Are There Dangers in the Microfoundations Consensus?

Simon Wren-Lewis
University of Exeter

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1. Consensus through microfoundations

Twenty years ago, a standard way to teach macroeconomics was to contrast alternative ‘schools of thought’. There was a Keynesian approach, a Monetarist approach, a New Classical approach and so on.[1] Each school of thought had it’s own basic model, and it was often unclear how these models were related to each other. This made it hard work for students, but perhaps the notion of gladiatorial combat between schools added spice. Microeconomics was, and still is, rather different. By and large, there was a clearly defined core of theory that was widely accepted as mainstream.

Nowadays macroeconomics, at least at the more advanced level, is taught in a very different way.[2] Different schools of thought have largely disappeared. Instead we teach students that in macroeconomics, as in microeconomics, there is a mainstream core, associated with representative firms maximising profits and a representative consumer maximising intertemporal utility in an environment of perfect capital markets. By and large macroeconomists no longer label themselves using particular approaches, and it does not define their research.[3]

Of course controversies in macroeconomics still exist. In particular, the debate about what constitutes the main propagation mechanism behind the business cycle remains unresolved. It is possible to talk about ‘New Keynesians’ on the one hand, and those following a ‘Real Business Cycle’ approach on the other. However, labelling these different approaches as different ‘schools of thought’ would give a misleading impression, for two related reasons. First, both approaches to modelling the business cycle share a large amount of common ground. Both adopt the core assumptions concerning firms and consumers outlined above, and both assume rational expectations. Second, this common core allows a clear understanding of how the two approaches differ. In fact, it would not be a complete exaggeration to say that the two approaches differ only to the extent that one (the New Keynesian) assumes nominal inertia, while the other does not.[4]

So, for example, David Romer in his very popular graduate textbook (Romer, 2001) includes two chapters on the implications of nominal inertia (including analysis based on his own contributions to this literature), but these are preceded by chapters presenting the Solow growth model, infinite horizon and overlapping generations models, new growth theory and real business cycle theory. In their textbook on international macroeconomics, Obstfeld and Rogoff (1996) place an analysis of price rigidities in their last two chapters. This does not represent in any sense a downgrading of nominal inertia: indeed, their final chapter outlines a model of their own that incorporates nominal inertia that has

[1] See Snowden and Vale (1997), for example.

[2] See, for example, Romer (2001), Obstfeld and Rogoff (1996) and Blanchard and Fischer (1986), which are among the most popular graduate macroeconomic texts.

[3] While there may be consensus in the mainstream, there remain active groups outside the mainstream, such as the post-Keynesians.

[4] It is true that New Keynesian models are more likely to be based on imperfectly competitive markets rather than perfect competition. However, this is in part because imperfect competition facilitates the analysis of nominal inertia and increases its impact.

proved highly influential in current research. Instead it reflects the fact that there is a wealth of material in international macroeconomics that would be regarded as independent of any New Keynesian/RBC debate.[5]

It is possible to describe this emerging consensus in terms of a synthesis between alternative schools: Romer (1993) talks of a New Keynesian Synthesis, while Goodfriend (2002) describes a New Neo-Classical Synthesis. It is certainly the case that the new consensus adopts a good many of the ideas originally associated with New Classical economists, such as rational expectations, or intertemporally optimising consumers and Ricardian Equivalence. However, I think it is more illuminating to view this emerging consensus in macroeconomics as an inevitable consequence of the microfounding of macroeconomics.[6]

Over the last two or three decades, there has been a growing acceptance that macroeconomic theory and macroeconomic models should be explicitly derived from microeconomic analysis. In the case of consumption, for example, it is no longer acceptable to write down a consumption function with income, interest rates and wealth as arguments simply because such a formulation seems reasonable, or because it fits the data. Instead, consumption functions need to be derived from explicit optimisation by agents.

Analysis today would start with a representative consumer that maximised discounted utility, where utility depends on consumption each period, and probably labour supply each period. We add a period budget constraint, which together with an assumption about unconstrained borrowing and lending gives us an intertemporal budget constraint. Maximisation gives us a consumption function, where consumption depends on total wealth (current financial wealth plus human capital), and the real rate of interest[7]. This function can also be written as an Euler equation, where current consumption depends on future expected consumption and the real rate of interest.

A consumption function of this type is at the heart of nearly every macroeconomic paper in the current academic literature. Why has this formulation replaced the typical Keynesian consumption function (which is based on current income rather than human wealth)? I would argue that it has little to do with econometric evidence: the ‘excess sensitivity’ of consumption to current income within the intertemporal framework is well known. Instead I believe it is the clear microfoundations of the intertemporal consumption function that has led to its domination in the academic literature.[8]

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[5] Traditionally Keynesian economists have been seen as more critical of market processes compared to their neoclassical counterparts. Of course it follows directly from the externalities generated by nominal inertia that interventionist monetary policy can make Pareto improvements. It is also probably true that those economists more ‘attuned’ to market failure might find it easier to work with nominal inertia, but I doubt that many would claim any logical connection. We can also note that more than one well known New Keynesian has been an economic advisor to President George Bush!

[6] If the goal of New Classical economists was simply to impart microfoundations, then the two views are identical.

[7] If utility is logarithmic, consumption is a fixed proportion of total wealth.

[8] The same applies to Ricardian Equivalence. The evidence that consumers behave in a Ricardian manner is extremely mixed, yet Ricardian Equivalence is typically the reference point from which any analysis of the impact of temporary tax changes begins. Once we recognise that the government is also subject to an intertemporal budget constraint, then Ricardian Equivalence follows naturally with intertemporal

By becoming microfounded, macroeconomics has inherited the consensus of microeconomics. Of course this does not imply uniformity, if only because considerable variation can occur in microeconomic analysis. It does, however, imply a certain commonality in approach, which gives rise to the feeling of a current consensus, at least compared to the diversity of earlier times.

The microfounding of macroeconomics has been a gradual process. It has not been without incident, as the battle over rational expectations showed, but at the end of the day nothing has stopped its progress. In particular, it can be argued that two recent developments in the last ten years have completed the microfoundations of macroeconomics.

The first was provided by Michael Woodford (see originally Rotemborg and Woodford, 1997, but more particularly Woodford, 2003), who showed how the traditional objective function assumed for policy makers (quadratic terms in output and inflation) could under certain conditions be derived from the utility of the representative agent (assuming policy makers were benevolent), and how in this case the trade-off between inflation and output stabilisation is no longer ad hoc.

The second was the extension of the microfoundations approach into the construction of large scale forecasting models used by policy makers. As I discuss in the next section, the Bank of England's new model has a Stochastic Dynamic General Equilibrium (SDGE) model at its core.

In my view growing consensus in macroeconomics is an inevitable by-product of the microfounding of macroeconomics. In the next section I will claim that this change has not just brought consensus, but it has also changed the methodological foundation of macroeconomics. The microfoundations project has changed how macroeconomics is done, both in academia and policy making institutions. To understand the full implications of the microfoundations project (and hence the growing consensus), this methodological change must also be understood.

Economists do not normally feel comfortable in talking about methodology. In this they are quite different from their colleagues in other social sciences. One reason for this discomfort may be that those economists who do talk methodology are often outside the mainstream, and use methodological arguments to attack the mainstream. This is neither my position nor my intention. However, every methodological approach has its problematic elements, and policy makers as well as academics need to be aware of what these are. In particular, the last section of this paper highlights a debate between what I describe as 'microfoundations purists' and 'microfoundations pragmatists', the outcome of which may have important implications for how macroeconomics develops.

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consumers. Although we need the fiction of 'Barro bequests' to achieve exact Equivalence, models based on finite lives and no bequests (such as the model of perpetual youth) give rise to results that are quantitatively very close.

2. Internal and external consistency: a contrast of methodologies

To understand how the methodology of macroeconomics has changed, consider the process of building an empirical macroeconomic model, to be used by a central bank or finance ministry for forecasting and policy analysis. Twenty years ago, the emphasis would have been on the relationship between the model's equations and the historical data. Macroeconomic theory would have been used to suggest equation specification, but if parameter restrictions implied by theory were rejected in econometric tests, they would not survive in the model. The theoretical justification for including particular variables in relationships was often fairly loose, and there was a marked absence of cross equation parameter restrictions. No one worried too much if the theoretical rationale for one part of the model appeared to be different from another part, as long as both parts tracked the data.[9]

Contrast this with the approach taken by the Bank of England in building their new forecasting model (see Harrison et al (2005)). I choose this example partly because it is familiar to me (I had an advisory role in its construction), but also because I believe it is at the frontier of this type of model development, and because it represents a considerable intellectual achievement by those Bank economists who built it.[10] In this model, theoretical consistency plays a critical role. The model consists of a set of 'core' relationships, which are derived from a unified theoretical approach, and where parameters have been largely calibrated rather than estimated. Parameter restrictions implied by theory are always imposed. Data consistency is achieved by a set of 'non-core' relationships, which are subsidiary to (in technical terms, recursive to) the core model[11]. The core model is an extremely elaborate and complex Stochastic Dynamic General Equilibrium (SDGE) model, and data inconsistency revealed in non-core relationships is not allowed to 'infect' this SDGE model, but instead provides an indicator for the future theoretical development of the core.

Now consider two methodological approaches to economic knowledge. The first, often associated with Karl Popper, but advocated eloquently by Mark Blaug (Blaug, 1980), sees data consistency as a defining characteristic of scientific method. While theory confirmation is problematic (observing a million white swans cannot prove the statement 'All swans are white'), theory rejection is decisive (observing a black swan disproves the statement). Bogus scientific theories either ignore data rejection, or propose statements that are immune from rejection. Proper science proposes theories that could be rejected by the data, and when they are, the theory is replaced.

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[9] Models of this kind are often called Structural Econometric Models (SEMs).

[10] I also choose it as an example because it seems to represent a growing trend. The Bank's approach follows similar developments in the central banks of Canada and New Zealand, and developments in modelling in other countries appear to be following in the same direction.

[11] Non-core equations will typically take the form $A(L)X = B(L)X^{*}+u$, where $A(L)$ and $B(L)$ are polynomials in the lag operator, X is the data for some variable, X^{*} is the equivalent variable from the core model, and u is a white noise error. In some cases, however, additional variables may also appear on the right hand side. Critically, however, core variables (X^{*}) are only determined by other core variables, and not non-core variables (X), so the non-core is recursive to the core.

The second methodological approach, which has sometimes been described as axiomatic or deductive, is rigorously expounded in Hausman (1995). This sees economic theory as being constructed from a small number of fundamental axioms, of which rationality is the most important. Rationality is not an article of faith, but is an empirical proposition that is backed up by a variety of types of evidence, from experimental data to the ability of individuals to learn from their mistakes.[12] This solid foundation gives theories derived from these axioms the presumption of empirical relevance. When it comes to testing these models, the methodology follows Mill (Mill, 1843) in stressing that theory proposes 'tendencies', and so correspondence with data will always be inexact. Even where data rejection appears clear cut, this does not lead to complete theory rejection, but instead represents 'puzzles' that require theory adaptation or augmentation.

Whereas Hausman applied his methodological account only to the core of microeconomic theory, I would argue that the microfoundations project has extended it to macroeconomic theory. The basic axioms of microfounded macrotheory are the same as microeconomic theory itself, together with the aggregation assumption of the representative agent.[13]

I think the two ways of doing macroeconomic modelling that I described fit quite closely with the two methodological approaches.[14] The pre-microfoundations approach puts the stress on data consistency: models that are not consistent with the data (in an econometric sense) should be rejected. In contrast, the Bank of England's new model embodies a quite different approach. Internal consistency is vital, because only then can we be sure that relationships are consistent with the axioms of microeconomic theory. Econometric consistency is not essential (it is 'handled' via ad hoc, non-core relationships), but instead is a pointer to future theoretical development.

Seen in this methodological light, some features of the microfoundations project are easier to understand. The 'debate' over rational expectations was so strong and intense in part because each side had different presumptions about what a theory should be. Lucas described rational expectations as a 'consistency axiom': as economic theory depended on rationality, and rationality implied rational expectations, what was there to argue about? To its detractors, the rational expectations hypothesis was empirically incredible; how could its proponents continue to ignore its frequent rejection in the data? The almost

[12] For example, one of the key components of rationality in transitivity: if x is preferred to y, and y is preferred to z, then x will be preferred to z. Agents that do not obey transitivity can be exploited in what is described as a 'money pump'. As a result, it is argued, non-rational agents will learn to behave rationally in order to be better off.

[13] The assumption of a representative agent is not trivial, of course, and has been strongly criticised by some (for example, Kirman, 1992). Microfounded models will probably be forced to incorporate at least limited heterogeneity in the future if they are to tackle important macroeconomic phenomena such as unemployment.

[14] An exact correspondence is problematic for a number of reasons that I cannot explore here. In particular, with probabilistic theories the contrast between observations that confirm and those that reject becomes weaker. Abandoning complete theories following data rejection hardly ever happens, as Lakatos and others have discussed. We would also need to examine the methodological position of models that are almost entirely statistical (as in a VAR, for example).

commonplace use of rational expectations today reflects the dominance of the microfoundations approach, which stresses internal consistency.

I do not want to enter a debate here about whether macroeconomics should be following one methodological approach rather than another. For better or for worse, it is currently following an approach that appears closer to Hausman's view rather than Blaug's. Of course the econometric modelling of macroeconomic time series continues, as do the debates about how this should best be done. However I would suggest that econometrics is having less and less direct influence on the macroeconomic models used for policy analysis, and instead provides a source of evidence that guides the future development of these models. In particular, results using VAR type models are increasing seen as means of summarising the data as a way of evaluating SDGE models, rather than as an alternative to SDGE models.[15]

Another way of looking at the methodological distinction I want to explore is in the importance attached to two criteria: internal and external consistency. By internal consistency I mean the consistency of all the elements of the model with its basic microeconomic assumptions. As we have already noted, rational expectations is internally consistent with models based on rational decision-making. Models that allowed unexploited arbitrage opportunities (where no barriers to such opportunities existed) would be inconsistent with profit maximising firms and utility maximising consumers. External consistency is consistency with data.

Ideally a model would be both internally and externally consistent. In reality, perfection is not possible, particularly in macroeconomics. I want to claim that the methodologoical approach that now characterises macroeconomics (and which has generated the consensus) holds that internal consistency should never be compromised. Under this view, a model that is internally inconsistent is simply incorrect (and should be rejected), while a model that is externally inconsistent can be tolerated, at least until a better model is found.

Let me give just two pieces of evidence that justify this claim. The first is Uncovered Interest Parity (UIP)[16]. UIP is almost universally used in current open economy macro. (International Risk Sharing – an even stronger proposition with even less empirical backing – implies UIP.) Its appeal is based on simple arbitrage considerations: with no barriers to the purchase of overseas currency assets, arbitrage implies UIP. However, the empirical evidence supporting UIP is at best mixed, and many would argue that it is clearly rejected by the data.[17] How

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[15] The difficulty with VARs from a policy maker's point of view is that their atheoretical nature makes story telling difficult. Structural Econometric Models (SEMs) can be seen as attempting a compromise between largely data driven VARs and theory driven SDGE models, but the problem with this compromise is that it fails to satisfy both econometricians and theorists. Wren-Lewis (2000) discusses these points further.

[16] Uncovered Interest Parity implies that differences between short term interest rates on different currencies are related to expectations about exchange rate movements between those currencies. In its simplest form the interest rate differential is equal to the expected change in the exchange rate. A more general formulation adds a risk premium to this equation, but the risk premium is normally assumed to be fairly constant over time.

[17] Messe and Rogoff (1988) is a classic reference on the empirical validity of UIP.

then do we account for its routine use in macro theory? The answer, I would suggest, is that UIP is internally consistent with most simple macromodels, even if its external consistency is seriously problematic.

The second piece of evidence comes from the structure of academic papers in macroeconomic theory. These papers are normally scrupulous in setting out the microfoundations for all the model's relationships, even if this means repeating the same derivations in article after article. One important reason why this is done is because it allows an easy check on internal consistency. To take a very simple example, we can easily check that the consumption function and labour supply function are the result of the same optimisation process. If, instead, the consumption function's specification (in paper X) was justified by reference to another paper (paper Y), and the derivation of the labour supply relationship was derived by reference to yet another paper (paper Z), then it would be much more difficult to check that the derivations in paper X were internally consistent. In addition, macromodels in the microfoundations mould often deal with primitive structures: economies made up of yeoman farmers, for example, rather than firms and workers. For models that are meant to apply to highly developed economies this seems strange. Yet it is again easier to ensure internal consistency in such systems: we do not need to worry whether firms are acting in the interests of their owners etc. Finally, modern papers hardly ever justify the components of their models by reference to empirical evidence.

As I have already suggested, one consequence of this change in methodology has been the macroeconomics has inherited the consensus associated with microeconomic theory. In the next section I want to explore another consequence, which may be more problematic. I will suggest that the development of macroeconomics may now depend critically on the speed at which theory evolves, and that this may be detrimental for policy. To do this, I will focus on the microfounding of Keynesian theory.

3. Internal Consistency and Keynesian Theory

In the days before the current consensus, there was a clear divide between New Classical/RBC theorists and Keynesian economists. The latter relied on some form of price stickiness, and the former argued that price rigidity was inconsistent with microfounded theory. For example, Mayer (1993), although deeply critical of the microfoundations project, writes (p115) 'Lucas's defence of the market clearing proposition is entirely appropriate for formal science economics. Here we need to explicate carefully and precisely our microfoundations, and the various theories of price inflexibility do not provide these. Hence, it is better simply to assume that prices move enough to clear markets…'

Nowadays such a view about price rigidity seems rather old fashioned. Microfounded models that incorporate some form of nominal inertia are now routine in the literature. Largely as a result, microfounded models have extended their influence into central banks. However, it is important to note how this change came about, both for what it shows us about microfoundations methodology, and for what it may imply for future model development.

Although we now have more evidence on price adjustment than we did twenty years ago, both at the individual firm and aggregate level, this does not seem to have played any significant role in the acceptance of nominal inertia as a component of microfounded models. We have always known that most firms changed prices infrequently, but why they do this remains unclear. Blinder's (1991, p89) comment that "Most economist would, I think, agree that we know next to nothing about which of several dozen theories of wage-price stickiness are valid and which are not" still remains true, if by valid we mean realistic.

The key development that led to the acceptance of nominal inertia within microfounded models appears to be theoretical. New Keynesian theory showed how apparently 'second order' menu costs (the costs of changing price lists) could lead to 'first order' effects on aggregate demand that might be sufficient to generate business cycles. A crucial element in this body of theory was interaction generated through imperfect competition, and interaction between goods and labour markets (see Ball and Romer (1990) for example). [18]

Only after these models were developed was it possible to refute the claim that nominal inertia was inconsistent with microfoundations. The importance of this theoretical development for the acceptance of nominal inertia makes sense if you accept a primary role for internal consistency in model development. Indeed, the point follows almost automatically, if microfounded models are required to be internally consistent. Nominal inertia could only be allowed into microfounded models once this New Keynesian theory had been developed. Empirical evidence might have provided a motivation for this theoretical development, but

[18] An alternative strand of this theory focuses on contracts. Once again, interaction under imperfect competition may be crucial in extending aggregate inertia beyond the contract length.

the theory needed to be established <u>before</u> microfounded models could include nominal inertia.[19]

This straightforward observation has an interesting implication. It means that microfounded models used for policy analysis can only develop as fast as theory allows. This is important, because theoretical development takes time. It has taken a couple of decades for Real Business Cycle models (which almost by definition exclude nominal inertia) to evolve into Stochastic Dynamic General Equilibrium models (which allow nominal inertia). For those working in the microfoundations methodology, that means two decades before they can adequately explore the operation of monetary stabilisation policy.

In practice this didn't matter too much for policy makers, because it is only recently that the central models in places like the Bank of England have been explicitly microfounded. Indeed, it could be argued that it is precisely because microfounded models can now be Keynesian that has allowed them to play such a central role in policy making institutions. However, now that microfounded models are a key part of policy making, it follows that the development of these models will be governed by the speed of theoretical development, which as the nominal inertia example shows may be rather slow.

Let me give two examples where this might be important. The first concerns inflation inertia, which is the close relation to nominal inertia. A standard New Keynesian approach uses Calvo contracts: the assumption that a firm's price will only change next period with some fixed probability (see Calvo, 1983). This assumption appears to mimic more detailed models that formally incorporate menu costs. (I say 'appears to' here, for reasons that will become clear below.) However, Calvo contracts imply a Phillips curve where current inflation only depends on future expected inflation, and not on lagged inflation. This is turn implies a number of macroeconomic properties, including costless disinflation.

Although the empirical evidence is (as usual) mixed, it does suggest for most economies that the Phillips curve should include some impact from lagged inflation. This is termed 'inflation inertia'. However, as yet there is no clear microfoundation for this effect. How, then, should policy makers proceed? Should they continue to use microfounded models that ignore inflation inertia, until the microfoundations for this effect become clear. Or should they investigate the implications of inflation inertia now, using non-microfounded models?

A second example concerns social welfare. As I noted earlier, recently Woodford has shown how the objectives of benevolent policy makers can be derived from the utility of the representative agent. Furthermore, Woodford showed that if we assume Calvo contracts, then these objectives take a familiar form, which is to minimise quadratic terms in the output gap and inflation. In the past the trade-off between these two objectives was always thought to be a policy choice, but Woodford showed how the relative importance of inflation and the output gap could be derived from the preferences of agents combined with key model parameters in the microfounded approach. In particular, for fairly

[19] Some might argue that motivation also came from the relative difficulty that RBC models had in replicating business cycle data: see McCallum (2000) for example.

standard calibrations, inflation appeared to be considerably more important than the output gap.

The influence of Woodford's analysis has been dramatic – it is now standard for microfounded models in the literature to derive social welfare in this way when doing policy analysis. These social welfare functions tend to share the characteristic that inflation dominates the output gap in importance. However this may not be too surprising, given that nearly all microfounded models ignore unemployment. The reason for this is straightforward: unemployment requires heterogeneity across agents, and this is rather difficult to model. It seems reasonable to conjecture that, in time, microfounded models will be developed that can adequately capture unemployment. We can also conjecture that microfounded measures of welfare derived from such models may raise the importance of the output gap relative to inflation[20]. However, until this theoretical work is done, policy analysis using purely microfounded models will imply inflation concerns are dominant.

Is it either inevitable or desirable that the pace of theoretical development will govern the tools that policy makers use under the microfoundations consensus? The answer depends on whether most macroeconomists turn out to be what can be described as 'microfoundations purists' or 'microfoundations pragmatists'. In exploring this, I will also suggest that the purists' case is not quite so clear as they might believe.

[20] Validating this conjecture may require recognition of the utility costs of exclusion from the workplace. Evidence on happiness suggests that increases in unemployment are more damaging than increases in inflation, even for those who do not become unemployed: see Layard (2003)

4. Microfoundations purists and pragmatists

I have already discussed the issue of inflation inertia: a potentially important aspect of the inflationary process that seems significant empirically but which has, for the moment, no clear microfoundation. A few years ago I was at a conference in which a couple of papers looked at the implications of inflation inertia in otherwise microfounded models. This provoked some debate amongst the participants, with at least some suggesting that this was not a legitimate thing for microfounded analysis to do.

The argument against including inflation inertia in microfounded models, which we might describe as the 'microfoundation purist' position, was that including non-microfounded elements in otherwise microfounded models meant that the model as a whole was not microfounded, because there was no way of establishing its internal consistency. Until a theory for inflation inertia was developed, we had no way of establishing whether its presence in the model was consistent with optimising agents, or with the other relationships in the model.

If we define microfounded models in the way I have suggested (which is that they must be internally consistent), then this proposition is true by definition. So is this a purely semantic debate about the label 'microfounded'? In practice there are more substantive issues involved. Microfoundations purists would argue that macromodels analysed in the top academic journals should always be (100%) microfounded. By implication, until a theory explaining inflation inertia is developed, macromodels analysed in these journals should not contain inflation inertia, and so the policy implications of nominal inertia would not be explored by this literature.

'Microfoundation pragmatists' might counter that it is important that policy makers explore the implications of inflation inertia, because inflation inertia might be both important empirically, and a theoretical rationale for inflation inertia might emerge in the years to come which was reasonably consistent with standard microfounded modelling. In other words, while models containing inflation inertia could not be demonstrated as being internal consistent today, in the future they might turn out to be. They might note in support that originally Keynesian models appeared to be internally inconsistent with microfoundations, but today we know better.

The purist might retort that they had no objection to exploring the implications of nominal inertia in this way – just do so outside the microfoundations framework. Purists sometimes describe non-microfounded models as 'policy models'. The pragmatist could respond that this would banish such analysis from the mainstream academic literature, which would be unfortunate. It would also be a mistake to assume that policy makers had access to sufficient modelling possibilities to do this analysis themselves.

Both groups appear active in macroeconomics at the moment. To pursue the inflation inertia example, one or two papers have appeared in the top journals that have explored the implications of this inertia for policy (see Steinsson (2003) for example). On the other hand, the dismissive referee's comment that some element of a paper 'lacks clear microfoundations' remains commonplace, and is often enough to lead to rejection by an editor.[21]

One of the attractions of the microfoundations purist case is that their methodological position appears clear. The internal consistency of the model is sacrosanct. However, I want to suggest that this is partly a delusion. In an important sense today's microfounded models have already abandoned internal consistency, at least in its simple form. To see this, we need to return to New Keynesian theory, which allowed nominal inertia into microfounded models.

As I have already noted, one of the most frequently used devices for introducing nominal inertia into microfounded models is Calvo contracts. It was Calvo contracts that enabled Woodford to derive a traditional social welfare function from consumer's utility, and thereby complete the microfoundations process, along the lines noted above. Calvo contracts assume that firms have a fixed probability of changing prices each period.

At first sight, Calvo contracts appear to be inconsistent with profit maximising firms. A firm that optimised would not choose to change prices with a fixed probability. Instead, the probability of changing prices would depend on the state of the firm in any particular period: for example, had costs or demand just increased or were they stable. A firm that ignored such considerations when changing prices would seem to be ignoring opportunities to maximise profits.

However, Calvo contracts are not meant to be taken literally when used in microfounded models. Instead, they are used because they appear to mimic more complicated models based on menu costs. Menu costs are fixed costs in changing prices (e.g. the cost of reprinting price lists), and they are clearly a feature of the real world. They are one reason why firms do not change prices by small amounts every day. So why not explicitly include menu costs in every microfounded model exploring nominal inertia, rather than Calvo contracts?

The answer is simply tractability. While it is possible to explore some of the implications of menu costs in simple New Keynesian models, to embody them in more complex models exploring other issues is too difficult.[22] In contrast, Calvo contracts are much easier to work with. Calvo contracts therefore represent a shortcut, with an associated claim that they work 'as if' firms optimised in the presence of menu costs.[23]

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[21] Ironically, I think microeconomists are much less likely to take a purist position, as the current interest in behavioural economics illustrates.

[22] The basic difficulty is that menu costs introduce a simple non-linearity: prices either change or remain fixed, depending on the size of menu costs relative to the cost of being away from the profit maximising price. In this situation, assuming all firms are identical would produce implausible discrete changes in aggregate behaviour. However, introducing heterogeneity alongside non-linearity gets very complicated.

[23] The tractability of Calvo contracts is a necessary, but not sufficient, condition for their use in microfounded models. The claim that they mimic the behaviour of optimising firms facing menu costs is also crucial. This is illustrated by another highly tractable way of incorporating nominal inertia into profit maximising behaviour, which is to assume quadratic costs in changing prices, as Rotemberg (1982) showed. Unfortunately, while fixed costs in changing prices clearly exist (i.e. menu costs), there appears to be no obvious reason for quadratic costs. Once a firm decides to change its prices, there is no apparent reason why it should go to the profit maximisation price in steps. Of course Rotemberg recognised this, and he tried to tell an 'as if' story involving customer markets, but the link between this and quadratic costs did

In one sense this claim, that Calvo contracts are a reliable short cut to a (more theoretically attractive but intractable) model with menu costs, stays within the methodological tradition that stresses internal consistency. The claim is validated by examples that are theoretical: simple models where the correspondence holds. We could describe this as an 'indirect internal consistency' claim: a model using Calvo contracts claims internal consistency by reference to other theoretical models. No reference to external consistency is involved.

However, there is a crucial difference between direct and indirect claims to internal consistency. We cannot know for sure whether the indirect internal consistency claim is correct. Calvo contracts may mimic the implications of menu costs in some models, but we do not know that this survives in every model where Calvo contracts are employed. We cannot know for sure precisely because such models are intractable. Instead, the claim has to be an informed guess, and we may never know its truth.

The indirect internal consistency claim for Calvo contracts therefore works as follows:

(a) Calvo contracts appear to mimic the implications of menu costs in very simple models

(b) It is intractable to include menu costs in more complex or general macromodels

(c) We judge that the property of Calvo contracts mimicking the implications of menu costs will remain in these more complex models, so we use Calvo contracts as a shortcut for menu costs in these models.

Now I happen to think that, in most cases, this is a reasonable judgement to make. But crucially, it is a judgement – it cannot be formally demonstrated in the way that direct internal consistency claims can be. As a result, we cannot claim to know that a microfounded model that includes Calvo contracts is internally consistent. Instead, we can only judge that it probably is. As a result, adopting Calvo contracts has already seriously compromised the position of the microfoundations purist.

The microfoundations pragmatist can assert to following claim:

(d) That in time, a theoretical rationale will be found for inflation inertia, that is consistent with the kind of optimising behaviour in current microfounded models.

This too is an informed judgement. The microfoundations purist position implies that we are somehow on much firmer ground with (c) than we are with (d). It is not obvious to me why this is the case. Seen in this light, arguments that models embodying inflation inertia compromise the microfoundations project seem to

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not appear theoretical convincing. As a consequence, the quadratic cost formulation is rarely used, and instead Calvo contracts have become the standard tool.

have less force. Such arguments rely on treating internal consistency as sacrosanct, yet internal consistency has already been compromised in most models that include some form of nominal inertia.

I have used inflation inertia here as a convenient, if also important, example. However, I suspect the issue is much more general, and is likely to become increasingly important. Other examples of ‘shortcuts’ that appeal to indirect internal consistency claims exist, such as adding money into the utility function (which stands in for more elaborate, yet intractable, cash in advance type analysis). As microfounded macromodels try and embrace more of the complexities of the real world (like unemployment), the need for shortcuts like this will surely increase, and so the debate about the methodological purity of microfounded macromodels may intensify.

5. Conclusions

In this paper I have argued that the current consensus in macroeconomics is largely an outcome of the gradual success of the microfoundations project. The microfoundations project has been a success in two senses. First, most top-level theoretical analysis in macroeconomics is now undertaken with models where relationships are explicitly derived from microeconomic theory. This includes both the Keynesian analysis of the business cycle, and most recently an analysis of the objectives of benevolent policy makers. Second, microfounded macromodels are in many cases now being used as the principle means of advising policy makers, such as with the central forecasting model at the Bank of England.

As a result of the microfoundations project, macroeconomics has inherited the consensus associated with microeconomic analysis. It is no longer the case that we have alternative 'schools of thought' in macro with their own, apparently distinct models. Instead, different views within the mainstream share the same common, microfounded analysis. I have argued that the microfoundations project has also changed the way macroeconomics is done, with a much greater emphasis on the internal consistency of models and greater tolerance of external inconsistency (inconsistency with the data).

There is a danger that this consensus has been achieved in part by narrowing the range of phenomenon macromodels can address. Features of real economies may not be incorporated into models because their rationale in terms of microeconomic theory has yet to be established. Taken literally, the microfoundations methodology implies that the pace of development of macromodels is governed by the speed of theoretical innovation, rather than empirical discovery.

This appears to be the position of those I have termed 'microfoundations purists', who assert that all models analysed in the better academic journals should be fully microfounded. I contrast, the 'microfoundations pragmatists' would allow elements with empirical backing but no clear theoretical rationale to be included in otherwise microfounded models, on the basis that a theoretical rationale for these elements may emerge in the future. I have argued that the methodological position of the purists has already been compromised by the use of analytical shortcuts, such as Calvo contracts. However, how the debate between purists and pragmatists will go is far from clear. It is a debate that those macroeconomists advising policy makers should follow, and perhaps even influence.

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