basis of evidence.

But here comes the snag: the Financial Crisis has shown, if nothing else, that people do not trust economics. If even *The Economist*, a magazine not normally known for Marxist leanings or otherwise hostility to economics, blames the Crisis on a failure of (macro and financial) economics (*The Economist* 2009, see below), we clearly have to worry about trust in the discipline.

What the Crisis has shown is that objectivity in the three-pronged sense hasn't worked: economics has been criticised for being objective in all three senses in its wake. So what might be alternative strategies to win back trust in economics?

4. What's Wrong With Economics Experts?

Suppose for a moment we gave up objectivity in the three-pronged sense. In the resulting economics expert judgement would play a much more overt role than in traditional objectivist economics. If there is no privileged 'economic aspect' of phenomena one can abstract from everything else and theorise about in isolation, someone has to make a selection of which features of the experienced phenomena that are worth theorising about. If mechanical methods of inference fail, judgements about relevance and importance, quantity and quality of evidence that go into an inductive argument have to be made. If economics cannot be value free, someone has to decide which values to use in the process of scientific investigation. And who might this someone be? 'The economics expert' is the answer that suggests itself.

But it would clearly be preposterous to assume that giving a more prominent role to expertise would by itself reinstitute trust in economics. If *The Economist* laments 'that macro and financial economists helped cause the crisis, that they failed to spot it, and that they have no idea how to fix it', the magazine does not refer to an impersonal behemoth called 'economic science' that churned out the wrong kinds of predictions in the run-up to the Crisis but rather to economics experts who used the wrong theories or methods or assumptions, or used them in wrong ways.

The point is, we don't just need a more prominent role for expertise in economics, we need a better kind of expertise. Existing economics expertise is very heavily influenced by objectivist thinking. Let me give two apparently extreme but not untypical examples. The first is from an interview of Eugene Fama, Chicago economist and hero of financial economics (or so I was taught when I read finance), with *The New Yorker's* John Cassidy. Fama not only says that the Crisis left the efficient-market hypothesis unscathed, he also makes the following rather astonishing remarks (Cassidy 2010):

> *Cassidy: Many people would argue that... there was a credit bubble that inflated and ultimately burst.*
>
> *Fama: I don't even know what that means. People who get credit have to get it from somewhere. Does a credit bubble mean that people save too much during that period? I don't know what a credit bubble means. I don't even know what a bubble means. These words have become popular. I don't think they have any meaning.*

> *Cassidy: I guess most people would define a bubble as an extended period during which asset prices depart quite significantly from economic fundamentals.*
>
> *Fama: That's what I would think it is, but that means that somebody must have made a lot of money betting on that, if you could identify it. It's easy to say prices went down, it must have been a bubble, after the fact. I think most bubbles are twenty-twenty hindsight. Now after the fact you always find people who said before the fact that prices are too high. People are always saying that prices are too high. When they turn out to be right, we anoint them. When they turn out to be wrong, we ignore them. They are typically right and wrong about half the time.*
>
> *Cassidy: Are you saying that bubbles can't exist?*
>
> *Fama: They have to be predictable phenomena. I don't think any of this was particularly predictable.*

Thus, there hasn't been a bubble in the real-estate market prior to the Crisis. Perhaps this is an extreme case, but it's not uncommon that economists perceive phenomena through abstract theory – which, after all, describes the reality behind the phenomena. The second example concerns ethics. In his presidential address to the Eastern Economic Association, Harvard economist, one-time chairman of the Council of Economic Advisors and hero qua author of several undergraduate textbooks, Gregory Mankiw argues, quite amusingly, against a utilitarian foundation of optimal tax policies by deriving an apparent absurdity from it: an optimal tax system in which taller people pay higher taxes for the same income levels than shorter people (one reason is that height is an indicator of productivity). He calls his alternative moral foundation a 'just deserts theory' and circumscribes it as follows (Mankiw 2010: 295):

> That is, each person's income reflects the value of what he contributed to society's production of goods and services. One might easily conclude that, under these idealized conditions, each person receives his just deserts.

In equilibrium, and let's assume we are in equilibrium, every person receives his or her marginal product, this is just what he or she deserves, and under a just deserts theory this is a fair outcome. The problem is only that this 'theory' ignores centuries of ethical argument. Let me give one instead of many. Individuals' incomes are usually determined by bargaining (whether at the individual level or that of groups such as workers in an industry). And, as Adam Smith, an ethically more sensitive economist, pointed out, the outcome of bargaining processes depends on a host of factors other than people's contributions such as the availability of outside options, the number and education of the individuals on each side of the bargain, their relative wealth, what they know and so on (Smith 1776[2008]: Book I, Ch. 8). Without an analysis of the conditions under which a bargain (or any exchange for that matter) is struck, one should not assume that people get what they deserve.

Paul Feyerabend didn't like scientific experts very much, for the reasons alluded to here (Feyerabend 1999). To him, an expert is primarily a specialist, one who tries to achieve excellence in an exceedingly narrow field at the expense of what he calls a 'balanced development' – *some* knowledge of a variety of disciplines and fields such as history, philosophy, classics, the arts, pop culture and so on. Feyerabend describes living in a society in which experts (in this sense) have much influence on politics and society with his characteristic fervour (*ibid*.: 119):

> Now, the peculiar situation in which we find ourselves today is that these inarticulate and slavish minds have convinced almost everyone that they have the knowledge and the insight not only to run their own playpens, but large parts of society as well, that they should be allowed to educate children, and that they should be given the power of doing so without any outside control and without supervision by interested laymen. [...] Should we allow a bunch of narrow-minded and conceited slaves to tell free men how to run their society?

I don't know about conceited, but one can hear that current economic experts tend to be too narrow-minded for the discipline's own good hither and yon. Our own Gregory Mankiw admits this much (Mankiw 2010: 285-6):

> I should say at the outset that the issues I will discuss with you here involve not only economics but also some political philosophy. Because I am not a political philosopher by training, I hope you will forgive me if my occasional philosophical ruminations seem like those of an amateur. If I am right that the issue of redistributive justice will be at the heart of the coming policy debate, it will be hard to leave the topic to the philosophical experts. And in light of the inextricable linkages between philosophy and economics that characterize this topic, I hope it is possible that those experts might learn something from humble economists like me.

What Mankiw says about himself, other economist note about the profession: economists don't read enough history (Hoover 2012: 3), not enough humanities (McCloskey 2003: 167ff.), not enough ethics (Atkinson 2001). Far be it from me to suggest that economists are universally ignorant about these topics. But what one can say with some confidence is, I believe, that 'reading widely' is not a value young economists' are taught in graduate schools, certainly not in the leading ones. This is widely perceived to be a deficiency in the economics curriculum, for instance among employers of economists (Coyle 2012).

Let me give just one more piece of evidence. One way to read Mankiw's admission of ignorance of political philosophy is as displaying the virtue of modesty. An alternative is to read it as a display of the vice of arrogance. Here we have a world-class economist who, on the one hand, believes that there are 'inextricable linkages between philosophy and economics' but, on the other hand, does not see the need to read up on what philosophers (and economists! – see *e.g.* Buchanan 1975) have said about the topic. At a recent conference I attended a very prominent economist (I cited one of his works here, but this is the only clue you will get) mocked my use of the word 'ontology' – it being a word he does not understand. Lesson: many in economics regard ignorance, and not reading widely, as a virtue.

5. Economics Experts: Which Ones Should You Trust?

There is an alternative conception of objectivity which contrasts sharply with the above. This conception is more pragmatist in character and thus starts from the end result and works its way back. Now, as we have seen, the purpose of doing science in an objective fashion is to gain trust. The alternative conception regards any trust-making feature of science as objective (Fine 1998: 18). That is, anything goes – as long as the practice promotes trust in science. In contraposition to the three-pronged traditional conception, we may call this conception 'instrumentalism' about objectivity.

Instrumentalism turns the question what features of the process of scientific investigation count as objective into an empirical and contextual one. It is empirical in that anything that stands in the right kind of causal relation with public trust will count as an objective feature of science. There is no way to tell a priori what these features might be. It is contextual in that there is at least the possibility that these features vary with time, place, discipline and other contextual elements. It may well be, for instance, that the three-pronged traditional understanding once has promoted trust in economics, even if it no longer does so. It may also be that it is these features that promote trust in other sciences. And it may be that trust-making features vary with social and political circumstances – different features may be salient in different stages of development or between war and peace times and so on.

It is thus in the nature of the instrumentalist conception of objectivity that there is no answer to the question, 'What is objectivity?' without an empirical study of the question 'Which kinds of experts do you trust?'. To end this paper, I will nevertheless make a number of five, fairly mundane, recommendations of strategies that might be tried for success.

Strategy 1: Read More Widely

The great economist (and philosopher and legal scholar) Friedrich Hayek once said, 'But nobody can be a great economist who is only an economist – and I am even tempted to add that the economist who is only an economist is likely to become a nuisance if not a positive danger' (Hayek 1956). To find out what else an economist needs to be in order not to become a nuisance, consider another great economist (and philosopher and statesman), John Maynard Keynes (Keynes 1925: 12; quoted from Colander 2012: 11):

> The paradox finds its explanation, perhaps, in that the master-economist must possess a rare combination of gifts. He must reach a high standard in several different directions and must combine talents not often found together. He must be mathematician, historian, statesman and philosopher – in some degree. He must understand symbols and speak in words.

That an economist must also be a mathematician hardly needs defence these days. Still, two comments are in order. First, as David Colander argues, there are some reasons to believe that economists need more mathematics rather than less *(ibid.*: 12). To model economies more adequately, an economist must understand how complex non-linear systems of heterogeneous agents work – which cannot be done using the simple linear dynamic models en vogue today. Second, and relatedly, economists need to use simulations models more than they currently do (Reiss 2011). Simulations are a lot more flexible tool than analytically solvable mathematical models and can more readily capture the behaviour of complex systems such as economies.

Through the study of economic history, the economist will gain some perspective, fill law schemata with empirical content and, in particular, understand that things could have been different. Richard Jones singlehandedly refuted a number of doctrines from Ricardo and Malthus that had become received wisdom at the time by simple appeal to the facts of history. For instance, he showed that Ricardo's universal definition of a rent relationship between a landowner and his tenant in fact only applied to England, Holland and parts of the United States at the time of his writing and that elsewhere and at other times other kinds of relationships prevailed. Similarly, he showed that Malthus's population principle wasn't even true of England's own history by comparing the contemporary state of population growth and food supply with that of pre-Elizabethan times (Snyder 2011: 124).

An economist has to be a statesman in at least two senses. She first has to know the institutional landscape at home and abroad. Even the most widely applicable economic principles hold only on account of an underlying institutional structure, and differences in institutional structure afford differences in causal relationships. Moreover, individuals act within many kinds of strictures. If a prediction of an economic outcome is to have any chance of success, it therefore must take into account these strictures.

Second, economics isn't (or shouldn't be) a science that discovers truths for the sake of truth, and even less one that promotes the interests of a small élite. The economist as statesman should know of the social problems of her time and aim to discover exactly those truths that help to resolve these problems.

Finally, the economist has to be part philosopher to understand that there is more to scientific method than deduction from first principles and RCTs as well as to follow the relevant ethical debates.

Rather than being a specialist in one thing, the economics expert who knows a little bit about everything will command public trust to the extent that the public realises that there is no one talent that makes for a good economist and that knowledge of institutional and historical facts is as important as that of economic principles and the ability to make reasoned value judgements.

Strategy 2: Invite Non-Economists to Economics Experts Committees

Not everyone can be expected to know everything and have all the required talents. Nor is this necessary. Many problems require interdisciplinary collaborations, so depending on the problem, one can import the expertise asked for. Unfortunately, economists tend not to like to talk to non-economists. But they should.

There are areas that are more obviously interdisciplinary. Economists, psychologists, philosophers and political scientists all engage in the study of well-being and its measurement for instance. But non-economists tend to be suspiciously absent from expert committees when it matters. The Advisory Commission to Study the Consumer Price Index or 'Boskin Commission' after its chair Michael Boskin, for instance, had exclusively economists as members. But a price index measures the changes in the cost of living, and well-being experts other than economists surely have to say something about that (Reiss 2008: Ch. 2)?

Similarly committees such as the Council of Economic Advisors would gain in credibility if its members had a broader range of expertise. Why not invite a financial historian? A philosopher? A legal scholar?

Strategy 3: Put Your Values On the Table

I used Mankiw's presidential address above as a example of economists' ignorance of discussions outside of economics and their taking pride in it. But Mankiw's discussion of optimal taxation policy is quite exemplary also in a different, a positive sense. Mankiw does not pretend that the topic can be studied in a way that is free of value judgements. To the contrary, he analyses the value framework of the standard approach, offers an alternative, and argues in favour of his alternative by pointing to some apparently untenable consequences of the standard view. His own view might, too, be untenable, but he does not hide it behind a tangled mass of apparently value-neutral mathematics.

Mankiw is quite unusual in this respect. Standard welfare economics is taught and usually presented as though it was a continuation of positive economics and did not involve substantive normative commitments at all (Gul and Pesendorfer 2008). At any rate, the normative commitments that are being made are seldom explicitly discussed (Atkinson 2001).

The situation is worse in positive economics. Perhaps this point is more controversial, but strong arguments to the effect that value judgements are all over the place in positive economics are not difficult to make (Reiss 2008, Reiss 2013). And these do certainly get swept under the rug.

Hiding where one is coming from is not a good recipe if the goal is to inspire trust. The – obvious – alternative is, much like Mankiw, to make ones normative commitments explicit in order to allow critical scrutiny of these commitments by others, other experts and laypeople.

Strategy 4: Distinguish Value-Ladenness from Interest-Ladenness

Now, there are of course reasons why economists (and other scientists) prefer science to be value-free. One of the reasons is that science is different from partisan politics. Scientific results should serve as neutral arbiters between different positions and serve politicians of all colours, not just those who happen to agree with the scientist.

But science – economics – can play this role without being value free. To be value-laden does not necessarily mean to be laden with the interests of particular groups. There are various mechanisms that would help to make the difference clear. One might, for instance, derive a range of results under different sets of assumptions expressing different normative frameworks. Each of the derivations would be value-laden, but the economist would not necessarily present a result or policy that best agrees with her own convictions. Or one might subject the normative assumptions one must make for deriving results to public debate. Rather than using one's own views, the public would feed directly into the determination of the normative commitments.

Strategy 5: Admit that Rational Disagreement is Possible, in Matters of 'Fact' as Much as Concerning 'Values'

Among economists the view prevails that disagreements about values are, essentially, disagreements of taste and therefore cannot be rationally resolved whereas disagreements concerning facts can, at least in principle, be resolved by attending to empirical evidence. Let me quote Milton Friedman once more, as he makes the point so vividly (Friedman 1953):

> I venture the judgment, however, that currently in the Western world, and especially in the United States, differences about economic policy among disinterested citizens derive predominant from different predictions about the economic consequences of taking action – differences that in principle can be eliminated by the progress of positive economics - rather than from fundamental differences in basic values, differences about which men can ultimately only fight. An obvious and not unimportant example is minimum-wage legislation. Underneath the welter of arguments offered for and against such legislation there is an underlying consensus on the objective of achieving a "living wage" for all, to use the ambiguous phrase so common in such discussions. The difference of opinion is largely grounded on an implicit or explicit difference in predictions about the efficacy of this particular means in furthering the agreed-on end. [...] Agreement about the economic consequences of the legislation might not produce complete agreement about its desirability, for differences might still remain about its political or social consequences but, given agreement on objectives, it would certainly go a long way toward producing consensus.

It is quite ironic that today, sixty years on, there isn't an iota more consensus on the issue than at the time Friedman wrote, despite quantum leaps in both econometric technique and the availability of data. And the disagreements are not, or not only, about 'fundamental differences in basic values'.

There are at least two reasons to expect disagreements to persist. One is the entanglement of fact and value mentioned above. A disagreement about underlying values can therefore lead to a disagreement about the facts, even when data and observations are shared. The other is the complexity and evanescence of economic phenomena and the pace with which political agendas change. Even if we have much better econometric techniques than we had sixty years ago, they all work only against a backdrop of assumptions, many of which will seldom if ever be met. How different methods apply to non-ideal cases is always a matter of judgement and can seldom be resolved entirely unambiguously. Moreover, even relatively unambiguous cases will require a great deal of time for their resolution, and time is a scarce resource in the policy arena.

Economists should admit that rational disagreement is possible simply to show respect for other people. However, it is also likely to promote trust in their science. If even enduring topics such as the costs and benefits of minimum wages cannot command consensus in economics, to deny rational difference of opinion amounts to admitting one's own irrationality. And who would trust a science whose practitioners think of themselves as foolish?

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