1. Introduction

> The composition of this book has been for the author a long struggle of escape, and so must the reading of it be for most readers if the author's assault upon them is to be successful, — a struggle of escape from habitual modes of thought and expression. The ideas which are here expressed so laboriously are extremely simple and should be obvious. The difficulty lies, not in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds.
>
> John Maynard Keynes[1]

> Problems cannot be solved by the same level of thinking that created them.
>
> attributed to Albert Einstein[2]

Keynes (1936, p. viii) is using a botanical analogy here: the ideas he refers to are 'invasive' and have developed ever more complex root systems throughout the mind. As any gardener knows, it is very hard to eradicate invasive plants that have extensive and complex root systems: the plants just start growing again from the fragmentary roots that remain in the soil.

My objective is to use what we know about the economics of innovation so that we can improve our knowledge of empirical economics – in particular, *rerum cognoscere causas* (to understand the causes of things). The reader will be aware that this book is to some degree critical of the *status quo* in economics. Before I go any further, therefore, it may be useful to the reader if I give a brief summary of where I am coming from, and where I am going.

PRELIMINARIES

The arguments in this book, and the proposals advanced, are based on seven basic beliefs. I shall not attempt to justify these in great detail, as that would require another book. But it may help the reader to know what these are.

1. I believe we can make some important improvements in the way we study empirical economics. In this book, I shall only discuss empirical

economics.[3]

2. My proposal is that we should do things we are not currently doing, or only infrequently doing, in mainstream empirical economics. I am not trying to stop mainstream empirical economists doing what they are already doing, though I shall discuss some problems that deserve their attention.
3. I am confident that pluralism is a good thing in empirical economics. I recognise that pluralism is viewed with suspicion by many mainstream economists, and that some of these fear that pluralism is damaging to the integrity and reputation of an academic discipline. I understand their concerns about pluralism in theory, though I don't share them. But I don't understand why an empirical science should be concerned about pluralism in the ways we gather *empirical evidence*.
4. I believe that the operation of the economy is a good model for the operation of an academic discipline. In an innovative sector of the economy, it is commonplace to find an industrial structure that includes some well-established and large incumbent firms who offer mainstream products and services, and also a fringe of newer and innovative firms who aim to improve on some of the offerings from the mainstream, and perhaps offer something radically different. This is entirely healthy.
5. In the same way, I think it is entirely healthy to create a similar 'industrial structure' in the economics discipline, and a similar fringe where radical innovations can flourish. That is radical, in the sense that it means going back to the roots and doing some things in a different way, but I don't think it is controversial. I understand why some academic disciplines are rather conservative, but I believe it is not healthy for economics to be more conservative than the economy.
6. My thinking is guided by the structure-conduct-performance framework.[4] That framework asserts that changes in the structure of an industry will lead to predictable changes in firm conduct and therefore to predictable changes in economic performance. In the same way, a change in the structure of the economics discipline can help to improve the quality of empirical research.
7. I believe that what the economics of innovation tells us about the value of innovation in company strategies is also relevant to innovation in the context of an academic discipline. One of the greatest thinkers about creativity and innovation, Herbert Simon, made exactly the same assumption in his own work: "I am confident that the foundations of creativity are the same in management as they are in science" (Simon (1985, p. 19).

I should add one more point of clarification. In what follows, I shall use the

terms 'mainstream' and 'non-mainstream' to distinguish two mutually exclusive groups of economists. But why, the reader may ask, do I use the cumbersome term, 'non-mainstream', rather than the more elegant word, 'heterodox'? The reason is this.

An important group of researchers outside the mainstream prefer to call themselves 'pluralist' rather than 'heterodox'. As I understand it, they consider that the non-mainstream community is divided into two mutually exclusive groups: 'pluralists' (who are happy to co-exist with the mainstream) and 'heterodox' (who are not). The implication of this is that if you believe in pluralism, which I do, then you should call yourself pluralist and not heterodox.

I have a slightly different view on this. Firstly, the two adjectives, 'pluralist' and 'heterodox' are rather different in scope. If your research is heterodox, then it is different from the orthodoxy, or mainstream. But to be pluralist is not so much a statement about your own research, as a statement that you believe it is healthy for different researchers to take different approaches. Secondly, while I accept that most pluralists are non-mainstream, there are certainly some pluralists in the mainstream. In the language of the Venn diagram, the set of pluralists is not exclusively a subset of the non-mainstream community: it intersects with mainstream and non-mainstream.

In this book, the distinction between mainstream and non-mainstream is essential. I cannot use the word 'pluralist', as that is not synonymous with non-mainstream. And I cannot use the word 'heterodox', because some believe that term means 'non-pluralist'. Therefore, to avoid confusion, I stick with 'non-mainstream' – *except* when I am quoting directly from other authors.

THREE MAIN STEPS

There are three essential steps in the book, which are discussed in Parts I, II and III. The first is a reappraisal of the status quo in empirical economics: the assumptions that guide empirical research, the most important methods by which it is studied, and what are thought to be the most important qualities we need to develop in empirical researchers. The reappraisal in Part I casts doubt on several elements of current methods, and suggests that some of us, even if not all of us, need to do some things in a different way.

The second step is to consider what sorts of innovation are needed to help move us towards a better empirical economics. Part II reviews some of the essential ideas from the economics of innovation and concludes that for innovation to have the maximum beneficial effect on an economy, we need a

mix of incremental and radical innovations. To solve many of the problems discussed in Part I calls for radical innovations. These radical innovations are not necessarily 'rocket science', and some indeed are quite simple, but they all have one essential thing in common: we must go back to the roots and build something different.

The third step is to describe what specific innovations are required. This is done in Part III. To start with, we need to move away from the idea of empirical economics as a monolithic, or unitary discipline where all research questions are answered by a 'universal solvent', and instead embrace the idea of economics as a federation of semi-autonomous sub-disciplines. Only with that semi-autonomy can we expect to see the research innovations required to develop a better empirical economics. This idea of a federation may be radical in economics, but it is hardly new. Indeed, it is found in many other academic disciplines – notably in medicine.

PART I: RE-APPRAISAL

The next five chapters of Part I consider some specific aspects of current empirical economics that, in my view, most urgently need reappraisal, while the last two chapters summarise some of the other concerns described by those who use or depend on academic economics, whether inside the academy (students and other academic disciplines), or outside (government, central bankers, business and ordinary citizens).

Chapter 2 considers the prevailing view held by most empirical economists that econometrics provides the most rigorous and precise research method available, and this makes it unnecessary to pursue any other empirical research methods. We examine a sample of 2,220 parameter estimates and show that econometrics is nowhere near as precise as econometricians think. In particular, the parameter estimates in this sample are based on very small signal-to-noise ratios. (The Appendix provides the mathematical results necessary for interpreting these data.)

Chapter 3 re-examines the idea, most commonly associated with Friedman's discussion of positive economics, that realism of assumptions used in economic research does not matter, so long as the models 'explain the data'. We argue, as did Hayek, that Friedman's argument is really rather dangerous, and that if it is possible to examine the reality of assumptions, then we should do so.

When economists talk about rigour, they almost invariably mean the pursuit of rigour in logical arguments and in mathematical proofs. But Chapter 4 argues that there are really three distinct sorts of rigour that are important in empirical economics. We call these Arrow rigour, Pasteur

rigour and Mill rigour, after the three great thinkers who are known for their attention to such rigour. On reflection, it is far from clear that Arrow rigour (logical and mathematical rigour) is the most important of the three in empirical economics.

In Chapter 5 we consider the famous maxim due to the scientist, Lord Kelvin, about the desirability of expressing scientific knowledge in numbers. We shall argue that this maxim has been grossly misunderstood by many economists, who use it as a justification for considering only those phenomena that can be expressed in numbers, and for ignoring any factors that can only be described in qualitative terms.

Chapter 6 is, in a sense, the 'flip side' of this typical attitude in empirical economics, which treats qualitative research, such as case studies, as being of little value. We also reappraise some of the myths that surround case study work. We see that some of the things economists say to dismiss case studies make little sense, and in reality, case studies are very valuable.

Chapter 7 considers some of the sources of discontent with economics within the academy. These include criticisms from students and from other academic disciplines. Our summary of the former is based on a book written by three students of economics (Earle et al, 2017). A common complaint from many academics in other disciplines is that, in their relations with the rest of the world, economists don't seem to exploit some of the most important principles of economics, notably: the benefits of trade, division of labour, and competition.

Chapter 8 considers the wider discontent with economics, including some of the criticisms from politicians, government, central banking, business and ordinary citizens. Some have suggested that economics is facing a crisis because it is not held in high regard by the wider public. Some have cited the failure of economic models at the time they are most urgently needed.

The main challenge in acting on this reappraisal is captured in Keynes' famous remark, quoted at the start of the chapter. These ideas are deeply embedded in the economist's training and thinking and were taught to us by some of the most revered figures in the history of economic thought. In view of that, it is not easy to escape from the hold of these ideas. But it is essential that some economists, at least, do escape from these ideas, for otherwise we cannot do what is necessary to create some really secure empirical foundations for economics. I don't imply that these 'old' ideas are necessarily wrong, though some of them are definitely wrong some of the time. Rather, the grip of these old ideas is preventing us pursuing areas of research that we must pursue.

PART II: INNOVATION

As my stated objective is to use what I have learned about the economics of innovation to improve the quality of what we know about the empirical economy, the next step is to explore some of the lessons from the economics of innovation that are most important in the context of this book.

Chapter 9 offers a very simple summary of some of the key ideas from the economics of innovation. For anyone who is familiar with that field, there is probably nothing in this chapter that they would not know already. But others may find it useful. It covers the definition of innovation, where the ideas for innovation come from, and the different roles played by the division of labour, on the one hand, and by the student of everything, on the other, in successful innovation. We describe an essential distinction between incremental innovation and radical innovation and argue that for innovation to have the greatest beneficial effect on the economy, we need a judicious mix of these two types of innovation. It is recognised that the right environment in which to create incremental innovation is usually very different from the right environment in which to create radical innovation.

Chapter 10 applies the ideas from Chapter 9 to explain why mainstream economics offers a very good environment for incremental innovation. We then explore whether incremental innovations are enough to resolve the issues discussed in Part I. For the most part, they are not.

Chapter 11 then discusses why mainstream economics is not, on the whole, very good at radical innovation. We then go on to discuss why, in addition, mainstream economics is also resistant to radical innovation. But the essential challenge that faces us in this book is that we do need some radical innovations in economics to resolve the issues discussed in Part I. The chapter explains four reasons why they are essential. The chapter concludes by discussing the sort of academic environment necessary to deliver the necessary radical innovations as well as the existing incremental innovations. This is the idea of a federation, which contains mainstream economics and some new semi-autonomous sub-disciplines.

PART III: THE FEDERATION

This third part of the book looks at some of the specific innovations that could help to give economics a far stronger empirical foundation, and would help to address many of the issues mentioned in Part I.

Chapter 12 discusses the principal reasons for seeking to emulate medicine, in general, and anatomy in particular. One of the most important reasons is that medicine is not really a single discipline, but a federation of

semi-autonomous sub-disciplines. I shall argue that many of the problems recognised by critics of the mainstream, and by practitioners and those who use economics, are easier to solve if economics also becomes a federation. Chapters 13–19 then describe some of the essential elements that should be found within the economics federation.

Chapter 13 discusses economic anatomy, or the study of structure. This is probably the single most important new component in the economics federation. I shall argue that economic anatomy is, or should be, as important to economics as anatomy is to medicine. And, if it is alarming to think of a surgeon performing an operation without a decent knowledge of the relevant anatomy, it should also be alarming to think of empirical economists who do not have a decent knowledge of the economic anatomy of their particular part of the economy. It seems unlikely that many mainstream economists will want to acquire such detailed 'anatomical' knowledge, but if they don't have it, then it is essential that someone else in the federation does have such expertise.

Chapter 14 discusses economic physiology, or the study of function. While the detailed study of structure in economic anatomy is under-developed, the study of function in economic physiology is comparatively well developed – but we need to stress one caveat. In medicine, physiology depends on a detailed knowledge of anatomy. In economics, that is not yet the case. The study of function in economics is still, for the most part, a black box study. We posit production functions, demand functions, and so on, and try to estimate these using econometric methods, but we make little or no use of detailed descriptive knowledge of what actually goes on within the black box. Some economists in the federation need to take on the task of creating an economic physiology based on detailed economic anatomy.

Chapter 15 discusses economic pathology – the third of the three new core sub-disciplines where the economic federation would emulate medicine. When talking about the possibility of pathology in the economy, it is easy to oscillate between two extreme positions. The first is found in some ultra-free-market philosophies, where it is believed that there are no pathological conditions in the economy. The second is found in the musings of economic critics, who believe that pathological conditions are actually quite common, and perhaps especially so in sectors such as financial services. To resolve this dilemma, we need to develop a precise criterion to identify what makes a pathological condition, and to do this we draw on the relevant literature in medical philosophy.

Chapter 16 considers another aspect of pathology which perhaps needs more attention in economics than in medicine. This is the phenomenon of pathological conditions within the economics discipline. If critics of mainstream economics are right, then the economics discipline shows signs of

sickness more often than it should. A particular area of concern is what we shall call dual pathology: this is where there is a pathological condition in the economy and a pathological condition in the discipline, and these two conditions are inter-related. For example, if public policy is informed by a bad economic theory, then that can create a pathological condition in the economy. Alternatively, if a pathological condition exists in the economy, but the economics discipline accepts it as being quite normal, then that also creates a pathological condition in the discipline.

In the federation, we can expect to find a variety of hybrid sub-disciplines. Chapters 17 and 18 describe two generic types of hybrid that are important for those who wish to solve the problems identified in Part I.

Chapter 17 considers hybrids involving interdisciplinary collaboration. As an illustration, I focus on the interdisciplinary hybrid I know best: the study of innovation. To those who spend most of their research effort studying innovation, there is little doubt that the insights about innovation available to those who follow the hybrid are richer than those available from that segment of mainstream economics concerned with innovation. However, this is not to deny that the mainstream economics of innovation is very important, as it plays an essential role in linking innovation to other aspects of economics.

Chapter 18 considers hybrids involving collaboration with practitioners. As an illustration, I focus on the practitioner hybrid I know best: the development of economic policy towards standards. The ideal team to carry out such work involves a wide variety of practitioners (from industry, government, consumer associations and other groups) and academics (from economics, politics, science and technology).

Chapter 19 reviews the general reasons why other sub-disciplines may be useful within the federation. One that is well developed in medicine, but needs to be better developed in economics, is what I might call the data-reality interface. In medicine, there has been careful study of the accuracy with which tests and diagnostic scans can identify a pathological condition. In general, there is a trade-off between cost and accuracy, starting from inexpensive blood tests, through inexpensive scans (e.g. ultrasound) to the most expensive and precise scans (e.g. MRI).

And finally, Chapter 20 asks whether the federation will survive and considers some of the factors that may undermine it. One factor will certainly be the extent to which mainstream economists are prepared to embrace pluralism. But I conclude that the federation is so important in addressing the problems raised in Part I, that it has to survive in one form or another. If these unfamiliar sub-disciplines are unwelcome in the community of mainstream economists, then they will find a home elsewhere.

In Part III, I shall be travelling far and wide, and cannot offer a detailed account of all these new sub-disciplines in this single book. I shall focus on

the 'big picture' and will not dwell on details. Some readers will be frustrated with this, but I hope they will forgive me. I think that G.H. Hardy's celebrated remark offers me an excuse (quoted in Dyson, 1996, p.43): "Young men should prove theorems, old men should write books."

NOTES

1 Keynes (1936, p. viii)

2 There are several versions of this quotation in circulation, and some doubt about a reliable source for the original remark. All we seem to know is that Einstein said something like this at some point. I include the remark here as it has something very important in common with Keynes' observation.

3 This does not imply that I have nothing to say about economic theory, but I respect the principle that those who criticise an area of research must be very sure of their ground. I think I can pass that test in empirical economics, but not in theory.

4 The structure-conduct-performance (SCP) framework is most closely associated with Bain (1959). It is one of the most durable ideas within industrial economics, and underpins much of competition policy, and other areas of industrial policy.