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Microfoundations

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Comments, as always, most welcome.

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Abstract

The paper argues that the microfoundations programme can be understood as an implementation of an underlying methodological principle, methodological individualism, and that it therefore shares a fundamental ambiguity with that principle, viz, whether the macro must be **derived from** and therefore **reducible to**, or rather **consistent with** micro-level behaviours. The pluralist conclusion of the paper is not that research guided by the principle of microfoundations is necessarily wrong, but that the exclusion of approaches not guided by that principle is indeed necessarily wrong. The argument is made via an examination of the advantages claimed for dynamic stochastic general equilibrium models, the relationship between parts and wholes in social science, and the concepts of reduction, substrate neutrality, the intentional stance, and hypostatization.

1 Introduction

The microfoundations of macroeconomics project has attracted considerable critical attention, including hundreds of papers and several books, over recent decades. Yet the matter is now of greater importance than ever. The macroeconomics which practitioners actually do – in leading centres for research, including central banks – and the macroeconomics which we teach – at postgraduate, and increasingly at undergraduate levels – are overwhelmingly based on the dynamic stochastic general equilibrium (DSGE) approach – that is, they are “micro-founded”. For the use of DSGE models in central banks, see below, and Harrison et al (2005). The first words of Williamson (2002: xxi) – a standard mainstream intermediate undergraduate text in macroeconomics, with many subsequent editions – are “this book follows a modern approach to macroeconomics by building macroeconomic models from microeconomic principles”, while the first words of Wickens (2011: 1), a standard postgraduate macroeconomics text, are “Modern economics seeks to explain the aggregate economy using theories based on strong microeconomic foundations”.

The present paper makes a number of comments on the microfoundations project. The next section locates the origin of the project in the postwar neoclassical synthesis, and addresses, and rejects, the usual assumption that the approach is rooted in the Lucas critique of econometric policy evaluation. The Smets-Wouters model is briefly considered as an example, and the advantages claimed for it by the European Central Bank (ECB) are appraised. A second substantive section analyses the microfoundations approach in relation to an underlying methodological approach, namely methodological individualism, and suggests that both approaches share an ambiguity regarding the relation between micro and macro. The section concludes with a discussion of Watkins’s “half-way” and “rock-bottom” explanations, and of top-down versus bottom-up methodological stances. A further substantive section addresses the relationship between wholes and parts in social science, drawing on the concepts of *substrate neutrality* and *the intentional stance* due to Daniel Dennett. The relevance of the concept of hypostatisation is explained and the standpoint of Mises and Nagel on hypostatisation is contrasted with that of Smith, Marx, Hayek, Dawkins, Toynbee and Dennett. The final substantive section addresses the use of the concepts of equilibrium and the representative agent, arguing that a number of key assumptions required for tractability are essentially *ad hoc*. A final section draws the conclusion that while the use of microfoundations for one’s research is a legitimate strategy, the use of a requirement for microfoundations to police the research of others imposes heavy costs.

2 History and importance of microfoundations

This section will give a very brief statement of the historical origin of the topic – brief as most of it is very well known – and some discussion on the importance of the topic today. That significance lies in that most modern mainstream macro is based on DSGE, and, although the microfoundations issue long pre-dates DSGE, it is DSGE which is regarded as the microfoundation of mainstream macroeconomics today. We will therefore have to identify the relationship between DSGE models and microfoundations.

It is generally thought that in the postwar period neoclassical microeconomics fused with Keynesian macroeconomics to constitute the neoclassical synthesis. This is a simplification. The “neoclassical

microeconomics” referred to here is the Marshallian partial-equilibrium approach, and the “Keynesian macroeconomics”, a bowdlerised, neoclassical re-interpretation of some of Keynes’s ideas. But there is another trend which, like Marshallian partial equilibrium, emerged from the marginalist revolution of the late-nineteenth century, namely Walrasian general equilibrium theory. Partly because Walras’s *Elements of Pure Economics* was published in French, and partly because it was couched in a mathematical formalism for which the profession was not yet ready, this framework for thought about the economy had remained relatively obscure. After the Second World War, a number of factors, including the use made of general equilibrium arguments by the socialist side in the socialist calculation debate, the greater mathematisation of the discipline, and the translation of the *Elements* into English, prepared the ground for a significant and rapid improvement in its profile. Since general equilibrium is an attempt to theorise the economy as a whole, it can be viewed as an alternative, or at least an alternative **to**, macroeconomics. There were, therefore, a microeconomic trend, and **two** macroeconomic trends in play:

Around the mid-1950s two more or less separate approaches existed to studying economy-wide phenomena: general equilibrium theory and (Keynesian) macroeconomics ... The neoclassical synthesis reconciled general equilibrium theory and (Keynesian) macroeconomics by giving each of them its own domain of applicability: macroeconomics (with its assumption of sticky money wages) gives an accurate description of the economy in the short run, while long-run developments of the economy were considered to be adequately described by the general equilibrium approach. (Janssen, 2008: 2-3)

However, this synthesis led to dissatisfaction. The sticky money prices of the one contradicted the market-clearing assumptions of the other; moreover the generally accepted tenet of methodological individualism that the macro must at least be consistent with individual decision-making suggested that the Walrasian approach was in some sense more basic. According to Janssen (2008: 3) this was the trigger for the quest for microfoundations – the search for a description of agent-level behaviour from which aggregate-level consequences could be derived. The solution which the profession has converged on is DSGE modelling with representative agents, and the school of thought which has adopted this solution, following the merger of the New Keynesian and Real Business Cycle schools of thought, has been called the New Neoclassical Synthesis (NNS):

the NNS models have become the standard workhorse for monetary policy analysis ... Bayesian NNS models ... combine a sound, microfounded structure suitable for policy analysis with a good probabilistic description of the observed data and good forecasting performance. (Smets & Wouters, 2007: 587)

The Lucas critique (Lucas, 1976) is often regarded as an important step in the development of the microfoundations project and is still frequently referred to in support of a micro-founded approach, so it warrants discussion here. The problem is that the Lucas critique is a critique of inductive-theory-based empirical models. It is not a critique of any theory of macroeconomic entities. Such entities are assumed not to exist and ignored in the critique. So what it does say – that observed macro-level regularities cannot be assumed to remain regular in the event of a change in the rules of the game, such as a change in government fiscal or monetary policy – is perfectly reasonable; it is what it doesn’t say – whether macro entities can exist – which for us is the real point. Lucas’s critique is essentially a syllogism (Lucas, 1976: 41). The major premiss is that “the structure of an

econometric model consists of optimal decision rules of economic agents” – this is what is “given”, what he can assume that his audience will agree with. But it is this major premiss that already impounds microfoundations, not the remainder of the syllogism, which merely adds that a change in policy will change those decision rules, so the model will change – so don’t try to forecast the effect of a policy change using historical data which assume that the policy has **not** changed. The major premiss says that “the structure of the econometric model” (that is, our model of macro phenomena) “consists of” (is founded in, is completely reducible to) “optimal decision rules of economic agents” (the microfoundation of the macro phenomena).

The importance of the microfoundations issue is very simply that modern mainstream macroeconomics is based entirely on DSGE models. And DSGE in turn is synonymous with microfoundations. General equilibrium theory “is coextensive with the theory of the microfoundations of macroeconomics” (Weintraub, 1977: 1-2). “DSGE ... models are built on microeconomic foundations” (Sbordone et al, 2010: 23).

An interesting example can be seen on the ECB webpage relating to the research of the ECB (ECB, nd1). Links are provided to pages discussing four models, and to a report evaluating the research carried out at the bank. The latter foregrounds the “stellar example” of “the new area-wide dynamic stochastic general equilibrium model, used for producing ECB forecasts and policy simulations” (Freedman, et al, 2011: 31). Three of the four models mentioned are described as micro-founded. One such is a model developed by Frank Smets and Raf Wouters, in an article entitled “An estimated dynamic stochastic general equilibrium model of the Euro area” (Smets & Wouters, 2003). By the middle of the last decade this model was regarded as “a modern workhorse and benchmark model for analyzing monetary and fiscal policy” (Uhlig, 2007: 3), and it is now used routinely by central banks around the world, including the Federal Reserve and the ECB. (A senior central bank researcher, who must remain anonymous, complained to me in 2005 that the only model permitted at his place of work was a DSGE model with a representative agent.)

The Smets-Wouters model, according to the ECB webpage (ECB, nd2), combines “a rigorous microeconomic derivation of the behavioural equations of macro models with an empirically plausible calibration”, and offers three main advantages – advantages which are worth dwelling on:

1. “They [sc microfoundations] provide a theoretical discipline on the structure of the model that is being estimated, which may be particularly helpful in those cases where the data themselves are not very informative, for example regarding the long-run behaviour of the economy or because there has been a regime change.
2. “Being able to relate the reduced-form parameters to deeper structural parameters makes the use of the model for policy analysis more appropriate, i.e. less subject to the Lucas critique, as those structural parameters are less likely to change in response to changes in policy regime.
3. “Micro-founded models may provide a more suitable framework for analysing the optimality of various policy strategies as the utility of the agents in the economy can be taken as a measure of welfare” (ECB, nd2).

This neatly summarises the rationale for adopting microfounded – ie DSGE – models. They provide a modelling structure where the data, when allowed to speak for themselves, fail to say anything very much, they avoid the Lucas critique, and they provide a basis for estimating the desirability of policy. Let's consider these in turn. We have already seen that the Lucas critique has nothing to say on the existence of macro entities worthy of consideration in their own right, but *assumes* that modellers will adopt a microfoundations approach. The assertion is that these “structural” parameters – ie the tastes and preferences of households and the technology available to firms – are “deeper”, ie more rooted in agent behaviour than “ad hoc” atheoretical econometric parameters. The first point, that microfoundations “provide a theoretical discipline” and may be helpful for looking at the long run or examining cases of “regime change” is essentially the same point. The claim is that “ad hoc” models may be unable to say anything about the long run, and may be misleading if there has been a regime change. Micro-founded models can point towards long-run trends and should not be vulnerable to regime changes – that is, they avoid the Lucas critique.

The final point, that micro-founded models provide a basis for comparing the optimality of alternative policies is set out clearly by Woodford:

A second advantage of proceeding from explicit microeconomic foundations is that in this case, the welfare of private agents – as indicated by the utility functions that underlie the structural relations of one's model of the transmission mechanism [of monetary policy] – provides a natural objective in terms of which alternative policies should be evaluated. (Woodford, 2003: 12)

Woodford spells this out in Ch 6, “Inflation, Stabilization and Welfare”:

An important advantage of using a model founded upon private-sector optimization to analyze the consequences of alternative policy rules is that there is a natural welfare criterion in the context of such a model, provided by the preferences of private agents, which are displayed in the structural relations that determine the effects of alternative policies. Such a utility-based approach to welfare analysis has long been standard in the theory of public finance. It is not too common in analyses of monetary policy, perhaps because it is believed that the main concerns of monetary stabilization policy are assumed away in models with explicit micro-foundations. But we have seen [in previous chapters] that models founded on individual optimization can be constructed that ... allow for realistic effects of monetary policy upon real variables. (Woodford, 2003: 382)

Wren-Lewis comments on this:

Woodford's approach to deriving the objectives of benevolent policy makers has been immediately adopted in the literature, such that papers now routinely use this approach in deriving policy objectives. This is despite the fact that such derivations may result in policy objectives that are highly unrealistic, because the models from which they derive generally contain no unemployment and no bankruptcies. Wren-Lewis (2011: 131)

Not only are the models unrealistic in the sense Wren-Lewis describes, relating to the assumptions of the model, but they are also unrealistic in the Friedmanian sense that they do not make good predictions: according to a recent Bank of England Working Paper discussing the Bank's forecasting

platform “the *absolute* forecast performance of DSGE models and their competitors is poor. In terms of their ability to forecast individual variables, like GDP and inflation, these models typically fail to beat simple univariate statistical models” (Burgess, et al, 2013: 7). According to Dotsey, writing in a journal of the Federal Reserve Bank of Philadelphia, “at short horizons (one quarter), DSGE models do about as well as purely statistical procedures when forecasting output and inflation, but at horizons of one year, they do somewhat better ... However, forecasts that use various model restrictions in forming priors still generally outperform those from DSGE models” (Dotsey, 2013: 14 and n7).

The lack of realism of DSGE models makes them highly unsuitable for policy evaluation. Moreover, the argument that the utility function that underlies the structural parameters of a model provides a basis for the examination of social welfare is erroneous. These utility functions are not derived from the behaviour of specific individuals, but posited as the property of a single individual to which the economy as a whole has been reduced. That means that they are quite divorced from the wants and needs of any really existing individuals in the economy.

Moreover, this habitual mode of presentation of the matter – one in which microfoundations merely offer “advantages” – is disingenuous. What is much more worrying is that the requirement of microfoundations acts as a shibboleth, facilitating a policing function. The criterion of the presence of microfoundations can be used to ensure that only the orthodox get published and are attended to. Wren-Lewis mentions an unnamed conference he had attended “within the microfoundations modelling community”:

The concern expressed at the conference ... was not that papers that included non-microfounded elements were mislabelled, but that these papers should not have been discussed alongside fully microfounded models. Typically the argument would be that serious academic analysis should be restricted to fully microfounded models, and that any hybrid models should be reserved for discussion elsewhere. (Wren-Lewis, 2011: 137)

For example, “papers analysing inflation inertia should only be discussed in (the better) academic circles after the microfoundations for such behaviour have been worked out”. So microfoundations are, or at least on this view should be, required as a prerequisite for the “serious” discussion of a researcher’s work. Wren-Lewis considers at length the example of price rigidity. The problem here was that, despite empirical evidence that such rigidities existed, and acceptance by economists that that was a very relevant consideration for macroeconomic models, price rigidities were seen as contradicting the assumption of rational individual behaviour: “Why did agents write fixed price contracts, when it appeared to make them worse off? The argument that such contracts existed in reality did not appear forceful enough: internal consistency overrides external consistency.” (Wren-Lewis, 2011: 139). The consequence was that there was a hiatus of more than two decades before it became respectable to include price rigidity in mainstream models. Only once thoroughly microfounded models with price-rigidity had been demonstrated, starting with a 1995 paper by Obstfeld and Rogoff, was such an approach considered respectable.

Thus mainstream economics partitions macroeconomic research activity into two kinds: microfounded models, regardless of their distance from reality, are scientific, while “ad hoc” models, that is, everything else, regardless of their proximity to reality, are conjectures, which may or may

not lead to scientific theory to the extent that, over time, they are discovered to be amenable to being microfounded.

It is worth pursuing this a bit further. The overall conclusion of Wren-Lewis is that internal consistency has taken a central position in the development of macroeconomics:

the dominant approach in macroeconomics ... appear[s] to adopt a clear methodological approach, which promotes internal consistency above external consistency as a necessary condition of admissibility ... macromodelling based on microfoundations represents a methodological position that is clearly distinct from other approaches in macroeconomics. In particular, this type of modelling elevates internal consistency to a necessary condition of admissibility. In contrast, external consistency, in the sense of conformability with empirical evidence, is desirable ... but it is not essential.... The key to the rapid adoption of Woodford's approach is that it avoids an internal inconsistency that might otherwise arise between different parts of the model. Thus the realism of these derived social welfare functions is not the key issue. They have been immediately adopted because they are internally consistent with the rest of the model. Wren-Lewis (2011: 131)

Since I will discuss the issue of consistency in the next section it is important to be clear that what Wren-Lewis is talking about here is something different. The consistency that I will discuss in one possible interpretation of methodological individualism is that between phenomena and their substrate. The consistency that Wren-Lewis is discussing is that between different parts of a theoretical ensemble. This becomes clearer when he discusses the modelling strategy of the ECB. At the core of that strategy is the Smets-Wouters model. But recall that the ECB's boast was that this strategy combined "a rigorous microeconomic derivation of the behavioural equations of macro models with an empirically plausible calibration". The calibration of the Smets-Wouters model involves Bayesian estimation of the relevant parameters of the model,

However, estimation is not allowed to immediately modify the theoretical structure and dynamics of the model, as it could do in a structural econometric model for example. Instead estimation follows the RBC tradition where data dynamics not captured by the theory is ascribed to unexplained shocks, which as a result can be highly persistent. This, together with tight priors on estimated parameters, ensures that empirical evidence does not compromise the internal consistency of the model's structure. Perhaps later new theory might be developed or applied which could explain this dynamics, but only in a way that ensured the internal consistency of the model remained in place. (Wren-Lewis, 2011: 131)

Moreover this procedure, such that empirics are at all costs prevented from contaminating and hence undermining the core, is even clearer in the case of the Bank of England:

At the centre of the Bank's latest forecasting model ... is a 'core' that is essentially an elaborate DSGE model. Inconsistency with the data is handled by additional 'non-core' equations, which relate variables from the model's core to the data ... Critically, data inconsistency is not allowed to 'infect' the core, but instead determines the structure of the non-core equations. The non-core equations are completely recursive to the DSGE core: there is no feedback from non-core variables to the core. In a more traditional structural econometric model, there is no distinction between core and non-core, and any additional

dynamics suggested in the data would be incorporated directly into the model's equations. In the Bank's approach the structure of the core is protected from such 'data-based' augmentation. (Wren-Lewis, 2011: 131-2)

What Wren-Lewis has uncovered in his discussion of the twenty-year delay in the recognition of price stickiness, and the core/non-core distinction invoked by central banks to avoid infection of the theoretical core of their models with empirical facts, is an academic discipline which seems prepared to go to any lengths to protect the core of the neoclassical paradigm, that is, the principle that the economy can be understood by starting with optimising agents and building up. The same thing can be seen to have happened as the discipline came to terms with the criticisms of perfect competition brought forward by Austin and Chamberlin. According to Minsky these views were deemed acceptable to the extent that they left profit-maximisation and hence the price system untouched:

A reason which can be advanced for the ready acceptance by economists of profit-maximizing behavior, independently of the changes in the market structure which are introduced in their analysis, is that profit-maximizing behavior leads naturally to mathematics in which derivatives of the difference between total cost and total revenue are set equal to zero. In this sense, under profit-maximization the behavior of the competitive and non-competitive firm are formally identical – the mathematical set-up is the same. The complexity added by non-competitive firms is resolved by the introduction of the demand elasticities confronting the firm at appropriate places in the analysis. In general equilibrium analysis, the existence of monopoly does not lead to any adjustment in the equilibrium relations if profit-maximizing is assumed; rather the effect of different degrees of monopoly is in the distribution of income and the allocation of resources. (Minsky, 2004: 100 n 20)

The emergence of microfoundations as a talisman to facilitate a policing function can be understood in this perspective. The neoclassical mainstream can accept innovations once it is convinced that they can be reconciled with its core propositions, in particular, that we can start with rational agents who maximise utility and profits, and derive macro results from their behaviour. Approaches which contradict this view can be designated "ad hoc" and unscientific. As will be argued in the next section, this proposition constitutes a strongly reductionist world-view deriving from methodological individualism.

3 Analysis of microfoundations

The microfoundations programme can be understood as an application of an underlying standpoint, methodological individualism, widely held to be fundamental to neoclassical economics as well as Austrian economics: "The quest for microfoundations grew out of the widely felt, but rarely explicitly stated, desire to stick to the position of methodological individualism" (Janssen, 2008: 1). Howitt, too, speaks of

the reductionist methodological predisposition that economists of almost all persuasions share to some degree, according to which no explanation of economic phenomena is truly satisfactory if it does not reduce the phenomena to a question of individual actions by basic decision-making units. (Howitt, 1987)

It is this reductionist methodological predisposition – methodological individualism – which requires economics, in particular macroeconomics, to have its foundations in microeconomics. But as the literature on methodological individualism shows, there is much ambiguity and confusion concerning what it might mean. In particular, the core principle of methodological individualism can be expressed in two apparently similar but actually profoundly different claims:

A: macro-level phenomena must be derived from, that is, reducible to, micro-level phenomena; and

B: macro-level phenomena must be consistent with micro-level phenomena.

Claim B is difficult to argue with: it is not clear what kind of paradigm would be unconcerned by inconsistency of this kind. And indeed the motivation for the bulk of microfounded research work is typically expressed in terms of this internal consistency. Claim A expresses the standpoint, sometimes known as atomism, encapsulated in the statement that the whole is the sum of its parts. Adopting an admittedly procrustean approach to the use of words, in what follows I shall refer to this as *reductionism*: a reductionist account of something, in this use of the term, regards the thing as constituted by its elements **taken in isolation**. Claim A is a special case of the looser formulation in claim B: if X is reducible to Y, the two are certainly consistent; but consistency does not itself entail reducibility. According to semantic holism, for example, the meaning of a sentence must be consistent with the meaning of the individual words, but cannot be reduced to them. Rather it emerges from the way the words *relate* to each other. For a reductionist, there is no consistency beyond reduction, but for a non-reductionist there is. There is considerable debate amongst philosophers as to whether or not apparent instances of such *emergence*, as the putative existence of irreducible entities is called, refute reductionism (defined in this sense). It is therefore at the very least unwarranted and premature for economists to assume that that debate is over and that reductionism is necessarily correct.

The microfoundations programme inherits this ambivalence from methodological individualism. It is logically possible to advocate a non-reductionist microfoundations approach, where the requirement to demonstrate microfoundations only means that individual behaviour must be shown to be consistent with the macro entities posited by the theory adopted. The micro in this case is sufficient, but not necessary, for the macro to be possible. A researcher has ticked this particular box when he has shown that there is a possible constellation of individual-level behaviours which could underpin the posited macro entities of the model, without being required to show that it is that very constellation which really does underlie the macro phenomena addressed by the model.

We have then, two possible stances. Either macro models need to be microfounded, that is, shown to be an inevitable outcome of a specific constellation of micro behaviours – this is the approach of DSGE modelling today. Or macro models need to be shown to be consistent with at least one constellation of possible micro behaviours. This is the alternative – there is no supposition that any constellation of micro behaviours will necessarily lead to this rather than that macro outcome. Multiple outcomes are potentially consistent with a given set of micro behaviours.

There is another dimension to be considered. This involves both a whole-and-parts aspect and a time aspect. It may be that there is a difference between what a science as a whole should aim at,

and what should be required of an individual researcher or study. As is well known, Watkins distinguishes between such “half-way” and ultimate or “rock-bottom” explanations:

There may be unfinished or half-way explanations of large-scale social phenomena (say, inflation) in terms of other large-scale phenomena (say, full employment); but we shall not have arrived [sic] at rock-bottom explanations of such large-scale phenomena until we have deduced an account of them from statements about the dispositions, beliefs, resources, and inter-relations of individuals (Watkins, 1957: 106).

It may be that we all want macroeconomics as a whole to be well founded, in the sense of being consistent with a possible constellation of micro-level behaviours, and, indeed, that we will regard the science as incomplete until it isolates the true micro-foundation. But, if we adopt Watkins’s stance here, that does not in any way imply that *any specific enquiry* is defective if it fails to attain to “rock-bottom” explanation. A theoretically compelling and empirically supported theory can still be enlightening without any micro-level support whatever. However, the absence of a reference to the corresponding micro-level theory will always be felt as an absence, a lacuna that in due course needs to be addressed. The tension here is between the individual study and the science as a whole; it is itself a micro-macro dichotomy.

A further issue which is clearly related to, and indeed sometimes conflated with, the issues of microfoundations and methodological individualism, should be mentioned here. As just argued, we might well want a macroeconomics which is shown to be consistent with a constellation of micro behaviours, and we will want to find out what those behaviours are. This implies that it is legitimate to work towards this reconciliation from either direction – top-down or bottom-up. The finished theory, if ever a theory can be finished, will be a unity of bottom-up and top-down explanation. In the process of developing that theory, top-down and bottom-up theorising and explanations will both contribute. That top-down and bottom-up approaches are in principle equally valid is exemplified by Friedman’s (1976: 316) statement that while both he and Keynes used a top-down methodology, most Keynesians and monetarists used a bottom-up approach. Similarly, Trotsky (1973: 233-234) illustrates a discussion of Marxist notions of science by means of equally approving references to the top-down psychological approach of Freud and the bottom-up research strategy of Pavlov. Herbert Simon, too, contrasts “the usual conception of the sciences as building upward from elementary particles, through atoms and molecules to cells, organs and organisms” with “[t]he actual history”, which “has unfolded, as often as not, in the opposite direction, from top down” (Simon 1996: 172). The implication is that both procedures are to be regarded as legitimate scientific research strategies. My own view here is that the choice of top-down or bottom-up heuristic is a wholly pragmatic matter: there is no issue of principle here, no golden key to knowledge of the world. The methodologically pluralistic statements of Simon, Trotsky, and Friedman are therefore to be endorsed.

4 The relationship between parts and wholes in social science

The key question I wish to focus on, which underlies the microfoundations debate, concerns the relationship between parts and wholes. I would like to explore this by drawing on the work of Daniel Dennett on the “intentional stance”. If we adopt Watkins’s distinction between unfinished and rock-

bottom explanations, we can, according to Dennett, make gigantic strides towards helpful and scientific – but not rock-bottom – explanations by invoking the intentional stance:

when they [sc the designers of a VCR] engage in reverse engineering – of some other manufacturer’s VCR, for instance – they avail themselves ... of what I call the *intentional stance* – they try to figure out *what the designers had in mind*. They treat the artefact as a product of reasoned design development, a series of *choices* among alternatives, in which the *decisions* reached were those *deemed best* by the designers. Thinking about the postulated functions of the parts is making assumptions about the *reasons* for their presence, and this often permits one to make giant leaps of inference that finesse one’s ignorance of the underlying physics, or lower-level design elements of the object (Dennett, 1995: 229-230)¹.

It is interesting to dwell on the terms in this passage which Dennett chooses to emphasise. The intentional stance focuses on what the designer *had in mind*, what he *chose*, what he *decided*, what he *deemed best*, and his *reasons*. The parallels with neoclassical economics are striking, although of course, in the dénouement Dennett’s designer turns out to be the Darwinian evolutionary algorithm, and no human designer at all.

Dennett’s approach here is completely consistent with Simon’s “pragmatic holism”: “In the face of complexity, an in-principle reductionist may be at the same time a pragmatic holist” (Simon, 1962: 468). This is a standpoint that I reject. In my view a bottom-up explanation of organic entities in terms of particles and subordinate components of the thing studied will always be incomplete without an account of purpose, the reason the part is there, the function of the part in the whole. Where there is an organic relationship, the whole is a precondition for the explanation of the parts. In these cases it makes sense to say that the micro is macrofounded.

This is not to say that congeries don’t exist: clearly they do. Marx famously compared mid-nineteenth century peasant small holdings in France to potatoes in a sack (Marx, 1937). And, of course, where there are purposes, they do not override or displace causation but work *through* causation. The important bit of the job is to discover where there are top-down and bottom-up explanations and successfully to marry them up. So in Dennett’s account, the role of the intentions considered by the intentional stance are not *auxiliary*, a helpful short-cut, merely “finessing your ignorance of the gory mechanical details” (Dennett, 1995: 359), but an *essential* part of explanation. Explanation is incomplete without it, whatever the level of detail of one’s knowledge of the substrate level may be.

Dennett captures this in his discussion of substrate neutrality:

The procedure for long division works equally well with pencil or pen, paper or parchment, neon lights or skywriting, using any symbol system you like. The power of the procedure is due to its logical structure, not the causal powers of the materials used in the instantiation, just so long as those causal powers permit the prescribed steps to be followed exactly. (Dennett, 1995: 50-51)

¹ Throughout the paper emphasis in cited passages is exactly as in the original source.

The claim I am making here is that just as mathematics or, in Dennett's argument, evolution, is a substrate-neutral algorithmic process, so is economic activity, and indeed not coincidentally, since that activity can itself be seen as an evolutionary process. But that takes us too far from our present theme. That theme is the relationship between macro and micro, and the Dennettian argument is that the macro – inflation, unemployment, and so on – is *instantiated* in the micro – a substrate of human agents. Just as Dennett says that there are some requirements for a substrate for long division, there are requirements for the human substrate of economic phenomena – posited macroeconomic entities must be *consistent* with the causal powers of the human substrate. But, given that constraint, they may be implemented in a wide range of agents – from agents with limited rationality driven by rules of thumb, to perfectly rational optimisers, for example, if those macro level entities are to any degree substrate-neutral. If so, then it will in general not be possible to deduce or derive the macro from the micro. Rather, the micro behaviours observed will be determined by the macro phenomena instantiated in them.

In discussing the behaviour of a person we could never be satisfied by an account, however complete, in terms of molecules and cells, let alone of fermions and bosons – even though we know that the person consists of nothing else. We would need to know about the person's identity, his past, his goals, his preferences. While it is of course the case that every aspect of the individual is underpinned by material substance, by organic activity at the cellular and system level, knowing about these subordinate levels, to any desired level of detail, would still leave us asking for more, asking about the beliefs and motivations of the individual. This is what Hayek seizes on as the foundation for the claim that his methodological approach is "individualist". We can intuit what it is like to be a person because we ourselves are persons – we can draw on *Verstehen*:

it is the concepts and views held by individuals which are directly known to us, and form the elements from which we must build up, as it were, the more complex phenomena ... it is the attitudes of individuals which are the familiar elements and by the combination of which we try to reproduce the complex phenomena, the results of individual actions, which are much less known. (Hayek, 1979: 65; see also Simpson, 2013: 7)

But this is only the start. This is an application of the intentional stance to other people. But they are not the only potential agents in the world. We can understand the purpose of things because we have purposes. We can think about the *meaning* of the Antikythera mechanism, or Paley's watch, because we know what it means to *mean* something. Whatever the provenance of either mechanism, we could not be satisfied by an account exclusively in terms of the component parts, the wheels and pinions and gears. We would have to be told how those parts *interacted* to achieve the *purpose* of the whole. For Dennett

There is no substitute for the intentional stance. Either you adopt it, and explain the pattern by finding the semantic-level² facts, or you will forever be baffled by the regularity – the

² Syntax is about the rules for manipulating words, semantics about their meaning. Dennett's discussion of what *West Side Story* and *Romeo and Juliet* have in common illustrates the point: what they share is "not a string of English characters, not even a sequence of propositions ... What is in common, of course, is not a syntactic property or system of properties but a semantic property or system of properties: the story, not the text; the characters and their personalities, not their names and speeches ... So it is only at the level of *intentional objects*, once we have adopted the intentional stance, that we can describe these common properties" (Dennett, 1995: 356).

causal regularity – that is manifestly there ... Even if you can describe, in matchless microdetail, every causal fact in the history of every giraffe who has ever lived, unless you go up a level or two and ask “Why?” ... you will never be able to *explain* the manifest regularities, such as the fact that giraffes have come to have long necks. (Dennett, 1995: 421)

Clearly, Dennett here is not positing a conscious designer of the giraffe’s neck; rather the designer is the Darwinian evolutionary process. Now the big issue is, whether indeed there *are* causally efficacious entities operating at social levels above that of the individual human agent. For some it is obvious that there are not, for others equally obvious that there are. This is the paradigmatic chasm across which we are trying to build bridges. The core issue here is that of *hypostatisation*. Hypostatisation is the attribution of substance or real existence to concepts or abstractions (Greaves, 1974: glossary entry for *hypostasis*). Mises sets out the view that hypostatisation is a mental error with great clarity in a subsection of *The Ultimate Foundation of Economic Science* entitled “The Pitfalls of Hypostatization”:

The worst enemy of clear thinking is the propensity to hypostatize, i.e., to ascribe substance or real existence to mental constructs or concepts.

In the sciences of human action the most conspicuous instance of this fallacy is the way in which the term *society* is employed by various schools of pseudo science ... society itself is neither a substance, nor a power, nor an acting being. Only individuals act ... Society does not exist apart from the thoughts and actions of people. It does not have “interests” and does not aim at anything. The same is valid for all other collectives (Mises, 1962: 78).

Kant, too, accused his intellectual antagonists of “a delusion in which they hypostatise something that exists merely in thought – that is, they treat it as a real object existing ... outside the thinking subject” (Kant, 2010: 191). Nagel agrees. Pointing out that the “extension” of, for example, *the French Enlightenment* (that is, whatever it is that the phrase “the French Enlightenment” refers to) “cannot be articulated with unlimited detail”, he suggests that this failure may lead to a “hypostatic” conception of it as a causally efficacious unitary whole:

such a hypostatic transformation of a complex system of relations between individual human beings into a self-subsisting entity capable of exercising causal influence is the analogue of vitalistic doctrines in biology ... such hypostatic interpretations have been useless as guides in inquiry and sterile as premises in explanations ... [T]he methodological assumption that all collective terms designate either groups of human individuals or patterns of behaviour leads to a more fruitful way of identifying the extensions of such terms than does the perplexing hypostasis of mysterious super-individuals. (Nagel, 1979: 537)

So, for Nagel, the answer to hypostatisation is, precisely, methodological individualism.

For writers as diverse as Marx, Hayek, Keynes, Dawkins, Toynbee and Dennett, however, it is pretty much a given that such “super-individual” entities exist, and the issue is to identify them and explain their working. For Keynes the class of parasitic *rentiers* and the institutional structure of atomistic

capitalism are creations of society which served their own interests, interests which now diverge from ours (Denis, 2002b). For Hayek, the networks of social relations within which individuals are embedded undergo a process of natural selection such that the traditions we inherit embody the rules we must follow, even if we don't understand them. For Hayek, value "can only be understood as the determinant of what people must do to maintain the overall structure" of the system within the individual is embedded (Hayek, 1983: 36). Traditions here clearly exist and follow their own logic. For Hayek, this logic is to act in our interest, but no mechanism is specified which guarantees this (Denis, 2002a). For Toynbee, the unit of social analysis is the civilisation, the "intelligible field of study" (Toynbee, 1972: 45). The activities which take place within the civilisation are directed towards the maintenance of the civilisation, for example, the sustenance of a minority, including the soldiers, administrators and priests, who are free from the necessity of producing the material requirements of the society (Toynbee, 1972: 44). For Dawkins and Dennett, the individual is itself a hypostatisation: individual organisms are "gigantic lumbering robots" built by genes to serve as their vehicle, but a vehicle which comes to have its own interests, which diverge from those of its creators (Dawkins, 1989: 19, 332; Dennett, 1995: 471). Finally, for Marx, states and capitals are hypostatisations of the activity of social individuals, organic social forms which have acquired their own interests, opposed to the interest of, and parasitic on, the human substrate of which they are formed (Denis, 2011).

If substrate neutrality applies, then it is not the preferences, technology, and resource constraints facing the individual human which matter. What matters is the properties of the social evolutionary algorithmic process which is *instantiated in this substrate*. When individuals act socially they are implementing plans and decisions of which they care nothing and often do not even know. For Adam Smith, the capitalist "intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention" (Smith, 1776: IV.ii.9). Hayek, too, knows this, and so does Marx. For these writers the key economic phenomena with which we are to deal are the **unintended consequences** of our self-seeking behaviour. Those consequences are coherent social plans, and it is those plans that must be studied to understand what the individuals are doing.

It is not my purpose here, however, to argue that all or indeed any of these views are correct. All that we need to say is that they are not all obviously incorrect. The possibility of non-human social entities cannot be dismissed *in limine*, but has to be explored and – if incorrect – refuted in each case. The microfoundations programme as a research strategy is a legitimate approach³ for individual researchers to adopt. As shibboleth it is an indefensible means to the exclusion of non-mainstream approaches to economics.

³ This statement has been rejected by readers. An anonymous referee has criticised the statement, saying that this legitimacy has not been established – and indeed *cannot* be established, and another has said that "atomism is treated too leniently". I am not asserting that this is a *good* research strategy, or one I would endorse; rather I am saying that it is up to individual researchers to follow whatever strategy they choose. I am very reluctant to endorse proscriptions on particular research strategies. It is possible to generate insight even within profoundly mistaken paradigms. Pasteur made his discoveries in the process of attempting to refute theories of spontaneous generation, which he regarded as contradicting the Christian doctrine of creation.

5 Equilibrium and the representative agent

The DSGE approach is an *equilibrium* approach and it therefore behoves us to examine the relevance of the notion of equilibrium which it expresses to the microfoundations project. I have already said a bit about the neoclassical deployment of the concept of equilibrium (Denis, 2007), and I won't repeat that here. DSGE is also characterised by use of "representative agents", usually just the one, but in some cases a small number of heterogeneous agents, standing for all the agents in the economy. We explore this below, starting with the representative agent and concluding with equilibrium.

Modern general equilibrium theory is built on the work of such writers as Walras, Ramsey and Arrow and Debreu. It is notable that Walras regarded himself as a scientific socialist (Morishima, 1977: 2) while the Ramsey model of 1928 (Ramsey, 1978: 261-281) is a model of the actions to be taken by a central planner who wishes to establish the optimal rate of saving for the community (Wickens, 2011: 2-3). This suggests a revision of Paul Sweezy's apothegm, such that modern neoclassical macroeconomics is the economics of socialism, while the economics of such writers as Keynes and Marx is the economics of capitalism. To some extent, it is not that DSGE is wrong, but that it happens – as Keynes liked to say about the classical economics of his time – not to describe the economy we actually inhabit, namely a *capitalist* economy. To be specific, mainstream economics treats the economy as one of direct exchange of products for products, use-value for use-value, rather than as exchange driven by the needs of capitalist accumulation – in Keynes's language, a real exchange economy rather than a monetary production economy.

Wickens identifies as the "starting point" for the study of DSGE models a small general equilibrium model "commonly known as either the Ramsey (1928) model or the representative-agent model". The Ramsey model is a representative-agent model in the sense that a single agent is set the optimisation problem of choosing the fraction of its income to save. This agent is regarded as "a nation" (Ramsey, 1978: 261), and so can be said to represent all the agents in that economy. So the assumption is that the whole of society can behave as a single individual; viewed as such we can say exactly what the nation must do, if the individual agent representing it is a *Homo economicus*. Now – *pace* many heterodox critics – it is not wrong to consider what humanity as a whole will do, or what it must do in order to behave rationally, in its interaction with the physical world. Marx does this in Chapter 1 of *Capital*, where he considers the activity of Robinson Crusoe on his island, as he allocates his labour between the various activities required to produce the products that he consumes. Marx concludes the paragraph with the very surprising statement that "All the relations between Robinson and the objects that form this wealth of his own creation, are here so simple and clear as to be intelligible without exertion ... And yet those relations contain all that is essential to the determination of value" (Marx 1954: 81). The surprise, and the deep significance of this, is that "value" in *Capital* (*Wert* in the German) is ineluctably a *social* category in Marx's analysis – it simply *cannot* apply to the case of the individual producing and consuming use-values, such as Crusoe. Robinson, here, is a *synecdoche* standing for society as a whole. This is brought out in the subsequent pages. In a "community of free individuals", that is, under socialism, Marx says, "[a]ll the characteristics of Robinson's labour are here repeated, but with the difference that they are social, instead of individual. Everything produced by him was exclusively the result of his own personal labour, and therefore simply an object of use for himself. The total product of our community is a social product" (Marx, 1954: 82-3). A "community of free individuals", for Marx, is

one in which capitalism has been overcome, a socialist community. So for Marx, like Walras and Ramsey, the isolated individual is a figure standing for society as a whole, when that society is conceived as a community of free individuals.

What neither Marx nor Ramsey did, however, was to extend this normative view of what a (socialist) society should do so as to make it a descriptive statement of what a capitalist society actually does. That is the approach of the Walrasian tradition issuing in today's DSGE paradigm.

Turning to the issue of equilibrium, we note that there are two equilibria to consider – short- and long-run. This attempts to graft a Marshallian construct on to the Walrasian framework. We can summarise the DSGE position in the somewhat paradoxical-sounding statement that the economy is always in short-run and never in long-run equilibrium. The economy is in short-run equilibrium by definition. “The economy is viewed as being in continuous [short-run] equilibrium in the sense that, given the information available, people make decisions that appear to be optimal for them ... The economy is assumed to be always in short-run equilibrium” (Wickens, 2011: 1). This is irrefutable: whatever obstacles to such a state of affairs may be proposed, can simply be impounded in the constraints the agent faces – the individual does not have enough information to act rationally? No, he does the best he can subject to this constraint – by forming expectations rationally, for example. He lacks computing power? Again, he does the best he can – by means of habits, institutions, and so on – subject to this constraint. There is nothing intrinsically wrong with this – a definition cannot be wrong, only of greater or lesser utility. As we will see below, the problems arise when that short-run equilibrium is *interpreted* in a specific way.

On the definition of short-run equilibrium agents are doing the best they can subject to constraints – but that cannot mean that the outcome is optimal in the sense that they have no incentive to change their behaviour in subsequent periods. One constraint might be that in period $t - 1$ they do not know what other agents intend to do in period t . When period t arrives, the actions of the agents, whose intentions other agents had to surmise, are now evident. Mostly the outturn behaviour of others will be different from what the agent had expected; the agent will therefore have an incentive to adjust his behaviour to adapt to the new circumstances. Keynes's “beauty contest” analysis in the *General Theory* (Keynes, 1973: 156) supports this view. Agents, Keynes says, are in the business, not of forecasting the fundamental value of assets, but of forecasting the general opinion of the general opinion of ... asset values in the next period. There is an infinite regress separating the expectations from the underlying fundamentals. This gives rise to “conventional” values in which the future is assumed to resemble the past (Keynes, 1973: 152), punctuated by inherently unpredictable landslides in sentiment caused by waves of optimism and pessimism sweeping the investment community. The standard interpretation of the short-run equilibrium which the economy is always in, as one in which there is no incentive for the agent to change his behaviour, is an *additional*, tacit, unsupported, and indeed wholly unwarranted, assumption.

Turning to the long run, the economy will, in practice, never be in long-run equilibrium as “The long run, or the steady state, is a mathematical property of the macroeconomic model that describes its path when all past shocks have fully worked through the system”. This will in general never be reached as the economy is continually subject to new shocks. “It is not, therefore, the economy that is assumed to be in long-run equilibrium, but the macroeconomic model” (Wickens, 2011: 1). The long-run equilibrium can be “either a static equilibrium, in which all variables are constant, or, more

generally, a growth equilibrium, in which in the absence of shocks, there is no tendency for the economy to depart from a given path” (Wickens, 2011: 1).

This approach makes a number of profound, but tacit, assumptions. The distinction which Wickens makes, between the model and the economy, is well taken. But the nature of the difference between the two is systematically understated. For Wickens, the *economy* is in short-run equilibrium (so the economy and model coincide in the short run), and the only reason that it is not in long-run equilibrium is because the latter is what happens when past shocks have all been adjusted to, but in reality new shocks are arriving continually. However (a) there is no guarantee that the short-run equilibrium that the economy is in will gravitate, in the absence of further shocks and as adjustment to past ones is completed, to a long-run equilibrium or steady state. On the contrary, there might be no final stopping point, or the system might cycle endlessly or behave chaotically. And (b) it might not be a matter of adjusting to exogenous shocks at all, but of the emergence of endogenous change within the system (Simpson, 2013: 6; Denis, 2007).

These assumptions are made for one reason: tractability. The consequence is that they are essentially *ad hoc*, not derived from anything more basic. There is no *theoretical* reason why the economy that the model is supposed to represent will display these features. The DSGE approach is thus guilty of the ad-hocery of which it convicts “traditional macroeconomics”. As John King says, in this very context, “the temptation to slide from the intractable real world into tractable fantasy worlds is very difficult to resist” (King, 2012: 228; see also Colander et al, 2008: 238).

6 Conclusion

The paper has made a number of comments on the microfoundations project. It is suggested that the advantages claimed for a micro-founded approach are unconvincing. This is because the alternative in general is wrongly posed as that between DSGE/RBC models, on the one hand, and traditional, theoretically inconsistent econometric modelling, on the other. It is an instance of Timothy Garton Ash’s apothegm that the selection of the counterfactual question to be asked often anticipates the desired answer. The real opportunity cost, however, is the exclusion of such theoretical contributions as Hayek’s theory of social evolution, Marx’s analysis of capital as a parasitic complex of social relations, and Keynes’s theory of aggregate demand.

The microfoundations approach is analysed in the paper in relation to methodological individualism, suggesting that both approaches share an ambiguity regarding the relation between micro and macro. Does the assertion of a requirement for microfoundations mean, uncontroversially, that the macro must be consistent with the micro, or, more demandingly, that the micro must be reducible to and derivable from the micro? The latter is identified as expressing a reductionist or atomistic standpoint, such that the whole is just the sum of its parts. Such a requirement is shown necessarily to exclude a vast array of contributions to economic science. A discussion of Daniel Dennett’s concepts of substrate neutrality and the intentional stance suggests that such non-reductionist approaches cannot be easily dismissed. Further it is suggested that hypostatisation – the ascription of substance and causal efficacy to abstractions – is a key feature of the approaches excluded by the requirement of microfoundations, and that it is unreasonable to exclude such approaches *in limine*. Finally, it is argued that the assumptions which microfounded approaches make in connection with

the representative agent and the notion of equilibrium at the heart of DSGE show a striking degree of ad-hocery – a failure to ground key assumptions required for tractability. I conclude that the adoption of a micro-founded approach to a research project is unexceptionable – its likely success a matter for the researcher. The use of the requirement for microfoundations as a shibboleth, policing the research activity of others, however, is to be deplored.

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