

REVIEW ESSAY

The econocracy: the perils of leaving economics to the experts, by Joe Earle, Cahal Moran and Zach Ward-Perkins, Manchester, Manchester University Press, 2017, 232 pp., £10.00 (paperback) ISBN: 978-1-5261-1013-8

A crisis in economics?

Introduction

The authors write of their ‘worries and frustrations about the current state of modern economics.’ These concerns include their discontent with the economics curriculum in universities; the absence of plurality in that curriculum; the ways in which mainstream economics is seeking to marginalise heterodox economists; and perhaps above all, the way that graduates of this curriculum are becoming the recruits of an *econocracy*. To understand this neologism, think of a bureaucracy where government administrators are replaced by mainstream economists.

The authors of this book need little introduction to British readers, but those outside the UK may not have heard of them. They are co-founders of the *Post-Crash Economics Society* at the University of Manchester, established in 2012. This society was formed to make a statement about student discontent with the economics curriculum and, in particular, the fact that discussion of the 2007–08 financial crash – one of the greatest economic shocks of our lifetime – was still almost entirely absent from their economics curriculum (see Inman, 2013). This was not the first student society to express such discontent, but it was certainly one of the most influential, and was immensely successful in generating press coverage of their work (see, for example, Inman, 2014; Chakraborty, 2017) and in winning support from many influential people in Britain and other countries.

It is by no means easy to be a dissident student of economics. I know this from my own experiences as a dissident PhD student at the London School of Economics. Referring to the United States, Leontief (1982, p.107) famously remarked that, ‘the methods used to maintain intellectual discipline in this country’s most influential economics departments can occasionally remind one of those employed by the Marines to maintain discipline on Parris Island.’ There is perhaps an element of hyperbole in this remark, although Leontief was certainly not known for hyperbole. But either way, there is no doubt that Leontief – a Nobel laureate in 1973 and one of the greatest of all empirical economists – was profoundly concerned about the state of economics (see, for example, Leontief, 1971, 1982).

To be a dissident, you have to be the ‘unreasonable man’ of G.B. Shaw’s famous maxim: ‘The reasonable man adapts himself to the world; the unreasonable one persists in trying to adapt the world to himself. Therefore all progress depends on the unreasonable man.’¹ And you have to hold on to the idea in the last sentence, that by continuing to be an ‘unreasonable’ dissident, you will eventually make progress in persuading others of your point of view.

As I read this book, I thought back to the student protests that started at the London School of Economics in 1967 and spread to other British universities (see Mair, 2003). Initially, these protests were about teaching, but became overtly political and involved some disobedience. The concerns of the Manchester students, in contrast, were about economics *as a discipline*, and the imbalance in the curriculum between mathematical models and the interpretation of economic facts. I also believe that the 1960s protests generated more heat than light, while the Manchester students have managed to shine a searchlight on the state of economics.

In addition to the fulsome foreword by Andy Haldane, the book carries many endorsements from major figures in the world of economics, and elsewhere. I think these establish, beyond doubt, that this is a very important book, and there is no need for me to repeat that. In this essay, therefore, I want to reflect on this book in the broader debate about the state of the economics profession.

Crisis? What crisis?

In his foreword to *The Econocracy*, Haldane (p.xiii) writes,

... it would not be too much of an exaggeration to say that the financial crisis (of 2007–08) has spawned a crisis in economics and finance. At root, this was every bit as much an analytical crisis as an economic and financial one.

The idea of a ‘crisis’ in economics is not new; consider this from a leading historian of economic thought (Hutchison, 1984, p.1):

For over ten years, a ‘crisis’ in economics, or in economic policy, or economic theory has been widely discussed ... This decade or more of crisis talk followed one of the most extraordinary intellectual booms in the history of the subject, which had lasted through much of the preceding quarter-century, a period of confident pretensions and prestige comparable only with that of the English classical boom of more than a century before.

The phrase, ‘this decade or more of crisis talk’ is a reference to some of the first prominent discussions of a crisis within economics in the Presidential Lectures by Leontief (1971) and Phelps Brown (1972) of, respectively, the American Economic Association and the Royal Economic Society. Indeed, if you follow the critical literature from that time onwards, you find that, in the view of some authors at least, economics has been in a state of crisis ever since.

Haldane has used the word ‘crisis’ on other occasions, most recently in a speech in January 2017 (see Inman, 2017). Shortly after he made the speech, Miles (2017) responded with an article in the *Financial Times*, entitled ‘Andy Haldane is wrong: there is no crisis in economics’. And Wren-Lewis (2017) added his thoughts to those of Miles, concluding that, ‘We should be talking not about a phoney crisis in economics, but why policymakers today have ignored economics ...’. I believe that the main reason Haldane and others speak of a crisis (while mainstream economists deny that there is a crisis) is because the two parties are actually talking about different things. This is what I hope to show in this essay.

Before going further, however, I need to clarify two points. First, in what follows, I shall be considering in what sense *mainstream* economics could be said to be facing a crisis. I shall not be considering whether the same could be said of *heterodox* economics. In limiting my attention in this way, I do not imply that heterodox economics faces no crisis. I simply mean that if heterodox economics did face a crisis, the form of that crisis and the reasons for it would be quite different.

Second, we need an answer to a linguistic puzzle: When does a *problem* become a *crisis*? The word ‘crisis’ has medical origins. In this context, crisis refers to a critical point in the development of an illness when a difficult and important decision *must* be made about remedial action. If the doctor takes the right action, then the patient may recover fully; but if the doctor takes no remedial action, or takes the wrong action, then the patient will suffer permanent damage, or death. Therefore, a medical problem only becomes a crisis if it threatens permanent damage to, or the death of, the patient. In the present context, a problem in mainstream economics becomes a crisis only if it threatens permanent damage to, or the end of, mainstream economics.

Is there a crisis within mainstream economics? Referring to the crash, Miles (2017) writes:

If existing economic theory told us that such events should be predictable, then maybe there is a crisis. But it is obvious that economics says no such thing. In fact, to the extent that economics says anything about the timing of such events it is that they are virtually impossible to predict; impossible to predict, but most definitely not impossible events.

While this a reasonable start to an answer, I think many would feel that it is an incomplete answer. To see why, consider the following analogy. Suppose a patient asks a doctor, ‘Am I at risk of having a heart attack?’. If the doctor could reply only, ‘It is not possible to predict the timing of a heart attack’, and no more, then most patients would consider this an entirely inadequate answer. However, in reality, most doctors would not be slow to reel off a long list of risk factors that raise the risk of a heart attack: high cholesterol, high blood pressure, smoking, drinking, bad diet, stressful job, insufficient exercise, being overweight, etc.

In the same way, the answer that financial crashes are unpredictable is just not good enough on its own. Surely economists can produce a list of the risk factors that increase the risk of a crash? And in principle, they can. Miles (2017) goes on to offer a wide variety of factors which would influence the probability of a crisis. Wren-Lewis (2017) reproduces a very telling graph about bank leverage and concludes that most economists, if they had been shown the graph before the financial crisis, would have been very concerned about the risks.

Very good. Now, given this knowledge of risk factors, presumably it should have been possible for enough people to observe that we were entering dangerous territory before the financial crisis of 2007–08. Surely it would have been possible to provide some sort of ‘yellow warning.’² And yet it seems most people did not see the crisis coming. Why not? Referring to the graph mentioned in the last paragraph, Wren-Lewis (2017) answered as follows: ‘The problem before the financial crisis was that hardly anyone looked at this data. There is one institution that surely would have looked at this like this [*sic*] data, and that was the Bank of England.’

In short, the mainstream response is that there may have been a system failure, but the failure was in the Bank of England and not in the academic sector of economics, and therefore it is wrong to talk of a crisis in academic economics. Indeed, if you look at the workings of the Royal Economic Society in recent years, you do not get any sense that it is a society in crisis. In 2012, the Secretary General wrote in his *Annual Report* (Beath, 2012):

The bee is the symbol of the Society and I am happy to be able to report that our hive is particularly healthy this year for two reasons. The first is that our established colonies are maintaining their vigour; the second that new colonies are establishing.

I believe that this harmonious account of life inside the hive is absolutely correct. Inside the hive, all the bees were entirely happy with themselves. Perhaps there should have been concern at the fallout from the crash, but there was not. As Wren-Lewis (2017) says, many mainstream economists did not feel challenged by the financial crash:

Economics is much more than macroeconomics and finance. Look at an economics department, and you will typically find less than 20% are macroeconomists, and in some departments there can be just a single macroeconomist. Those working on labour economics, experimental economics, behavioural economics, public economics, microeconomic theory and applied microeconomics, econometric theory, industrial economics and so on would not have felt their sub-discipline was remotely challenged by the financial crisis.

We shall see below that some of those groups who use or relate to mainstream economics find this attitude much too complacent, but those inside the hive did not. And while macroeconomists and financial economists had to come up with some sort of answer, they felt they could exonerate themselves by reference to the efficient markets hypothesis. They could not have predicted the timing of the crash, and that is the end of the debate. And indeed, life carries on as normal within the hive. Mainstream economists do not perceive themselves to be in a crisis and, at one level, they are right. For a mainstream economist in the UK, a crisis is not having a good enough collection of research papers for the Research Excellence Framework (REF). That would certainly be a severe threat to an academic career; but failure to predict a major economic shock would not.

The rules of the REF have not been changed by the financial crash. The overwhelming priority is still to publish in leading journals, and little else matters. The best way of achieving this

objective is completely unchanged by the crash. Now, it may be that a heterodox approach to economics (for example, that of Minsky, 1986/2008) is a better route to assessing the risk of a financial crash. But producing work in the style of Minsky is not an easy route to publication in the top journals. In short, the reward model by which mainstream economics is managed has not changed in any way since the crash, and there is no incentive to pay any attention to the crash.

When Haldane talks about a crisis in economics, however, he is talking about something different. Haldane is concerned about ‘the distance that the economics profession needs to travel if it is to win heads, to say nothing of hearts’ (p. xiii). If mainstream economics is so far from winning hearts and minds, this implies a serious problem in the relationship between mainstream economics and those who use or relate to it. Practitioners (such as Haldane) are probably better placed than most academics to assess how mainstream economics is regarded in the wider community, because few mainstream economists choose to spend much time talking to this community. Moreover, Haldane is by no means alone in thinking this. For example, even the *Guardian*, a newspaper normally very supportive of academics, makes an exception for economics: ‘the standing of the (economics) profession with the general public is probably at an all-time low’ (Inman, 2016).

So, is there a crisis in the relationship between mainstream economics and those who use it or relate to it? Has economics really reached a point where, without remedial action, the relationship between mainstream economics and those who use it will be seriously damaged or simply come to an end? To help answer this question, I shall first describe some particular problems in relationships between mainstream economics and six specific groups of users or ‘relatives’. Then, in the conclusion, I shall give my answer.

Mainstream economists reading this review essay – though I suspect that relatively few *Prometheus* readers are mainstream economists – may feel that my critique is rather one-sided. I say too much about what is wrong with mainstream economics and not enough about what is wrong with other sorts. This is probably fair comment, but is inevitable for reasons of space. So, let me stress this: I am certainly not implying that the others in these relationships are always easy to deal with, or paragons of reason.

Mainstream economics and its students

I shall start where the authors’ journey started, as students of economics. Chapter 2 of their book describes their experiences. It has a pretty stark title: ‘Economics as indoctrination’. Strong words? Well yes, but not as blunt as some others. Colander (2007) asked many students at several US universities about their economics education. One remark from a student stands out: ‘I remember our first year here. One of our professors here said, “I’m not here to teach you, I’m here to brainwash you.” And that’s been pretty much successful’ (Colander, 2007, p.157). This comment is very telling because not only is a student saying that he experienced brainwashing, but also a professor is quoted admitting (albeit perhaps tongue in cheek) that this brainwashing was the intention.

The book quotes the views of the authors’ classmates. Maeve grew up in a mining town in the North East of England. She encountered the hard side of economic life as a child and realised that if she was ever to understand the experience, she had to know about economics. That was her rationale for studying economics as an undergraduate, but it did not turn out as expected (pp.35–6):

These aspirations end quickly. Students beginning an economics degree could be forgiven for thinking they had been transported to an alternative reality. The urge to learn about society, expressed by Maeve and many other economics students, must be suppressed as they are confronted with a series of abstract concepts and ideas that seem to have little to do with the actual economy. Students may wonder why it is

necessary to detach the study of economics from reality in this way, but they must also learn to inhabit this parallel universe if they want any hope of passing their exams.

And it gets worse (p.36):

A set of assumptions – typically long and obscure – is drilled into students’ minds, followed by the steps required to erect the logical superstructure built on these assumptions ... Sceptical students will be met with the catch-all that all theories make assumptions ... or are told that if they go on to do a PhD (which most of them won’t) then the assumptions will eventually be dropped.

I would have been tempted to change the end of this sentence to ‘... then the assumptions will eventually be dropped (which they will not.)’ The notion of PhD economics as an ‘age of enlightenment’ where all strong assumptions are replaced with realistic assumptions is unrealistic. Moreover, the phrase ‘drilled into students’ minds’ is pretty alarming. Perhaps, after all, there was no hyperbole in Leontief’s remark comparing economics education with military training.

The list of reasons for student discontent is a long one and includes:

- students are taught only one way of doing economics
- students are unaware of different theories and methods
- critical and independent thinking is discouraged
- there is little or no history or ethics in economics courses
- teaching is excessively abstract and mathematical
- too many exam questions are multiple choice or technical exercises
- far too few exam questions test the ability to evaluate or interpret
- ‘the near total absence of the real world in the classroom’ (p.54)

There are many others. The last observation resonates with what, to my mind, is the most succinct and precisely accurate criticism of the state of economics that I have ever heard: ‘The main thing that is wrong with economics is its disrespect for fact’ (Wiles, 1984, p.293). The students report the results of their impressive surveys that show these problems are not confined to Manchester University, but are to be found in many other UK universities.

Perhaps the last straw for these students was this. Their Post-Crash Economics Society (PCES) hosted a lecture on the 2008 financial crisis by an external speaker, because the topic had simply been ignored by their lecturers. They found the lecture very informative. At its conclusion, one of the authors was in conversation with a student in sociology. The sociology student had already received in-depth lectures on the financial crisis. As the book’s authors say (p.56): ‘When sociology students know more about financial crises than economics students, something is wrong – and not with sociology.’ Indeed, there is more to the Manchester story, which is taken up by the *Guardian* (Chakraborty, 2014):

Since last autumn, members of the university’s Post-Crash Economics Society have been campaigning for reform of their narrow syllabus. They’ve put on their own lectures from non-mainstream, heterodox economists, even organising evening classes on bubbles, panics and crashes. You might think academics would be delighted to see such undergraduate engagement, or that economists would be swift to respond to the market. Not a bit of it. Manchester’s economics faculty recently announced that it wouldn’t renew the contract of the temporary lecturer of the bubbles course, and that students who wanted to learn about the crash would have to go to the business school.

It is not appropriate for a business school economist like me to lecture economics departments on how they should teach economics, any more than it is appropriate for them to lecture me on how to teach economics to business students. But I would like to conclude this section with four observations.

First, the remark about business schools in this last quotation is an interesting one.³ Economics students may, with some justification, feel there is something wrong when there is little or no coverage of the financial crash in their curriculum in the school of economics. But is it necessarily a bad thing if students ‘have to go to the business school’? After all, business schools have

to be focused on recent developments in the real world. Moreover, many companies outsource some of their operations, and there are often very good reasons for this. I think the answer to this question depends critically on the spirit in which the outsourcing is done. As I see it, an organisation may choose to outsource an operation (X, say) for at least three different reasons:

- (a) We don't really want to do X, so let's outsource it to someone else who will do the job at a competitive price.
- (b) We don't have the necessary resources to do X in-house for now, so let's outsource it as a temporary fix, and aim to do it in-house later on.
- (c) We could do X in-house, but a specialist could do it better, so let's outsource it.

If a school of economics chooses to outsource teaching about the financial crash (or any other major real-world event) for reason (a), then this simply reinforces the view that economics, as a discipline, does not care about the real world. Reason (b) would be better, and reason (c) is better still. The best of all is where outsourcing for reason (c) is combined with a proper long-term partnership between schools of economics and business schools. However, with the honourable exception of my mainstream economics colleagues in Nottingham University, I have met few mainstream economists who believe anything much is better done in a business school!

Secondly, while I would not want to suggest that business schools are paragons of virtue when it comes to teaching, we do have an inbuilt mechanism that ensures we usually keep a very close eye on the real world. This mechanism is called the MBA (Master of Business Administration) student. These students are typically in their late twenties or early thirties, they have worked 5 or 10 years since graduation, and they know a great deal about the real world. They are bright and articulate, they are not slow to raise objections, and they expect quick responses. They feel empowered to behave like this because their fees are high and, as most of them have given up well-paid jobs to study on the full-time MBA programme, the opportunity cost of their time is *very* high.

My colleagues in the business school field of strategy often emphasise the value of having demanding customers as a way of ensuring a business remains competitive. And this is exactly the role played by MBA students. If MBA students were given the sort of abstract technical exercises described by the authors, they would object. If their curriculum offered no explanations of the financial crash, they would riot!¹⁴ However, this definitely does not mean that they have no interest in economic theories, nor does it mean they want to learn only facts. On the contrary, what they want, above anything else, is to learn how to use economic theories to interpret and understand economic facts. And, from what I read in this book, that is exactly what the authors and their classmates wanted.

Thirdly, after reading the book, I realise how incredibly lucky I was to be an undergraduate student in mathematics and economics in the early 1970s. My teachers included some from the 'old school', who taught us a great deal about what was happening in the real world: inflation and unemployment, the oil-price shock, inequality, the unexpected effects of the 'Green Revolution',⁵ the implications of UK entry to the European Common Market, and so on. And in addition to learning about mathematics from real mathematicians, I learnt from some of the young mathematical economists and econometricians how to use these methods to understand real-world questions. I would guess that if the book's authors had enjoyed a similar approach to the teaching of economics, they would never have written their book.

Finally, most academics, in my experience, are somewhat uncomfortable with the idea of the student as *customer*. The reason is simple: as the famous marketing slogan puts it, 'the customer is always right'. If students are customers, then students are always right, and therefore we can give them 100% for every assignment and exam. And if we know in advance that they will achieve 100% for everything, we can give them their first class degrees (with distinction) before the very

first lecture of the first year. Indeed, if they are always right, they hardly require education in the first place. Nevertheless, while the idea of student as customer may be problematic, the idea of an empowered student is certainly not. On the contrary, students have a right to get what they want. If they do not get it, they will go elsewhere.

Mainstream economists and heterodox economists

The authors make a very compelling case for pluralism in the economics curriculum (Chapter 3) and believe that pluralism is a necessary part of a liberal education (Chapter 5). By pluralism, we mean that students can study both mainstream and non-mainstream, or *heterodox* approaches. One of their main criticisms of their curriculum is that there is no pluralism. However, despite student wishes, I fear that mainstream economics departments will not move very far in this direction, and any gestures will be tokens rather than real reform. This is because the relationship between mainstream economics and heterodox economists is not in good repair. The authors reflect on this in Chapter 4.

One of the most influential pioneers of heterodox economics was Kenneth Boulding.⁶ I mean no insult at all when I say that Boulding perfected the art of heterodox economist as *court jester*. On the contrary, it is a great compliment: in mediaeval England, the court jester was one of the most powerful and trusted people in the King's entourage, and could say things that others were too fearful to say. Boulding's genius was his ability to make delightfully humorous remarks about some of the absurdities of mainstream economics, while managing to remain on friendly terms with mainstream economists, because they could laugh with him. One of the most memorable of his observations was that, 'anyone who believes that exponential growth can go on forever in a finite world is either a madman or an economist' (*Economist*, 2015).

Today, sadly, relationships between mainstream economists and heterodox economists are not always as friendly. Some mainstream economists consider their heterodox cousins to be ignorant of mainstream ideas, lightweight, disrespectful, and, ultimately, irrelevant. A perfect illustration of this attitude is given on p.106 of the book, where the authors quote a memorable remark made by one of their teachers: he compared heterodox perspectives on economics to, 'the outdated use of tobacco smoke enemas ... in medicine.' And for their part, some heterodox economists view their mainstream relatives as dismissive, complacent, and ignorant of facts. A particular concern of some heterodox economists is that mainstream economics appears to have a dysfunctional honour code that exhibits excess reverence and excess contempt, and this is damaging to economics as a science. This is a reference to a very important philosophical principle elaborated by Russell (1946, p.58):

In studying a philosopher, the right attitude is neither reverence nor contempt, but first a kind of hypothetical sympathy, until it is possible to know what it feels like to believe in his theories, and only then a revival of the critical attitude which should resemble, as far as possible, the state of mind of a person abandoning opinions which he has hitherto held. Contempt interferes with the first part of this process, and reverence with the second. Two things are to be remembered: a man whose opinions and theories are worth studying may be presumed to have had some intelligence, but that no man is likely to have arrived at complete and final truth on any subject whatever.

As to excess reverence, Romer (2016) makes an interesting observation:

Several economists I know seem to have assimilated a norm that the post-real macroeconomists actively promote – that it is an extremely serious violation of some honor code for anyone to criticize openly a revered authority figure – and that neither facts that are false, nor predictions that are wrong, nor models that make no sense matter enough to worry about.

When science is working properly, it should be much more important to expose false 'facts', wrong predictions, and models that make no sense, than to protect the egos of revered figures from any criticism. In the business school community, some revered figures have urged

researchers to be on their guard against *excess reverence*. For example, William Starbuck argues that it is healthy if researchers seek to undermine the social status of leading journals; he believes that status hierarchies are largely unjustified by quality differences.⁷ In contrast, it seems inconceivable that mainstream economists would ever dare to undermine the status of *Econometrica*, the *American Economic Review*, the *Journal of Political Economy*, or the *Quarterly Journal of Economics*.

As to excess contempt, an extreme example is the practice of ‘shouting down’ seminar speakers, which I first encountered as a PhD student, and which I observed from time to time between 1978 and 2003. By this, I mean the practice of members of the audience repeatedly and angrily interrupting the speaker. Where the chair of the seminar makes little or no attempt to restore order, the seminar is ruined as an academic event. Such incidents were usually confined to seminars when a heterodox economist gave a presentation to a mainstream audience. Some university honour codes explicitly forbid any disruptive behaviour (such as shouting down) because it violates a fundamental right of fair access to the academic experience (see University of Mississippi, 2011, p.7). And yet, the de facto honour code in mainstream economics appears to be to turn a blind eye to shouting down.

Why has the relationship turned nasty? The underlying reason is that both sides, in their different ways, think that the other side has become a threat. Some heterodox economists consider that the mainstream economists are no longer content with driving the heterodox out of economics departments, but now aim to drive the heterodox out of the academy altogether. Some mainstream economists, on the other hand, feel that heterodox economists are feeding the media with negative stories in order to sustain a popular onslaught against the mainstream. To illustrate this, I shall discuss three detailed examples of difficult exchanges between mainstream and heterodox.

Heterodox work and top journals

The first refers to a long-running sore in the relationship between mainstream and heterodox. Heterodox economists are judged harshly for their lack of publications in top journals. Many heterodox economists think this criticism unfair, because they find a pretty hostile attitude towards their work in the leading economics journals. Indeed, some would go as far as to say that these leading journals are more or less a closed shop to anything heterodox. The secretary-general of the Royal Economic Society is unsympathetic (Portes, 2008):

Mediocrity is rationalised on the grounds that it is hard for the ‘heterodox’ to publish in top journals – despite the examples of Joseph Stiglitz, Amartya Sen, Herbert Simon, Samuel Bowles, Herbert Gintis, and many others.

The problem with the Portes list is that the first three are Nobel laureates, and the other two are world-class scholars. Most journals will open their doors to people of that stature, but that implies little or nothing about the attitude of these journals towards heterodox work by good scholars from the next tier down in the academy. And if Portes considers these people to be ‘heterodox’, whatever word would he use to describe *really unorthodox* economists? Moreover, the classification of an economist as orthodox or heterodox depends on their stage in the life-cycle. For example, while much of Stiglitz’s recent work (and some would say, his best) is heterodox, much of the earlier work that made him famous and won him his Nobel prize was – in the estimation of heterodox economists, at any rate – almost entirely *orthodox*.

Moreover, Portes is actually making a very strong assumption if he thinks the peer review system used in top journals is working well when it rejects papers from academics working outside the mainstream (see Letto-Gilles, 2008). Editors and editorial board members of top journals are chosen from the upper ranks of the mainstream. Moreover, it is often ‘their graduate students who referee many of the papers (you are expecting Elvis, but you get the Elvis impersonator)’

(Deaton, 2013, p.4). How can graduate students, who have been ‘indoctrinated’ or ‘brainwashed’ to have mainstream views, possibly assess the true quality of a heterodox paper? They can assess whether it shows any deviation from orthodoxy, but they cannot judge quality in the way that a heterodox economist would judge it. As Deaton (2013) puts it, ‘a Harvard graduate student is playing dice with your future.’

The essential point here is a very simple one from the basic economic theory of preferences and choice. Heterodox economists have different priorities from mainstream economists, and, because of these different priorities, heterodox economists choose to do their research in a different way from mainstream economists. These choices are not mischievous or contrarian; they are made because heterodox economists sincerely believe that their priorities are correct. But to a mainstream economist, with a different set of priorities, the choices will look peculiar. When I had worked as an editor for some years and had learned the different priorities of mainstream and heterodox researchers (and therefore understood why their work looked so different), I felt I could make a fair assessment of papers in either tradition. But I could not have done that as a doctoral student at a very orthodox graduate school.

Refuges for the heterodox?

The second example follows on from the last, and has been seen by some as an audacious attempt by the mainstream to silence heterodox economists, though this may be an over-reaction. Shortly after being awarded the Nobel prize in 2014, Tirole wrote a letter to the French minister for higher education and research. He raised his concerns about a rumour that the French national council of universities would be creating a new section named ‘Institutions, Economy, Territory and Society’ in addition to the existing section for economics. The implication is that heterodox economists who did not fit into mainstream economics departments in France would find a refuge in this new section. Tirole says (Association Française d’Economie Politique, 2015):

Breaking up the community of French economists by creating a refuge for a disparate group, in trouble with the assessment standards that are internationally acknowledged, is a very bad answer to the failure of this group in its effort to have its works validated by the great scientific journals, that prevail in our discipline.

Tirole suggests a way of resolving this matter which, as the (heterodox) *Association Française d’Economie Politique* (AFEP) observes, was akin to asking, ‘a representative sample of a Papal conclave to decide about the legitimacy of a demand by a minority of Protestants.’⁸

In my view, Tirole’s argument is flawed because he is asserting that there is, and should only be, a single assessment standard. I believe it shows an excessively simplistic view of the role of standards. I have written a lot about standards, and spent much time advising governments and standards institutions in several countries about the right way to understand the economic role of standards. The basic economic theory of compatibility standards often defaults to the proposition that the way to maximise the benefits enjoyed by network effects is to have a single standard, and that any plurality of standards is sub-optimal. But this view is far too simplistic.

If you want to see the weakness in such an argument, try it out on the users of different personal computer systems. There are three main operating system standards in widespread use: Microsoft Windows, Apple, and Linux. These three segment the market in a pretty obvious way. Many of those using PCs for typical office applications use Windows. Those using PCs for design use mainly Apple. And those who need open-source software use Linux. I can safely say that if somebody issued a mandate that there must only be a single standard, there would be a riot! Apple and Linux users would never be happy to switch to Windows, while most Windows users could never cope in a Linux environment. Having three standards is undoubtedly a better solution than having one, and the loss of network effects is pretty modest.

Or, to take another example, try out the single standard argument on the accountancy profession. Different accounting standards have emerged in various countries, and these differences reflect the varieties of capitalism found in these countries. Over time, a process of harmonisation has reduced the number of standards. Some believe that this process should continue until there is a single international standard. Meek and Swann (2009), however, argue that the optimum number of standards should be small, but that a single standard is unlikely to be optimal. In brief, the argument is this. When a company in country X is assessed by the traditional accounting standard of country X, it may appear perfectly viable, and investors in country X will be confident to invest in it. When a company in country X is assessed by the accounting standard of country Y, it may appear loss-making. The implication is that enforcing a single standard will make some perfectly viable companies appear loss-making, and the ultimate effect of this will be to increase industrial concentration and reduce competition.

I would make one other point about Tirole's objection to the creation of refuges for heterodox researchers. There are some very interesting lessons to be learned here from the development of medicine as an academic discipline. The medical student does not study just medicine. First, the student studies a wide variety of some twenty or more basic sciences that underpin medicine (anatomy, physiology, biochemistry, biomechanics, cytology, epidemiology, genetics, etc.). Second, there are speciality subjects which come from a variety of surgical, clinical, diagnostic, and other disciplines. Third, the student may study a variety of interdisciplinary subjects (addiction medicine, forensic medicine, laser medicine, pharmacogenomics, etc.).

It seems clear that some – and probably many – of these various specialities would not have developed as sciences without a certain amount of autonomy. The history of biochemistry, for example, demonstrates this point very well. In its early days, biochemical research was marginalised by both the Chemical Society and the Physiological Society (Plimmer, 1949). The chemists did not recognise biochemists as proper chemists, and although the international conferences on physiology would accept some papers on biochemistry, these were scheduled to be presented together in a separate room, apart from the main conference. But it was clear that biochemistry was becoming a very important basic science in medicine, and also of interest to botany, agricultural research, brewing, and pathology *inter alia*. The founders of the Biochemical Society recognised that it was necessary to advance the status of their new science and that this would require a new and autonomous society devoted to biochemistry alone. It would not flourish as an annex to another discipline.

Perhaps this, above all, is why I find Tirole's statement rather alarming. If, at the time of the foundation of the Biochemical Society, someone had said that it would be a bad idea to break up the communities of physiologists and chemists by creating a refuge for a disparate group of biochemists, this would have been very unfortunate indeed. If economics is to advance as a science, we need to develop a much wider variety of empirical methods. There is no chance of these prospering in a mainstream environment which recognises only econometrics and experimental economics.

The reputational damage of heterodoxy?

The third example is the argument that the existence of heterodoxy is bad for the reputation of a discipline. As Kay (2015) puts it, so concisely, '... no one would cross a bridge built by a heterodox engineer'. Indeed, engineers are very wary of heterodoxy. When I asked an engineering colleague whether there were any heterodox engineers in the university, he replied, 'I would certainly hope not!' There are at least three reasons why engineers hope there is no heterodoxy:

- the existence of heterodoxy is bad for the reputation of engineering
- models are pretty good so not much contradictory evidence is found
- engineers are quick to respond to contradictory evidence, and improve their models.

So how does heterodoxy emerge as a phenomenon? As I see it, there are four steps. First, some researchers experience cognitive dissonance when they find that evidence contradicts orthodox theory. Second, these researchers decide to embrace the contrary evidence, to question the orthodox theory, and to suggest a heterodox alternative. Third, the mainstream response is that the contradictory evidence is just a nuisance, it should be swept under the carpet, and the heterodox alternative should be ignored. Fourth, the researchers who found the contrary evidence refuse to be silenced, and progress along the road to heterodoxy. Now, of course, some dissidents who feel they have contradictory evidence may be wrong. They may have misunderstood the orthodox theory, they may have misread the evidence, or perhaps both. In this case, the discipline is of course correct to ignore the dissident. But if a disparate heterodoxy persists, it is surely a sign that much contradictory evidence is being found and is being swept aside by the discipline.

Most scientists would hope that when their discipline is mature, there will not be an untidy state of pluralism. They would hope that, with the passage of time and the processing of contradictory evidence, a core of robust theories would emerge on which there is general agreement. But what about the short term, especially in a science that is nowhere near mature? Is it better to have an artificial consensus, an artificial show of unity? Or is it better to be open about the fact that a plurality of theories exists, and we don't know which works best? While the artificial consensus is sometimes essential in politics, it is a dangerous thing in science. If our understanding is incomplete and/or our measurements are inaccurate, it is best to be honest about this – even if this gives the (correct) impression of a discipline that is not yet mature.

In conclusion, I add one thing that is perhaps most important of all. Pluralism in economics is not just about lots of contradictory theories. More important, to my mind, is that pluralism is about finding and using many more facts, generated by as wide a variety of empirical methods as possible. Armed with a wide variety of facts, we can start the process of throwing out bad theories. I cannot see how that sort of pluralism could damage the reputation of economics. On the contrary, it is something that would enhance the reputation of our discipline.

Mainstream economics and ordinary citizens

Next, I turn to the relationship between mainstream economics and ordinary citizens. If the last section concerned an uneasy relationship between two groups of experts, and the one before concerned an uneasy relationship between the mainstream and empowered students, this section concerns the relationship between the mainstream and the unempowered and bemused citizen.

In Chapter 1, the authors introduce an important concept, an 'econocracy', which they define as follows (p.7): 'a society in which political goals are defined in terms of their effect on the economy, which is believed to be a distinct system with its own logic that requires experts to manage it.' They argue that this is not just a theoretical possibility, but also an apt description of the world in which we live. It is helpful in what follows to call this definition a 'type 1 econocracy'. In his foreword, Haldane says that he is not sure whether we live in this type of econocracy, and that is my view also. I do not dispute the examples cited by the authors in Chapter 1, which relate mostly to macroeconomics and finance, but I am not convinced that this is the norm throughout the economy. However, I am happy to agree that we do live in something that could be called a 'type 2 econocracy'. This is an idea that can be traced back to Carlyle and Marx, but my favourite definition is that from William Morris (1879/1966, p.82): 'Competitive commerce ... once the servant, and now the master of civilisation.'

What is the difference? In the type 1 econocracy, the economist is the master, while in the type 2 econocracy, the economy is the master. In short, the type 2 is a broader definition. In a type 2 econocracy, sometimes it may be the economist who is the master, but more often, I

would say, it is the captain of industry or the banker who is master. And sometimes, indeed, there is no one master; policy emerges from a dialogue among different parties. Indeed, in the area of industrial policy, which I know best, I would say that policy-setting was a process of achieving balance among government, businesses, the supportive institutions (standards, metrology, design, etc.), and economists.

Regardless of which type of econocracy is most relevant, both types have an important feature in common. In both cases, the relationship between the economy (or the economist) and the ordinary citizen is far from being a relationship of equals. The economy (or the economist) is dominant, and the ordinary citizen is mostly subordinate. Indeed, the authors describe how many ordinary citizens are quite bemused by the discussion of economics. It is conducted in a ‘foreign’ language that ordinary citizens do not understand, and some ordinary citizens wonder whether this is done deliberately to exclude them from the debate. Economists make statements about something being good for the economy and something else being bad for the economy, but often ordinary citizens see no correlation between their own experiences and these statements about macroeconomic effects. And some ordinary citizens find that their own experiences sometimes contradict the generalisations made by economists. We do not seem to care.

I said at the start of the essay that an econocracy is a bit like a bureaucracy, but with econocrats replacing bureaucrats. It is not misleading to say that public perception of econocrats is similar to public perception of bureaucrats. Neither is loved, nor are they necessarily trusted, even if it is accepted that we cannot do without them. Andy Haldane’s remarks remind us how far we economists have to travel to win hearts and minds. It is no exaggeration to speak of loss of public trust in economics and economists. Right-wing politicians, in particular, have found it easy to treat economics with ridicule when it suits them – for example, in the debate around the Brexit referendum. Moreover, economists have been the butt of popular jokes for a long time. Indeed, when a joke is told about a physicist, a chemist, and an economist on a desert island, the general principle appears to be that the listener should laugh at whatever the economist says or does. In Chapter 6, the authors argue that the relationship between mainstream economics and ordinary citizens could be much improved if it were more democratic, and if we had a higher proportion of citizen economists confident enough to debate with professional economists as political equals. I agree. A similar approach would help to improve many of the relationships between mainstream economics and its users and ‘relatives’.

Finally, in a healthy relationship between mainstream economics and ordinary citizens, the challenge is not just to explain mainstream economics to citizens in plain language. It is also essential to develop a flow of ideas in the reverse direction, from citizen to professional economist. This is a theme close to my heart. In some of my earlier work, I stressed the importance of vernacular (i.e. non-professional) knowledge about the economy, and encouraged empirical economists to give proper attention to such knowledge (Swann, 2006). I have also emphasised the role of common innovation (or vernacular innovation) in wealth creation, and urged economists to give this the level of attention they give to business innovation (Swann, 2014).

Relationships with other academic disciplines

I now turn to the first of three relationships that are not really discussed in much detail in the book. I am in no doubt that the success of economics depends on these relationships being in good health. The first is our relationship with other disciplines. One of the major problems other disciplines have relating to economics is caused by economists not following the principles of economics. We talk a lot about the benefits that derive from trade, the division of labour, and competition, and outsiders might expect economists to apply these concepts in their research. We do this far less than they expect.

For a discipline that puts such emphasis on trade, mainstream economics is remarkably bad at trading ideas with other disciplines. In particular, there is remarkably little trade between mainstream economics and the other social sciences – psychology, sociology, anthropology, law, and so on. What trade there is involves economists who make a virtue of inter-disciplinary work, and, for the most part, these people are not considered to belong to the mainstream.

For a discipline that puts such emphasis on the division of labour, mainstream economics makes less use of it than others would expect. Economics has several basic concepts which we all study: companies, production, selling, purchasing, consumption. The principle of the division of labour is that by dividing a task into many small parts, and then recombining the efforts of different workers, we can achieve much higher levels of productivity. And that is exactly what happens in business schools: specific fields study specific parts. Thus, organisation behaviour studies what companies actually do; operations management studies production and distribution; marketing studies how companies sell; consumer behaviour studies purchasing and consumption behaviour; and so on. But when mainstream economists are confronted with the work that emerges from these specialised fields, they tend to turn up their noses and say, ‘No thanks, we’ll do all this ourselves.’

And for a discipline that puts such emphasis on competition, it is surprising to see that mainstream economists often choose to behave like monopolists.⁹ Basic economics tells us that in competitive markets, we have choice, variety, and good value, while in monopolistic markets we often have limited choice, limited variety, and poor value. Linus Pauling, one of only four individuals to have won two Nobel prizes, had a famous maxim: ‘If you want to have good ideas you must have many ideas. Most of them will be wrong, and what you have to learn is which ones to throw away’ (see Crick, 1995). A scientist who follows such a maxim should, in principle, be interested in a wide variety of potentially promising ideas – wherever they come from. But mainstream economists are generally curiously resistant to ideas from outside the mainstream. Consider again what *The Econocracy* has to say about tobacco smoke enemas. It says something indeed about the state of mainstream economics when any alternatives are treated in so dismissive a fashion.

I have a few more observations about the relationship between mainstream economics and other disciplines, but to make sense of them I need to put these other disciplines into two broad groups: ‘superior’ disciplines, which occupy a higher rank in the academic pecking order; and ‘inferior’ disciplines, which probably occupy a lower rank. Mainstream economists would generally accept that mathematics, the hard sciences (physics and chemistry), and probably engineering occupy a higher rank in the academy than economics. Naturally, economists would like to have the approval of these superior disciplines, but they do not always get it. I shall give three examples.

The first is the much-cited conference of economists and physicists at the Santa Fe Institute, described in Waldrop (1994). This involved several economists giving presentations on recent developments in economics to the physicists, and seeing what the physicists made of them. An early expectation amongst some economists was that the physicists might think that the economists’ mathematics was rather unsophisticated, and that would be their downfall. But the actual outcome was totally different. In practice the physicists were ‘awestruck and appalled’ (Waldrop, 1994, p.140) at the mathematical prowess of the economists. One physicist observed: ‘They were almost too good ... It seemed as though they were dazzling themselves with fancy mathematics, until they couldn’t really see the forest for the trees.’ Equally, some of the economists were shocked to discover that physicists were comparatively casual about their mathematics.

The physicists were also disconcerted by how seldom economists actually talked about data and reality. And they were astonished that economists had apparently made no effort to consult

sociologists, psychologists, anthropologists, and others about some of the phenomena they were studying. And, perhaps above all, ‘The physicists were shocked at the assumptions the economists were making – that the test was not a match against reality, but whether the assumptions were the common currency of the field’ (Waldrop, 1994, p.142). Waldrop (1994, p.143) goes on to describe how these exchanges created a bad atmosphere in the meeting: ‘Most of the economists sat on one side of the table, and most of the physical scientists sat on the other.’

The second example is of how engineers view the economists’ response to the financial crash. As we have said before, engineers believe they are pretty good at learning from disasters and learning how to make their machines work properly. As one of my engineering colleagues in Nottingham put it, while disasters are tragic, they do offer a tremendous opportunity to learn. Learning from disasters is an essential part of the curriculum, and an essential skill for all engineers. In contrast, many engineers think economists are pretty bad at learning from disasters. Some have told me they are really shocked at the way many mainstream economists just washed their hands of the financial crash. If an engineer were to read that many economists ‘would not have felt their sub-discipline was remotely challenged by the financial crisis’ (Wren-Lewis, 2017), there would be accusations of complacency. The crash should have been a wake-up call for all economists. All economists should have taken a long hard look at it to see what they could learn, and what they might be doing wrong. Moreover, the engineer would not be impressed by the argument that it was the Bank of England, not academics, who had the responsibility of looking at the data. Granted, vigilance over data of this sort is not in the job description of academics, but what does it say about mainstream academics if scrutiny of such data is not a priority for them. After all, academic environmental scientists are vigilant in following data on the state of the environment. Why is it not a priority for economists?

The third example is of an exchange between a mathematician and econometricians, which I recall from my time as a PhD student. Rudolf Kalman, an outstanding mathematician, recipient of numerous awards, and best known for his invention of the Kalman Filter, came to the London School of Economics (LSE) to give a seminar about his work on inference from noisy data (Kalman, 1982). This is essentially the problem studied by econometricians, but Kalman’s mathematical approach was much more sophisticated. The session was of particular interest to me because I had already come to the view that ‘noisy data’ (specifically, measurement error) was a much more serious problem in econometrics than the LSE econometricians seemed to realise. Kalman’s central point was that noisy data cannot produce an exact model. This view is in conflict with the view of most econometricians, who believe that even with noisy data we can generate a model that is (almost) exact, as long as we have a very large sample of data. But in Kalman’s approach, the volume of data is not the issue. It is the presence of noise that makes exact identification impossible. Kalman went on to show that all econometric approaches to producing an (almost) exact model from noisy data were based on one or another kind of ‘prejudice’. I remember that some of the econometricians disliked the use of such a politically laden word, and I have some sympathy with this view. A more diplomatic replacement for a prejudice would be an assumption that cannot be checked against data.

I recall the typical response of the econometricians after that meeting. Some complained about Kalman being so negative about econometrics. Others just did not want to know, and said they would carry on as if they had never heard Kalman’s talk. Others told me that Kalman was just nit-picking and, empirically speaking, his arguments were not important. Nobody, as far as I can recall, said that these observations by a far better mathematician needed serious attention, and they would be thinking hard about their approach to econometrics. If any readers of *Prometheus* are thinking that this is all rather abstruse and of limited practical relevance, they might glance at Swann (2012), which gives a simple explanation. Kalman’s critique is fundamental, and no econometrician should ignore it.

My main point is this. Mainstream economists aspire to win the respect of the scientific aristocracy. But when the scientific aristocracy tells them that they are on the wrong course, too many mainstream economists just brush aside this advice, and carry on as usual. I am not saying that mathematicians, physicists, and engineers are always wise about economics. They are not. Nevertheless, economics needs the respect and co-operation of these superior disciplines. At present, economics does not have this respect.

Finally, what about the attitude to ‘inferior’ disciplines? And indeed, what are these inferior disciplines? Many mainstream economists tend to treat most of the social sciences outside economics as inferior – with the possible exception of anthropology, which is just very different. And almost every mainstream economist that I have met sees all the fields of business studies as inferior. The main problem is that most mainstream economists do not acknowledge a relationship with most inferior disciplines. They see no need for a relationship. And for their part, these other disciplines are rather tired of being treated as second-class citizens, and either indulge in negative stereotyping of economists, or just ignore us.

Many heterodox economists, on the other hand, would say that it is essential to recognise relationships with many disciplines if we truly want to understand our subjects. First, for example, I do not see how any economist can understand all the different facets of consumption behaviour, without having at least some relationship with sociologists and psychologists. And secondly, no sensible economist studying innovation, who really wants to understand that subject, would disregard the work on innovation done in other social science disciplines (sociology, psychology), or in business studies (entrepreneurship, marketing, operations management), or indeed in engineering and technology.

Economists in government, central banks, and regulation

The relationship between mainstream economists and the economists and policymakers working in government, central banks, and regulation is probably the nearest thing the mainstream has to a ‘special relationship’.¹⁰ Not all mainstream economists have a relationship of this sort, but plenty do – especially macroeconomists, but also many applied microeconomists. I think it is fair to say that the relationship is, for the most part, in quite good health. But there are some notable exceptions. For example, Howard Davies (2012), a former deputy governor of the Bank of England, wrote the following telling observation. Readers will note that Davies is just as angry with finance professors in business schools, as with economists.

In an exasperated outburst, just before he left the presidency of the European Central Bank, Jean-Claude Trichet complained that, ‘as a policymaker during the crisis, I found the available [economic and financial] models of limited help. In fact, I would go further: in the face of the crisis, we felt abandoned by conventional tools.’ Trichet went on to appeal for inspiration from other disciplines – physics, engineering, psychology, and biology – to help explain the phenomena he had experienced. It was a remarkable cry for help, and a serious indictment of the economics profession, not to mention all those extravagantly rewarded finance professors in business schools from Harvard to Hyderabad.

I would just like to make two observations about the relationship between mainstream economics and those in government, central banks, and regulation. The first is a personal experience. My work with this community has mostly been with the business department of the British government – originally called the department of trade and industry (DTI), but now called the department for business, innovation, and skills (BIS). In my experience, most of the policymakers there, and some of the economists too, were more comfortable with heterodox economists and industrial economists from business schools than with mainstream economists. Why is this? I think it was because, as business school economists, we were more willing to speak the language of policymakers and business, and to accept ideas from the business community, even if these were very different to those in mainstream economics. Secondly, this community is our strongest

ally, and a practitioner economist, such as Andy Haldane, is an especially important ally within the community. If his view is that mainstream economics is facing a crisis in its relationships with others, then mainstream economists really should listen.

The relationship with business and consultancy

The final relationship I wish to consider is that between mainstream economics and business. While the relationship of economists with government, central banks, and regulation is generally a good one, the relationship with the business and consultancy community is nothing like as good. A common accusation from this community is that mainstream economics suffers from 'groupthink', a term coined by Janis to describe how the desire for harmony and conformity in a group can lead to dysfunctional behaviour (see, for example, Bootle, 2016). Janis (1982) identifies several features:

- illusion that the group is infallible and invulnerable
- conviction that the group observes high moral standards
- unanimity is essential, and any member questioning the group is considered disloyal
- group members self-censor and shield the group from dissident ideas
- inconvenient facts that challenge the group's assumptions are explained away
- the group's opponents are subjected to negative stereotyping.

Obviously, it is very difficult for an 'outsider' to form a productive relationship with an organisation trapped in groupthink. Unlike Janis, I am no psychologist, but most, if not all, of these features of groupthink are familiar from my time as a PhD student in a mainstream graduate school. That said, for the business and consultancy community to accuse economics of being trapped in groupthink seems like a case of the pot calling the kettle black.

There are three further comments I can make about this relationship. First, some consultancies (and journalists, for that matter) seem to thrive on negative messages about academic economics. In some cases, I have detected an element of mischief. Exaggerated claims are made about the shortcomings of mainstream economics, but when I (or others) have attempted to correct these claims, nobody seems in the slightest bit interested. One inevitably speculates whether such exaggeration is part of marketing strategy: 'Don't waste your time with academic economists; come to us instead, because we understand the real world.'

Secondly, I cannot deny that amongst my business school colleagues, the economists often have the weakest relationships with business and consultancy. Colleagues in operations management, operations research, marketing, consumer behaviour, information systems, accounting, finance, and so on have a much better and regular relationship with business and consulting. Given the nature of their subjects, and their very high degree of specialisation, that is not altogether surprising. But it is still a reflection on the state of economics.

Thirdly, I recall one important lesson I learnt from a study we did some twenty or more years ago. I was one of a group of three industrial economists asked to estimate the effect of a possible regulatory change on prices and revenues. Another team of econometricians was set the same task, and, in addition, several consultancies and industry associations were asked to give their assessment, based on their knowledge of the industry rather than any formal economic modelling. Our study used only the most basic econometrics, but we also talked to people in the industry. The econometric team produced a far more sophisticated piece of econometrics, but it was entirely desk-based and involved no fieldwork. Our study could never have been published in a leading journal, but the other study probably could have been. When the sponsors held a meeting to 'test drive' the various models they had commissioned, the result was interesting. Using our crude model, the simulations were broadly consistent with what industry figures

expected. But using the ‘proper’ econometric model of the other team, the simulations were considered implausible. In short, the crude model’s predictions were believable; the sophisticated model’s predictions were not. I dare say that many who are involved in the messy business of economic forecasting would not be surprised by this outcome.

Conclusion

In conclusion, I return to the questions I posed near the start of this essay: Is there a crisis in the relationship between mainstream economics and those who use it or relate to it? Has economics really reached a point where, without remedial action, the relationship between mainstream economics and other users will be seriously damaged, or will simply come to an end? In general, mainstream economists deny that there is any crisis within their discipline. Heterodox economists may believe there should be a sense of crisis in the mainstream, but there is not. Indeed, the outside observer would detect no sense of crisis within mainstream economics, but instead a sense of business as usual and, as things stand, no incentive to change what is being done.

The essential point, however, is this. Whether or not there is a crisis within mainstream economics, most of the relationships between mainstream economics and those who use it or relate to it are in bad repair. Whether this bad repair has developed into a crisis is perhaps debatable, though my own view is that the word ‘crisis’ is fully justified. Many mainstream economists seem unperturbed by this crisis in their relationships, essentially because they do not think that these relationships matter all that much. Mainstream economists seem perfectly happy as a rather isolated community. But while some academic disciplines might, perhaps, be able to exist in relative isolation (old Norse, perhaps, or Anglo-Saxon), that is not the right future for economics.

We study economics because the economy is an absolutely central part of modern society, and so much of our quality of life and, indeed, our ability to survive depends on the proper operation of our economy. The social contract between economics as a discipline and society as a whole requires that the economics community can deliver solutions to problems. This requires good relationships between economics and those that use or relate to it. These relationships are not good, yet economics does not make improving them a priority. This suggests that mainstream economists care little about external perceptions and, to be frank, many could not care less. Worse still, this attitude suggests that what the profession is doing now is the best it can do. I can imagine the most ardent critics of economics lining up for a chance to say: ‘If this is the best you can do, then we don’t want to do any more business with you.’ When we reach this point, then the social contract between economics and society will be in peril. And, while this may sound like a line from *Alice in Wonderland*, the main reason for the crisis will be that there is no sense of crisis within the economics mainstream.

Notes

1. This comes from the ‘Maxims for revolutionists’, an annex to Shaw (1903).
2. The term used by the Meteorological Office in the UK to describe the first warning of severe weather.
3. I should mention in passing that I taught economics at Manchester Business School (part of the University of Manchester) from 1995 to 2004. In those days, it was rare for students from the School of Economics to take modules in the Business School, or *vice versa*.
4. In the midst of the financial crash, I was teaching part of the microeconomics module to our MBA students, and it was agreed that I should devote a three-hour session to what was going on. As the class was originally scheduled to be on innovation, I decided to focus on some of the financial innovations that played such a dangerous part in the crash (mortgage-backed securities, collateral debt obligations, high-frequency trading, etc.). This is an example of MBA teaching at its most productive, because it turned out that quite a few of our students knew about these innovations and could make a lot of useful contributions to class discussion. What they did not know, before the lecture, was just what damage these innovations had done to the economy.

5. I am referring to the technological revolution in the agriculture of developing countries, especially in the 1960s, where the use of artificial fertilisers, pesticides, and high-yielding seed varieties could enhance productivity. Unfortunately, it also had the unexpected effect of increasing poverty for some subsistence farmers who could not afford the necessary fertilisers and pesticides.
6. Boulding (1970, p.v) said that he started his 'transition from being a fairly pure economist' in 1948.
7. William Starbuck's own work is to be found in all the top management journals. He was once editor of *Administrative Science Quarterly*, one of the most highly regarded of management journals. Starbuck made these comments in a lecture at Nottingham University Business School in 2009. The slides of a similar presentation in Linköping are available in Starbuck (2009).
8. These remarks are made in an editor's introduction to Tirole's letter in *Association Française d'Economie Politique* (2015).
9. In particular, as Letto-Gillies (2008) points out, mainstream economics does not believe that there is any merit in competition between different research paradigms (e.g. mainstream and heterodox).
10. In using the term, 'special relationship', I am referring to the phrase used by many British politicians to describe the UK's relationship with the US. Most ordinary people in Britain, however, consider that this special relationship is a myth.

References

- Association Française d'Economie Politique* (2015) 'The translation of a letter of Jean Tirole to Madam Fioraso, state secretary in charge of higher education and research', available from http://assoekonomiepolitique.org/wp-content/uploads/TIROLE_Letter.pdf.
- Beath, J. (2012) 'Annual report of the secretary general 2012', *Royal Economic Society*, available from <http://www.res.org.uk/view/governanceStructure.html>.
- Bootle, R. (2016) 'Economic arguments about Brexit have succumbed to group-think', *Telegraph*, 5 June, available from <http://www.telegraph.co.uk/business/2016/06/05/economic-arguments-about-brexit-have-succumbed-to-group-think/>.
- Boulding, K. (1970) *Beyond Economics: Essays on Society, Religion and Ethics*, University of Michigan Press, Ann Arbor.
- Chakraborty, A. (2014) 'University economics teaching isn't an education: it's a £9,000 lobotomy', *Guardian*, 9 May, available from <https://www.theguardian.com/commentisfree/2014/may/09/university-economics-teaching-lobotomy-non-mainstream>.
- Chakraborty, A. (2017) 'The Econocracy review – how three students caused a global crisis in economics', *Guardian*, 9 February, available from <https://www.theguardian.com/books/2017/feb/09/the-econocracy-review-joe-earle-cahal-moran-zach-ward-perkins>.
- Colander, D. (2007) *The Making of an Economist, Redux*, Princeton University Press, Princeton NJ.
- Crick, F. (1995) 'The impact of Linus Pauling on molecular biology', *Proceedings of the Conference on the Life and Work of Linus Pauling*, Oregon State University, 28 February - 2 March, available from http://oregonstate.edu/dept/Special_Collections/subpages/ahp/1995symposium/crick.html.
- Davies, H. (2012) 'Economics in denial', *Project Syndicate*, 22 August, available from <https://www.project-syndicate.org/commentary/economics-in-denial-by-howard-davies>.
- Deaton, A. (2013) 'Letter from America: a Harvard graduate student is playing dice with your future', *Royal Economic Society Newsletter*, 161, pp.3–4, available from <http://www.res.org.uk/view/article1Apr13Correspondence.html>.
- Economist* (2015) 'Slower growth: disaster or blessing?', 1 July, available from <http://worldif.economist.com/article/12121/debate>.
- Hutchison, T. (1984) 'Our methodological crisis' in Wiles, P. and Routh, G. (eds) *Economics in Disarray*, Basil Blackwell, Oxford, pp. 1–21.
- Letto-Gillies, G. (2008) 'The RAE', *Royal Economic Society Newsletter*, 143, October, p.17.
- Inman, P. (2013) 'Economics lecturers accused of clinging to pre-crash fallacies', *Guardian*, 10 November, available from <https://www.theguardian.com/education/2013/nov/10/economics-lecturers-accused-university-courses>.
- Inman, P. (2014) 'Manchester University move to scrap banking crash module angers students', *Guardian*, 2 April, available from <https://www.theguardian.com/education/2014/apr/02/manchester-university-decision-scrap-banking-crash-module-angers-students>.
- Inman, P. (2016) 'Nobel prize in economics: the top contenders', *Guardian*, 7 October, available from <https://www.theguardian.com/business/2016/oct/07/nobel-prize-in-economics-the-top-contenders>.
- Inman, P. (2017) 'Chief economist of Bank of England admits errors in Brexit forecasting', *Guardian*, 5 January, available from <https://www.theguardian.com/business/2017/jan/05/chief-economist-of-bank-of-england-admits-errors>.
- Janis, I. (1982) *Groupthink: Psychological Studies of Policy Decisions and Fiascos*, Houghton Mifflin, Boston.

- Kalman, R. (1982) 'Identification from real data' in Hazewinkel, M. and Rinnooy Kan, A. (eds) *Current Developments in the Interface: Economics, Econometrics, Mathematics*, Reidel Publishing, Dordrecht, pp.161–196.
- Kay, J. (2015) 'We can reform the economics curriculum without creating new disciplines', 15 April, available from <http://www.johnkay.com/2015/04/15/we-can-reform-the-economics-curriculum-without-creating-new-disciplines>.
- Leontief, W. (1971) 'Theoretical assumptions and non-observed facts', *American Economic Review*, 61, 1, pp.1–7.
- Leontief, W. (1982) 'Academic economics', *Science*, 9 July, 217, pp.104–107.
- Mair, J. (2003) 'The agitators', *Guardian*, 10 July, available from <https://www.theguardian.com/education/2003/jul/10/students.uk>.
- Meeks, G. and Swann, P. (2009) 'Accounting standards and the economics of standards', *Accounting and Business Research*, 39, 3, pp.191–210.
- Miles, D. (2017) 'Andy Haldane is wrong: there is no crisis in economics', *Financial Times*, 12 January, available from <https://www.ft.com/content/f1c1dd00-d812-11e6-944b-e7eb37a6aa8e>.
- Minsky, H. (1986/2008) *Stabilizing an Unstable Economy*, McGraw Hill, New York.
- Morris, W. (1879/1966), 'Making the best of it: paper read before the Trades' Guild of Learning and the Birmingham Society of Artists' in *The Collected Works of William Morris*, 22, Russell and Russell, New York.
- Phelps Brown, E. (1972) 'The underdevelopment of economics', *Economic Journal*, 82, 325, pp.1–10.
- Plimmer, R. (1949) *The History of the Biochemical Society, 1911–1949*, Cambridge University Press, Cambridge.
- Portes, R. (2008) 'Secretary-general's annual report' *Royal Economic Society*, available from <http://www.res.org.uk/view/art1Apr08Features.html>.
- Romer, P. (2016) 'The trouble with macroeconomics', 14 September, available from <https://paulromer.net/wp-content/uploads/2016/09/WP-Trouble.pdf>.
- Russell, B. (1946) *History of Western Philosophy*, George Allen and Unwin, London.
- Shaw, G. (1903) *Man and Superman*, Constable and Company, London.
- Starbuck, W. (2009) 'The production of knowledge', slides of a lecture at Linköping, Sweden, available from <https://www.iei.liu.se/kite/dokument/1.117774/BillStarbuckpresent091012.pdf>.
- Swann, P. (2006) *Putting Econometrics in its Place*, Edward Elgar, Cheltenham.
- Swann, P. (2012) *Doubtful Significance: Can an Amorphous Cloud of Points Really Illustrate a Significant Relationship?*, research paper 2012-08, Nottingham University Business School, available from <https://ssrn.com/abstract=2127179>.
- Swann, P. (2014) *Common Innovation*, Edward Elgar, Cheltenham.
- University of Mississippi (2011) *The M Book: The University of Mississippi Handbook of Standards*, available from http://s3.amazonaws.com/os_extranet_files_test/7983_17385_mbook2011_12.pdf.
- Waldrop, M. (1994) *Complexity: The Emerging Science at the Edge of Order and Chaos*, Penguin Books, London.
- Wiles, P. (1984) 'Epilogue: the role of theory' in Wiles, P. and Routh, G. (eds) *Economics in Disarray*, Basil Blackwell, Oxford, pp.291–325.
- Wren-Lewis, S. (2017) 'Miles on Haldane on economics in crises', 13 January, available from <https://mainlymacro.blogspot.co.uk/2017/01/miles-on-haldane-on-economic-in-crises.html>.

G. M. Peter Swann
 Nottingham University Business School
 gmps2011@yahoo.co.uk

© 2017 G. M. Peter Swann
<https://doi.org/10.1080/08109028.2017.1339523>

