

Microfoundations

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Paper for AHE Annual Conference, July 2013, London Metropolitan University

Very preliminary draft – not for citation or further distribution. Thank you!

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 **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 hypostatisation.

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, such as central banks – and the macroeconomics which we teach, at postgraduate and increasingly undergraduate levels, are overwhelmingly based on the dynamic stochastic general equilibrium (DSGE) approach – ie, 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 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. The final 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. 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 the fact that most modern mainstream macro is based on DSGE, and it is specifically the latter which is regarded as the microfoundation of macroeconomics. 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, viz 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 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 fortunes. 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).

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) 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.

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 – ie don't forecast the effect of policy using historical data which assume that the policy has not changed. The major premiss says that "the structure of the econometric model" (ie our model of macro phenomena) "consists of" (is founded in) "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).

An interesting example can be seen on the European Central Bank (ECB) webpage relating to the research of the ECB (<http://www.ecb.int/home/html/researcher.en.html>). 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 the Smets-Wouters model, developed by Frank Smets and Raf Wouters, in an article (Smets & Wouters, 2003) entitled “An estimated dynamic stochastic general equilibrium model of the Euro area”. 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) 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 (http://www.ecb.int/home/html/researcher_swm.en.html), 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, nd).

This neatly summarises the rationale for adopting microfounded – ie DSGE – models. They avoid the Lucas critique, they provide a modelling structure where the data, when allowed to speak for themselves, fail to say anything very much, 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 final point, that micro-founded models provide a basis for comparing the optimality of alternative policies is set out 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 (2011: 131) 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.

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).

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 microfoundations acts as a shibboleth, facilitating a policing function. The criterion of the presence of micro-foundations 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 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 economists working within the paradigm partition economic research activity into two kinds: microfounded models, regardless of their distance from reality, are scientific, while “ad hoc” models, 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.

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. It is 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 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 refute reductionism (defined in this sense). It is therefore at the very least unwarranted for economists to assume that reductionism is necessarily correct.

The microfoundations programme inherits this ambivalence. 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, three possible stances:

1. Macro models need to be micro-founded, that is, shown to be an inevitable outcome of a specific constellation of micro behaviours;
2. Macro models need to be shown to be consistent with at least one constellation of possible micro behaviours; and
3. There is no need for macro models to be shown to be consistent with any possible constellation 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. 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 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. We will recall this passage later.

Dennett's approach here is completely consistent with 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. That is not to say that congeries don't exist. 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

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

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 "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).

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 – are 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 economic entities must be *consistent* with the causal powers of the 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).

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 *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 *hypostatization*. Hypostatization is the attribution of substance or real existence to concepts or abstractions (Greaves, 1974: glossary entry for *hypostasis*). Mises sets out the view that hypostatization 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).

Nagel agrees. Pointing out that the “extension” of, for example, *the French Enlightenment*, that is, whatever it is that the phrase “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

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).

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, 2005).

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 being *implemented 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 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. To put it another way: Individuals can be assumed to follow their interests. What therefore is of interest is to discover what it is that drives the changes in those interests that we observe.

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 approach for individual researchers to adopt. As shibboleth it is an indefensible means to the exclusion of non-mainstream approaches to economics.

5 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 is in general 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. Finally it is suggested that hypostatization – 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*. 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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