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# Economic methodology: an inquiry

SHEILA C. DOW

# ECONOMIC METHODOLOGY

## AN INQUIRY

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For my mother, Elizabeth Anderson
who has shown by example that women can be a
force for good outside the home as well as within.

# PREFACE

This book is addressed to those who are interested in reflecting on economics as a discipline, and are curious as to what the field of methodology has to offer. Methodology is the field which is concerned with the foundations of economics: what the role of foundations is, what is meant by foundations, and what they might consist of. It provides a framework within which we can discuss a range of issues which are important for modern economics—why economics is the way it is, what are its limitations, and what are its possibilities; whether or not diversity within economics is to be welcomed; whether or not economics is moving in a constructive direction; and so on.

There is a variety of perceptions and misperceptions of what is involved in methodology. For some it is what in the methodology field is referred to as 'method'—which tool to use for which purpose, within a particular methodological framework. But methodology is concerned more with the framework within which particular methods are chosen. It requires us to dig deeper. For others methodology is associated with rule-setting from outside economics. It is a set of principles for good science; if we don't follow them, we are not good scientists. The reaction to this role for methodology tends to be either to ignore it or to argue vociferously against it. In fact it is a lively area for debate within the field itself, what degree of guidance it is legitimate for methodologists to offer to practising economists.

The fact is that methodology is not the finger-wagging exercise which many mistake it for. While over twenty years ago there was more emphasis on interpreting rules for good science, to apply them to economics, now there is much more engagement with economics itself. Much of modern methodology in fact simply aims to build up a methodological account of what economists do. But there are also issues to address, such as how economics relates to other disciplines, how economic theory can best be constructed in order to generate policy advice, and so on. It is a rich field with active debates, wide-ranging arguments, and new developments occurring all the time. It certainly includes some criticism, but then a critical attitude, it could be argued, is a common feature of scientific activity. Because methodology now attracts such increasing numbers of scholars, and has built up its own institutional structure as a field (organizations, journals, conferences, and so on), methodology is a very lively area. But this specialization also brings its costs, in that methodologists increasingly talk an internal language which is in danger of excluding non-methodologists.

Which is where this book comes in. There are many more detailed treatments of methodological subjects than is offered here. The aim here is simply

to provide enough of an overview of methodology to give you an idea of what the field looks like, and to suggest, as we proceed, where you can follow up on particular questions of interest. So we spend more time on the reasons for drawing on methodology, and its implications, than do most methodology books. There is also an attempt at giving a flavour of the issues which methodology is concerned with and what the current areas of dispute are.

The book can be used as an introductory text for methodology teaching, as an entry point to the specialized methodology literature; each chapter ends with suggestions for further reading. It can also be used as supplementary reading for core theory or econometrics courses, to provide some guidance as to how to address the issues which arise in theory development and empirical work. Theories and econometrics do not appear like rabbits out of a hat—methodological principles (whether implicit or explicit) have been used to determine the direction and form of development, and how these developments are regarded. Some prior knowledge of economics is required if the methodology applications are to be understood, so that it is only suitable for advanced undergraduate courses, or postgraduate courses.

However, I would hope that the book would also have some appeal to colleagues who are curious as to what has been happening in economic methodology and how it relates to their practice. Or it could appeal to those who sense that there are some fundamental issues facing economics and are looking for some kind of framework within which to address them.

I became interested in the subject of methodology myself because I found it difficult otherwise to make sense of many of the debates within economics; there seemed to be something unacknowledged under the surface which was what the real arguments were about. Further, I was disturbed to find methodological statements being used to exclude some types of economics, for reasons which were not readily apparent; how is it established what is and what is not 'proper' economics? So my interest in methodology has always been from the perspective of practice, and in the spirit of inclusion rather than exclusion; this will be evident in the way that methodology is discussed here. Indeed, it should be made clear at the start that there is no neutral way of approaching methodology. The thinking (which we will be exploring) which led to the view that there was no neutral set of rules for economics also applies to methodology. But, as with economics, the best way of dealing with this is to be explicit about our own views, to be aware that there are other points of view, and to be open-minded.

I have benefited tremendously, in preparing this volume, from discussions with, listening to and reading the work of, many of those who work in the field of methodology, as well as many practitioners of economics over the years. Those who have helped along the way are too numerous to mention. But I would like to acknowledge in particular Victoria Chick and the Press's readers,

who all commented on several chapters. I am grateful too to the many honours students at the University of Stirling, and those in the Scottish Doctoral Programme, on whom I have tried out some parts of the material presented here. They were supportive in their enthusiasm and challenging in bringing their own ideas and questions to bear. I would also like to express my appreciation to Brendan George, formerly of Oxford University Press, who had the original idea for the book and provided support and encouragement in the early stages, and to Matthew Cotton who subsequently nursed the project on its way. Finally, I would like to express my fond appreciation for the immeasurable contribution made by my husband Alistair. In addition to providing comments on several chapters, it was he who first introduced me to methodology many years ago. He has contributed more than he knows since then by many discussions along the way.

S.C.D.

# CONTENTS

# 1

# WHAT'S SO FUNNY ABOUT ECONOMICS?

Many a true word is spoken in jest. So we start this inquiry into economic methodology by looking at economist jokes.

Although economics is often referred to as the 'dismal science',[1] economists like a joke as much as anyone, and there are many 'insider' jokes by which we make fun of ourselves. We use jokes to caricature what we regard as distinctive about economics. There are other kinds of jokes which economists tend to find less funny—the 'outsider' jokes which non-economists make to express their discomfort with economics. Here we pick two of each kind of joke to get an idea, first, of economists' own caricature of ourselves, and, second, of the way others caricature us.

*Three unfortunate people are shipwrecked—a physicist, a chemist, and an economist—and find themselves cast up on a desert island with little prospect of finding food. A tin of baked beans is also washed up onto the shore, but the problem is how to open it.[2] The physicist sets up a system of levers with stones and branches to exert pressure on the tin, but to no avail. The chemist tries a solution made from boiling a selection of barks and leaves, but that doesn't work either. The economist, who has been standing by all this time, smugly offers the solution: 'Let's assume we have a tin-opener.'*

Yes, economists are very conscious of the fact that their theories, and applied work, are based on simplifying assumptions. Theory by definition requires simplifying assumptions—otherwise it would amount only to description.[3] These assumptions inevitably make economics unrealistic in some sense. At the same time we want economics to tell us something about the real world. The same need for simplifying assumptions surely applies also to disciplines like physics and chemistry. But are economists' assumptions distinctive in some way? Is it simply that we are more aware of them because they apply to human behaviour—something with which we are all more intimately familiar than we are with inanimate objects? Whether making assumptions raises issues peculiar to economics or general to all science, it is

clearly such a central feature of economics that we will want to think about it more carefully.

*An economist is on a flight from New York to London in a four-engined plane. Some way into the flight there is a muffled bang, the plane drops, and the pilot announces that one of the engines has failed. The passengers are not to worry, but their arrival in London will be delayed by half an hour. Some time later the same thing happens again—a second engine has failed. The pilot again reassures the passengers, but warns of a delay now of an hour and a half. Later, incredibly, a third engine fails; now the delay will be five hours. The economist turns to the passenger in the next seat: 'At this rate, if the last engine goes, we'll be up here all night.'*

If economics is to be a guide to the future (for governments, for companies, for households), then there has to be some way of drawing on our past experience. After all, past experience is all we can go by. But how we extrapolate from the past raises challenging problems. How many readers, while reading this version of the joke, started wondering (like myself, while writing it) whether the time of travel would really correspond in this simple way to the number of engines lost? The time-delay problem raises the kind of interesting technical questions that distract us from recognizing the dire, irreversible consequences of all engines failing. Are there any serious parallels in economics? Again, since prediction on the basis of past experience is such a central feature of the practice of economics, this joke points to something else we will want to think about further.

But what about jokes made about economists by non-economists? We need to be aware of the way in which others regard economics for a whole variety of reasons, not least that most economists aim to make a useful contribution to society.

*An economist post was advertised in the appointments section of the paper; the ad specified that applicants should be 'one-handed'. A mystified two-handed economist phoned the company to ask for an explanation and was told: 'We're tired of hiring economists who always answer questions by saying "on the one hand . . . and on the other . . . ".'*

As economists, we don't tend to find this joke funny. Of course we answer practical (and even sometimes theoretical) questions in this way. We have to make assumptions, and it may be a matter for debate as to whether they apply to the question in hand. So our answers reflect the implications of making different possible assumptions. We can extrapolate from the past, but developments not addressed by the theory may disrupt the patterns of the past. We cannot rely on the *ceteris paribus* clause (that is, assuming other things being equal) to answer practical questions where there is no guarantee that other things will be equal. So we hedge our answers. Is this, as the joke suggests, a problem? Or is it an inevitable feature of applied economics? We need to think

more carefully about why it is difficult for economists to give more definite answers to practical questions.

*If you pose a problem to ten economists you will end up with (at least) eleven opinions.*

This joke makes a rather different point; not only may any one economist offer at least two opinions, but also economists cannot even agree amongst themselves. Why can't economists come up with the 'right' answer, or at least a consensus as to what the 'best' answer might be? Is disagreement the sign of an immature science; given time will we all converge on the same answers? Or is there something particular about economics which makes this impossible? Indeed might there be virtue in diversity, as there is in nature? We need to think more carefully about why economists disagree, and what they disagree about.

All of these jokes have been around for a long time, suggesting a common thread in the perceptions both of economists and non-economists about the discipline. But in the meantime, economics keeps on changing, putting a new slant on the kinds of issues raised by this sample of jokes as time goes on. In the next chapter we consider this process of change, what is driving it, and why that puts a particular slant on the types of issue raised by these jokes.

## Further Reading

The economics jokes website: http://netec.mcc.ac.uk/JokEc.html

## Endnotes

1. This term is most closely associated with the classical economist Thomas Carlyle.
2. This joke obviously predates baked bean tins with ring-pull tops.
3. Even description itself could be seen to require some assumptions.

# 2

# WHERE IS ECONOMICS GOING?

## 2.1 Introduction

Economics has a long history. In its modern form, it can be traced back to the work of Adam Smith in Scotland and François Quesnay in France in the eighteenth century, although there are plenty of earlier antecedents.[1] So why do we need to address fundamental issues now? Surely these have all been sorted out a long time ago.

The fact is that, even if economists at times consider issues to be close to being resolved, economics keeps changing, raising new issues. A cursory glance at textbooks from one decade to the next shows how both the questions asked and the tools used to answer them have changed over the years. Some would argue that this change is simply a matter of progress. If this is so, by what principles does economics progress, and be seen to progress? As practising economists, are we all aware of what it is we need to do to contribute to that progress? What are the criteria for judging progress? If we proceed by seeing what 'works', what does that mean? And do we all agree on what does work and what doesn't? There are too many questions here for us to take the notion of inevitable progress for granted.

So if we are open to the possibility of change that cannot necessarily be taken for granted as constituting progress, as well as change that occurs for reasons other than progress (by whatever criteria), then there is even more reason for considering what is driving developments in economics, and how we can each help to push it in a positive direction. This chapter therefore considers the forces behind change in economics and the issues that arise from this process. As in Chapter 1, we will draw attention to issues internal to the discipline and those that arise from outside the discipline.

## 2.2 Why Does Economics Keep on Changing?

### 2.2.1 Efforts to improve on existing theories

The force for change that is most obvious from reading articles and textbooks is perceived shortcomings in existing theories. For example, the rational expectations hypothesis was developed because the then-dominant adaptive expectations approach was claimed to be inconsistent with the assumption of rational individual behaviour. Why would individuals who were rational consistently make mistakes?

McCallum suggests that the rational expectations hypothesis:

> has one outstanding strength, namely, the weakness of its competitors. Each alternative expectational hypothesis, that is, explicitly or implicitly posits the existence of some particular pattern of *systematic* expectational error. This implication is unattractive, however, because expectational errors are costly. Thus purposeful agents have incentives to weed out all systematic components. (McCallum, 1980: 718; emphasis in original)

McCallum was right, but right within the terms of the rational expectations framework. Once rationality is defined in a particular way—in this case optimizing behaviour on the basis of complete information (including information about the structural model)—then systematic error would be recognized as such by individuals and would therefore be irrational. But this is not the only possible definition of rationality. The defence of the adaptive expectations approach was that rational individuals would only gradually adjust to new information because of inadequate information about the underlying causal mechanism (see, for example, Laidler, 1981). In other words, what is seen as an improvement to theory depends in this case on a new meaning given to a key concept, rationality. It is normal in scientific thought for terms to change meaning over time. Weintraub (1998), for example, explains how meanings have changed in mathematics. But, unless we can argue that change in meaning is always an improvement by some criterion, it does make identifying improvements in economics in general quite tricky.

It is in fact a common pattern in presenting new theoretical developments for economists to identify the shortcomings in what exists and show how the new theory improves on it. Similarly, an existing theory may be shown to be inconsistent with the evidence. For example, Lucas (1990) points out that standard neoclassical models of growth and trade would imply that the marginal product of capital in developing countries was significantly higher than in developed countries. Why then do we not see consistent capital flows from rich countries to poor countries? His article provides a good case study of how received theory could be modified to incorporate possible explanations for an

apparent anomaly, for example, replacing perfect competition with imperfect competition. He concludes by suggesting that development policy should have a totally different focus—human capital. And indeed new growth theory has done just that.

Another way of trying to improve theory may be to change the way assumptions are formulated, to make them more realistic. Thus, in macroeconomics imperfect competition theory has substantially replaced theory based on the assumption of perfect competition. The field developed more from concerns at the macroeconomic level (such as persistent unemployment) than from a realist approach to microeconomics. But the route taken for finding explanations was to focus on microfoundations, and consider different formulations at that level:

> [M]acroeconomics should be grounded in microeconomic principles . . . the microfoundations from which the aggregate behavior is derived can often be tested directly. A rejection of the underlying micro-hypotheses should suffice to cast doubt on the validity of the derived macro-theory. (Greenwald and Stiglitz, 1993: 24)

It is interesting that most of this literature is theoretical, or employs simulation techniques for empirical testing, rather than demonstrating evidence for the greater realism of the imperfect competition assumption. It seems that imperfect competition is presumed to be a generally accepted characterization of reality, usually without explicit reference to evidence on market structure. Backhouse (forthcoming) suggests that generalized statements about the nature of the economy are in fact characteristic of textbooks, without much reference to empirical data. This contrasts with some other areas where the realism of assumptions is challenged by counter-evidence. Experimental economics, for example, aims to test empirically whether individuals are in fact rational in the sense that we assume in the rational economic man concept.[2]

### 2.2.2 Technical change

While efforts to improve on existing theories are an internal source of change in economics in the line of progress, there may also be an external impetus. The most obvious one is the advance in information technology and the associated advance in the scope for data collection and analysis. Modern macroeconomics, for example, took its character from the new capacity, from the 1940s, to manipulate the increasing numbers of data series that were becoming available. Econometrics itself has undergone dramatic changes as a result of the increasing accessibility, and ease of operation, of statistical packages. Older readers may remember learning regression analysis using log tables. That is light years away from the current situation where highly sophisticated packages can be used with minimal training.[3]

The means by which the technical proficiency of economists improves may

itself be understood in terms of competitive behaviour within economics. Thus Harry Johnson (1971), for example, argued that younger economists recently trained in graduate schools have become accustomed to using new mathematical techniques that many of their elders cannot grasp; they push forward with these techniques in order to improve their career prospects.

Robert Lucas (1980: 701) has specifically referred to the role of technical advances in the analysis of expectations since Keynes's day; he gives as examples the idea that 'one might describe the economy as a system of stochastically disturbed difference equations, the parameters of which could be estimated from actual time series', and the technical capacity to construct a 'mathematically explicit theory of general equilibrium'. He therefore portrays the difference between Keynes's theory of expectations, developed in the 1930s, and his own as being due primarily to technological advance:

> It was a fortunate historical accident that ... technical advances in statistical and economic theory occurred, which transformed 'Keynesian Economics' into something very different from, and much more fruitful than, anything Keynes himself had foreseen. (ibid.)

Note that Lucas is employing a notion of progress in the term 'fruitful'. This is something we need to consider more carefully in what follows.[4]

### 2.2.3 Changing political environment

What we have considered so far are forces for change internal to the discipline. These are perhaps the ones we are most conscious of in the day-to-day business of academic economics. But among practising economists on a day-to-day basis, and for academic economists over the longer term, external forces for change can bring about much more dramatic shifts in the discipline.

One important external impetus for change in economics is the new questions posed by the changing political environment. For example, environmental issues are important now in a way that would have been hard to imagine fifty years ago. Having assumed for years that there was a given endowment of most resources, economists now need to address the issues raised by the real possibility of resource depletion.[5] One of the old puzzles for economists was the water–diamond paradox: why is the price of diamonds so high when the need for them is so low, while, although it is essential to life, water has so low a price? The answer was identified as lying in distinguishing between the total utility derived from water (value-in-use) and its marginal utility (value-in-exchange). The supply of water was thought to be so great that the cost of production was minimal. But now we are increasingly conscious that the supply of water in specific locations, and in particular the supply of water suitable for drinking, is limited. This awareness is reflected, for example, in the fact that we now pay water rates in the UK. The pricing, or