

THE CHANGING FACE OF MAINSTREAM ECONOMICS ?

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It has become increasingly fashionable to argue that economics is undergoing a process of fundamental change and that political economists are failing to recognise and productively adjust to this new reality. Such claims require analysis and response from political economists¹.

Those proclaiming a changed economics abound. Diane Coyle in her book *The Soulful Science: What Economists Really Do and Why It Matters* (Coyle 2007) argues that economics is now in a new and exciting phase. She sees the early to mid 1980's as the high watermark for neoclassical economics. According to Coyle, it was then that the neoclassical research programme came to be recognised as having become excessive, indulgent and faintly ridiculous. Since this time, Coyle views economics as having been in a process of steady retreat from absurdity.

Somewhat² similarly, John B Davis argues that, while neoclassical economics continues to dominate the curriculum, the mainstream research frontier differs from neoclassical economics and has as much in common with established heterodox schools as with neoclassical economics. Davis argues that the mainstream research frontier is now best classified as being 'mainstream heterodox'. It is further argued that there is a new generation of PhD students working in the mainstream research frontier and these younger scholars will slowly start to steer

1 The terms 'political economy' and 'heterodox economists' are synonymous for the purposes of this paper.

2 I use the word 'somewhat' here because in some respects Davis's analysis is more nuanced and also leads on to other arguments that are different to authors such as Coyle and Colander.

both research and teaching away from neoclassical economics (Davis 2006, 2007, 2008).

Coyle and Davis provide good examples of this ‘changing face of economics’ literature, but the most prolific and strident contributor to the literature is David Colander. Colander presents the ‘changing face of economics’ argument in its most vehement, voluminous and visible form (Colander 2000a, 2000b, 2003a, 2003b, 2005a, 2005b, 2008, 2009b). He not only outlines the full nature, and causal mechanisms, of the supposed intellectual revolution, but also has extensively chastised political economists for their dysfunctional response to this revolution and given them detailed instructions on how they must mend their ways. Because his analysis represents the changing face argument in its most developed form, it is useful to evaluate it as a test case.

The Go-Between

Colander has sought for many years to foster communication and understanding across established divides in economics by acting as a type of go-between. It is to be applauded that he seeks to do this and, in a number of ways, Colander is well suited to the role of mediator. While he clearly rejects the label of heterodox economist, he is not your run-of-the-mill orthodox economist. Colander, who refers to himself as ‘inside-the-mainstream-heterodox’ (Colander 2009b), states that he is often seen as a critic of mainstream economics and mainstream economics teaching by much of the profession (Colander 2005a). Indeed, his studies of graduate economics education (Colander & Klamer 1989) attracted the ire of some in the profession for allegedly damaging graduate enrolments in the United States (Colander 2009a), though these criticisms of graduate education have since evolved somewhat (Colander 2011; Colander 2005b, 2009a).

It must also be noted that some of Colander’s work, particularly his early work, was in keeping with many established heterodox positions: criticism of the excessive pursuit of technique over substance (Colander & Coats 1993); asserting that economic policy is an art not a science (Colander, Amato & Laudati 2001); and being critical of the way economists are educated (Colander & Klamer 1989). His earlier works, such as *Why aren’t Economists as Important as Garbagemen?* (Colander

1991) are also notable for their irreverent tone and often entertaining nature.

For all this, Colander has made some quite conservative methodological statements; and these statements help to explain his analysis of contemporary economics and his critique of political economists. For example, he asserts that economics is a science, as opposed to other 'ambiguous' disciplines such as political economy. It is a science because it builds formal models and then empirically tests those models. Economics should have 'no ideological bent at all' and should seek to be 'a philosophically neutral method of looking at a complex world' (Colander in Arnsperger 2008: xx). Myrdal's explanation of why 'values are always with us' appears to be rejected by Colander, just as it is rejected by nearly all mainstream economists.

Like Coyle, Davis and others, Colander believes that neoclassical economics is dying or dead (Colander 2000b) and that it is being replaced by developments that originate on the mainstream research frontier. This frontier is understood to include areas such as behavioural economics, experimental economics, evolutionary game theory, and in particular, complexity economics. This mainstream research frontier is said to be bringing about a desirable revolution in economics (Colander 2000a, 2000b, 2003b, 2005a, 2005b; Colander & Klamer 2007; Colander, Rosser & Holt 2004); bringing change to economic policy (Salzano & Colander 2007); change to economic teaching (Colander 2000a, 2003b); and even the history of economic thought (Colander 2000c):

Economists today are not neoclassical according to any reasonable definition of the term. They are far more eclectic, and concerned with different issues than were the economists of the early 1900s, whom the term was originally designed to describe (Colander 2000b: 130).

According to Colander, this revolutionary force will necessarily be attenuated by institutional inertia, but that will only delay the rising tide of change. What starts at the research frontier will inevitably reach policy and teaching.

The central trigger driving change is said to be the changing research interests of postgraduate students and younger staff members at elite universities in the US (Colander 2003b). In Colander's view, graduate students will choose research topics that are part of the mainstream

research frontier, and some of these students will then go on to become academics. It is then assumed that such academics will incorporate cutting-edge developments into the curriculum. The students who are the recipients of this renovated curriculum will then go further in expanding the research frontier when they choose their dissertation topics. These ongoing replicator processes are seen as the central driver of change, with other factors such as methodological critique being of only minor significance (Colander 2005b).

Colander provides two explanations for why graduate students and younger staff members will strike out into cutting-edge research. The first explanation is that the neoclassical research paradigm is becoming 'exhausted' (Colander 2003b: 1). This exhaustion appears to have less to do with running out of interesting puzzles to solve than it does with mainstream economists wanting to transcend the accumulated problems of the neoclassical program (Colander 2000a):

...mainstream economists know the standard problems with economics, and they are working to change them. Who does not want economics to be empirically grounded? Who does not want economics to be relevant? Who does not recognize that formalism sometimes runs amok? The debate is how to change economics, not whether it has problems. Economists working in the eclectic mainstream tradition are working to solve those problems, especially in cutting edge research (Colander 2005a: 338).

A key assertion to note (aside from the contestable rhetorical questions) is the belief that mainstream economists recognise real problems in their approach and are working to remedy these problems.

The second reason given for younger staff members and graduate students to embrace the mainstream research frontier is that advances in computing now mean that researchers can more easily conduct numerical simulations. Previously, in the absence of modern computing, numerical simulation was usually impractical. Model building had to be constructed via algebraic solutions. However, most dynamic equations cannot be solved algebraically, thus the scope for formal model building was previously limited. Table 1 shows that the majority of dynamical equations can only be addressed via numerical simulation.

Table 1: Scope for Using Analytical Methods to Solve Mathematical Problems

Equations	Linear			Non-Linear		
	One equation	Several equations	Many equations	One equation	Several equations	Many equations
Algebraic	Trivial	Easy	Essentially Impossible	Very difficult	Very difficult	Impossible
Ordinary Differential	Easy	Difficult	Essentially Impossible	Very difficult	Impossible	Impossible
Partial Differential	Difficult	Essentially Impossible	Impossible	Impossible	Impossible	Impossible

Source: Keen (2011: 409)

Numerical simulations are seen as so desirable because they extend the capacity for economics to be a model-building and model-testing discipline. For Colander, as with many neoclassical economists, this is what makes economics scientific and thus superior to political economy - a discipline he views as having a more ambiguous scientific status (Colander 2008). The increase in available mathematical methods is seen as offering a type of salvation, or redemption, for political economists, who are advised to discard their former predilections for analysis via discursive informal methods; indeed, he argues *all* economists are to embrace increasingly technical work if they wish to have any future:

I believe that the non-technical work of North, Williamson, or Coase is not the future of economics. Instead the future of economics is increasingly technical work that is founded on the vision that the economy is a complex system (Colander 2003b: 6)

It is worth stressing the assertion that acceptance of the economy as a complex system leads naturally to 'technical', computer-driven methods on the basis that these methods are more scientific.

Complexity Economics

Colander places considerable importance on a particular variant of the mainstream research frontier: complexity economics. Complexity economics is seen as not just revolutionising economics but revolutionising all the social sciences via the creation of a unified theory of the social sciences that is the equivalent of the unified field theory in physics (Hunt & Colander 2011). Complexity economics is a branch of

the science of complexity. Much of the early work in complexity economics originated at the Santa Fe Institute, where financial backing from Citibank has been important. Complexity science itself is somewhat of an umbrella term and its exact boundaries are not clear as it incorporates other areas of science such as chaos theory and network theory.

At the core of complexity economics is a conception of an economic system characterised by evolutionary processes of change that are, to some extent, adaptive and self-organising. Such systems are characterised by emergence, path dependence and positive feedback loops. They exist in historical time. They are also characterised by non-linearities and are often highly dependent on initial conditions (Beinhocker 2006; Kauffman 1995).

Such a conception of the economy will not strike many political economists as being cutting-edge. Explicit evolutionary theorising within economics goes back as far as Veblen and concepts of increasing returns, historical time and path-dependence are cornerstones of Post Keynesian thought. Because complexity economics has such features, and also because it usually rejects many of the tenets of neoclassical economics (such as equilibrium and perfect rationality), one might assume that complexity economics is something that should be best understood as in keeping with the traditions of political economy. This is not how Colander views complexity, arguing that the commitment to formal (mathematical) methods aligns complexity with orthodox economics (Colander 2000a: 5). Colander argues that 'good' orthodox economists have always raised the same questions and identified the same issues that political economists have. For Colander, the issue is simply that orthodoxy has not been able to accept the insufficiently scientific way political economy has responded to these questions and issues.

Colander's Critique of Political Economists

Colander considers political economists have had little role in changing economics (Colander 2005a, 2009b). Rather, he views change coming largely from inside the mainstream (Colander 2003b: 1). Attempts by radical critics to change economics, via sometimes confrontational methods and orientations, are seen as irrelevant, or even counter-productive (Colander 2005a, 2009b). He regards the marginalisation of

political economy as not being due to discrimination, intolerance and suppression by the mainstream – an explanation favoured by many political economists (Jones & Stilwell 1986; Lee 2009, 2012; Stilwell 2006, 2011) - but rather being due to the purported poor conduct, misconceptions and bad attitudes of political economists.

Political economists are admonished for not recognising the death of neoclassical economics (Colander 2000b) and thus for attacking a straw man (Colander 2005a). They are also accused of a lack of diplomatic savvy and good judgement, adopting the wrong ‘attitude’ and ‘tone’ towards their mainstream colleagues, alienating potential allies and collaborators within the eclectic mainstream:

...it is not beliefs that separate mainstream from heterodoxy; it is attitude and a willingness to compete within a given set of rules and institutional structures. Mainstream economists are willing to compete within those rules; heterodox economists aren't (Colander 2009b: 37).

In our view...inside-the-mainstream critics, want to separate themselves from that heterodox tone and attitude, not necessarily from heterodox ideas. It is because of the tone and attitude between the lines in heterodox writing that...inside-the-mainstream critics go to great lengths to disassociate themselves from heterodoxy (Colander, Rosser & Holt 2010: 404)

According to this reasoning, political economists, with their out-dated ideas and their bad attitude and tone, are self-marginalising. Given this description, the following prescription is provided for political economists. First, they should stop referring to themselves by terms such as heterodox. Second, they need to work on the assumption that mainstream economists are actively working to change and improve economics (Colander 2005a) — this is described as ‘giving the mainstream its due.’ Third, they have to fundamentally change how they go about their *own* work as economists. This involves less emphasis on methodology and more emphasis on forming alliances with mainstream economists who can help repackage heterodox ideas into a more formal (thus more respectable) guise. This amounts to adopting a stance of almost complete capitulation and deference. It is a strategy that stands in stark contrast to the demonstrably more effective strategy of greater institutional independence and differentiation for political economy (Thornton 2013a).

The Problem with the Colander Critique

There are substantial problems that afflict the ‘changing face’ analysis of economics. The view that complexity economics is a revolutionary force that will make for a more scientific economics is too simplistic. The science of complexity is a potentially useful contribution to economics, but its value to economics is contingent on being incorporated into economics in a way that is methodologically sensible. Economics has regularly been able to steal obscurantism out of the jaws of insight. For example, Blaug (1997) points out that when game theory first started gaining a profile in economics it was hoped that it would subvert established economic orthodoxy and lead to richer models of rational agency (Hargreaves-Heap & Varoufakis 2004). This has not occurred.

New insights are generally absorbed into mainstream economics in way that does not threaten core tenets of mainstream thought, with the more revolutionary insights being adopted within political economy. This is apparent in the production of various schisms: neoclassical Keynesian *versus* post Keynesian economics, old and new institutional economics, old and new behavioural economics. We might well find a similar dualism in complexity economics, where it is only ever absorbed into the economic mainstream via its bastardisation.

There is some evidence this process of bastardisation and bifurcation is already happening. For example, Steve Keen³, a prominent Post Keynesian, has raised concerns that some current work in complexity economics is incorporating IS-LM models and rational expectations, and in general lacks awareness of the history and debates with economic thought and methodology (Gallegati *et al.* 2006; Keen 2003). Furthermore, Velupillai sees the version of complexity promoted by Colander as ‘a disservice to those of us who would like to promote a complexity vision for, and of, economic theory, applied economics and the history of economic thought’ (Velupillai 2003: 4). This is more suggestive of *plus ça change plus c'est la même chose* than a revolution and unification of economics and all the social sciences.

What constitutes progress in economics is very much in the eye of the beholder. As Randall Wray has pointed out, one economist’s ‘cutting

3 Keen is a good example of how a political economist can engage with complexity in a productive manner: see Keen (2013).

edge research frontier' can readily be someone else's frontier of nonsense (Wray cited in Cohen 2009: 1), and much nonsense could come out of the 'complexity revolution'. John King's point about the history of the science being full of examples of 'prophets spurned, old truths forgotten or neglected, even older heresies enthusiastically embraced, and egregious errors pursued at great speed to the end of the appropriate cul-de-sac' (King 2002: 241-242) could be prescient for the supposedly imminent 'complexity revolution'.

Economic modelling using dynamic numerical simulations can be useful and interesting (see for example Epstein & Axtell 1996), while mathematics can be useful in clarifying and communicating ideas long associated with heterodoxy (Chick 2000; Keen 2009), but economics should not, and cannot, be totally beholden to Kelvin's dictum that 'when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind' (Thomson 1883: 73). Colander's analysis of progress in economics seems guilty of an old-fashioned misconception in equating the use of computers and mathematics with better science. This runs the risk of producing a more *scientistic* rather than scientific economics. By *scientistic* I mean the inappropriate use of the methods of the physical sciences in the social sciences (Stretton 1999). There is also no appreciation of the fact that, as models become more like reality, the modelling process itself becomes more difficult. Crespi's outlining of the problems that afflict mathematical models applies with as much force to complexity economics as it does to traditional mainstream economics:

...when one makes one's model more realistic by introducing more complex premises, one also thereby increases – sometimes dramatically – the problems involved in applying it. The more degrees of freedom in a model, the more parameters that have to be estimated, giving more potential sources of error. Moreover, the number of possible interrelationships that must be precisely specified grows geometrically with the number of parameters involved. The conclusions reached as the model is made more complex become less robust – more sensitive to small variations in the initial parameters - and greater and greater precision in the data inputs is needed to avoid reaching indeterminate conclusions. The result is often an elegant and complex but relatively useless model that cannot produce determinate results unless one has recourse to an often unavailable comprehensive and precise data set. This problem of unwieldiness is particularly likely to occur when the refinements introduced into a model

require the measurement of subjective factors – such as changes in attitudes or limitations on cognitive capabilities that are inherently difficult to measure and quantify, and to relate to other, more tangible factors in mathematically precise ways (Crespi 1997: 154).

The advent of powerful and cheap computer hardware and software can be useful, but it can only go so far in transcending the type of challenges outlined by Crespi, not to mention even more fundamental challenges, such as how to account for structure-agency problems and issues of emergence. There is clear evidence that these problems are being underestimated in the rush to proclaim a complexity revolution:

In agent-based modelling one essentially studies the economy by creating a virtual economy, which is then used to simulate policies. These simulations are used to guide policy-makers. No equilibrium needs to be imposed on the system; *all one needs to do* is specify the characteristics of the agents and the environment they operate in (Colander 2008: xiv, emphasis added).

If only it could be that simple: to *fully* and *accurately* specify the characteristics of individuals and the environment they operate is nearly always a very difficult, if not impossible, thing to do. In some respects it becomes *more* difficult to do this via numerical simulations because the slightest mis-estimation of any of the variables may lead to dramatically different outcomes. This creates doubts over the capacity for complexity simulations to reliably advise policy-makers.

The complexity revolution will struggle with the same problems that have always beset social and economic analysis. Conceiving of the economy as a complex adaptive system is of little more practical use than is conceiving of the economy as an open system: it is just an ontological starting point. If one wants to make theory operational, one is faced with the familiar challenges of where to make the provisional closures and abstractions. At this point all the established methodological schisms open up. Given their greater methodological literacy, complexity economists might therefore profitably seek the assistance of political economists, rather than the *vice-versa* arrangement that Colander advises. Another issue to consider is that the idiographic, unpredictable and difficult-to-model nature of complex systems may simply be too unpalatable to the mainstream, and thus may continue to be largely ignored. Complexity economics does not give rise to clear and

unambiguous answers, does not offer elegant theoretical proofs, and does not generate analysis that can reliably serve the given social and economic order. To understand the economy as a complex adaptive system is an invitation to modesty rather than hubris. All of these aspects of complexity economics make it hard to view as the sort of theory that will capture the imagination of mainstream economists. A marketing perspective is useful in respect of this point:

In many ways then the success of a theoretical approach should be understood just as a marketing manager would attempt to understand the success of a consumer or industrial product. If it is launched at an inopportune moment, is poorly packaged, is too complex for the consumer to grasp, and fails to appeal to traditional values, then it will not find a market (Dow & Earl 1982: 177-178).

If one also considers how much loyalty there is for the established product of orthodox theory and the ease this product has in supplying all the basic purposes of economics in contemporary capitalist society (which include being an instrument of social control) then further doubt is cast over its level of the demand for a fundamentally new economics. In general, there are copious examples of how intolerant neoclassical economists are to considering perspectives unfamiliar to them, and in seeking to undermine those that do offer such perspectives (Butler, Jones & Stilwell 2009; Glenn 2009; Thornton 2013b)

Colander's advice to political economists to repackage their ideas in mathematical form to gain favour with their orthodox colleagues is also rather problematic. Many political economy ideas do not fit, and may never fit, into mathematical form, at least not without some serious loss of potency or insight. A good example of this tension is 'circular and cumulative causation'. Argyrous has noted that, in 1944, when Myrdal was developing Veblen's original concept, he initially thought he should be able to put it into an interconnected series of quantitative equations for a truly 'scientific' solution (note his initial connection of mathematics and the truly scientific). However, by 1978 Myrdal came to the conclusion that this earlier hope was not possible. He came to opt for 'detailed historical analysis of the particularity and peculiarities of individual industries and countries' (Argyrous 2011: 150). Whereas Myrdal thought that certain aspects of circular and cumulative causation might be suitable to mathematical measurement and modelling, this

could only provide part of the explanation; fieldwork, historical case studies and the methods and ideas of the other social sciences were also required (Argyrous 2011).

Finally, there is a basic empirical problem: there is scant evidence of complexity gaining traction in research, teaching or policy. In regards to research, Leombruni and Richiardi conducted a survey of papers on agent based modelling (ABM), a cornerstone of the complexity approach, and found almost nothing in the top 20 mainstream journals: only eight articles out of a total 26,698 articles surveyed (Leombruni & Richiardi 2005: 104). US graduate students have also complained that top journals are conservative in what they seek to publish (Harley *et al.* 2010). The curriculum is also showing little sign of innovation. For example, of the 1,629 subjects surveyed in the Australian universities' economics curricula, less than 12 dealt specifically with areas of economics said to be revolutionising the discipline (Thornton 2013a). The most obvious aspect of contemporary economics is its complacency amidst a background of real-world crisis (Quiggin 2013).

Colander's analysis seems to be largely a reflection of his own methodological conservatism: economics should be (in the main) a formalistic modelling science (Colander 2009b) that utilises empirical verification to identify objective truth (Colander 2008). His political philosophy may also be very relevant. For example, he avoids criticising American society for limiting openness. This is due to his personal observations of radicals in the 1960s trying to restructuring society. He is also 'cynical about academics' ability to effect positive change' (Colander 2008: xxii). The new science of complexity is thus deployed to support these old and conservative beliefs:

Academics themselves are part of a complex system and, as such, need checks imposed on them by the society that funds them. If, after hearing an academic's arguments for the competition of ideas, society chooses to limit that competition, as it has generally done, it may well mean that sustainable complex systems must limit the degree of critical thought that their institutions are held up to. Complex systems, looked at from a critical perspective, will always fail to meet expectations. It is possible that continually pointing out those failures can undermine the sustainability of a complex system. It may be for this reason that real-world sustainable complex systems have always developed protection mechanisms that either prevent too much critical

thinking, or that push the serious critical thinking outside of the mainstream of the system—into such places as academic environments. So I am open to the possibility that critical thought itself must be reflexive and accept that critical thought is limited (Colander 2008: xxii).

Such statements about limiting the degree of critical thought to uphold the order of society recall Shapin and Schaeffer's important point that solutions to the problem of knowledge are inseparable to solutions to the problem of social order (Shapin & Schaffer 1985).

Conclusion

This article centres on two important claims made by David Colander: that mainstream economics is undergoing a revolution of which complexity is a central part; and that political economists are not recognising and responding to this change. In presenting a critical analysis of Colander's views, it contends that political economists should be open to complexity and to any other theory or method that may be potentially useful. In fact, because political economists are generally more methodologically aware, have greater sense of the history of their discipline and the contributions of other disciplines, they are better placed to develop complexity in a more intellectually meaningful and socially useful way. It is admirable to seek to promote communication and understanding across the great divide in economics. This is clearly one of Colander's primary aims. However, what is revealed in this particular instance is just how deep this great divide is, and how difficult it is to span.

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