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**INTERNAL CONSISTENCY, NOMINAL INERTIA AND THE
MICROFOUNDATION OF MACROECONOMICS**

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Internal Consistency, Nominal Inertia and the Microfoundation of Macroeconomics

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Abstract

Microfounded macromodels (SDGE models) represent the dominant approach in academic macroeconomics, and their influence now extends to the forecasting models of central banks. These models appear to adopt a clear methodological approach, which promotes internal consistency above external consistency as a necessary condition of admissibility. This paper develops two arguments. The first notes that internal consistency means that the development of microfounded macromodels depends on the pace of theoretical innovation. The delay in the acceptance of nominal inertia as part of microfounded modelling illustrates the importance of this constraint, and similar delays are already apparent in other areas. This has led to an internal debate between 'pragmatists' who argue for limited departures from internal consistency, and 'purists' who claim that this would fatally compromise the methodological integrity of microfounded macroeconomics. The second argument is directly relevant to this debate. It is that the inclusion of nominal inertia into these models via short-cuts like Calvo contracts has required a key modification of the microfoundations methodology, such that internal consistency can only be claimed indirectly by appeal to theory developed elsewhere. This modification is significant because these indirect internal consistency claims cannot be demonstrated formally, and instead depend on judgement. It seems implausible that this judgement is not influenced by external consistency. As a result, the methodological integrity of the microfoundations project is not as unblemished as the 'purists' imagine.

0. Introduction

The importance of ‘microfoundations’ to the recent development of macroeconomics cannot be overemphasised. As Hoover (2001, p72) writes ‘The conviction that macroeconomics must possess microfoundations has changed the face of the discipline in the last quarter century.’ This goes well beyond the use of microeconomic tools in producing theory. Modern macroeconomic research largely involves building and simulating Stochastic Dynamic General Equilibrium (SDGE) models, which are based on the original Real Business Cycle (RBC) tradition. These are parameterised (normally calibrated) models that provide quantitative outputs. Their protagonists typically claim that they are superior tools for quantitative analysis than those provided by alternative, more traditional approaches. Almost without exception, macroeconomic papers in the top economics journals now involve SDGE models. As we note below, even large forecasting models maintained by policy makers, which had been regarded as more empirically orientated and more eclectic in their use of theory, are now embracing the microfoundations project.

In the first section of this paper I argue that microfounded macromodelling represents a methodological position that is clearly distinct to other approaches in macroeconomics. In particular, microfounded macromodelling elevates internal consistency to a necessary condition of admissibility. In contrast, external consistency, in the sense of conformability with empirical evidence, is desirable and may motivate model development, but it is not essential. As a result, the methodological position that Hausman in particular ascribes to core microeconomic analysis (e.g. Hausman, 1992) can now equally be applied to macroeconomic theory.

In the second part of this paper I shall use the example of nominal inertia among others to examine how microfounded macromodelling progresses, and in particular how these models respond to empirical evidence. In academic papers, this often involves ‘puzzle resolution’. Theory is developed, augmented or adapted to account for some otherwise inexplicable (in terms of the existing microfounded literature) stylised fact. As microfounded macromodels have to be internally consistent, this means that the direction and speed of model development depends not only on the empirical validity of existing models, but also on the pace of theoretical innovation and the tractability of alternative approaches.

Given the inevitably slow pace of theoretical innovation in some ‘difficult’ areas, this has led some working in the microfoundations mainstream (we will call them ‘pragmatists’) to include in their models elements that are empirically grounded but which, as yet, have no clear microfoundation. This has proved controversial. It has led others (we will call them ‘purists’) to suggest that any such incorporation of non-microfounded elements cuts across the methodological principles of microfounded macroeconomics, and so should not be a part of the mainstream modelling endeavour.

In the third section of this paper I want to argue that as the microfoundations project has developed and become dominant, a key compromise has already taken place, and this is most evident in the case of nominal inertia. Whereas twenty or thirty years ago it was thought that assuming price rigidity was inconsistent with microfounded macromodelling,

now these models routinely incorporate some form of nominal inertia. What has altered over this time span is the development of New Keynesian theory, which showed in particular how menu costs can give rise to Keynesian business cycles in the context of rational expectations and profit maximisation. Crucially, however, menu costs are not formally incorporated into most microfounded macromodels that embody nominal inertia. Instead, models typically use short cuts, which it is argued act as *if* the model contained menu costs. I will argue that this *internal consistency by indirect reference* is crucially distinct from a methodological viewpoint. As the validity of the indirect theoretical references cannot be shown formally, whether they are admissible inevitably becomes a matter of subjective judgement rather than proof. In particular, it seems unlikely that this judgement will be independent of empirical evidence. As a result, the methodological integrity of the microfoundations project is not quite as clear cut as the 'purists' imagine.

A final section concludes.

1. The central role of internal consistency

Two recent developments exemplify the dominant position of microfounded macromodelling in macroeconomics today. The first is Woodford's derivation of the objectives of a benevolent monetary policy maker from the utility of the representative agent. The second is the adoption by key central banks of forecasting models that embody a SDGE approach. Both developments will be important in the arguments developed below.

Until recently, a major lacuna in Keynesian business cycle analysis from the microfoundations perspective had been the lack of any relationship between the objectives adopted by benevolent policy makers, and the utility of representative agents. Policy makers were generally assumed to minimise quadratic terms in output and inflation, and the relative importance of these two terms was described as a 'choice for policy makers'. The objective function of policy makers was not formally linked to agents' utility, in part because the latter appeared unaffected by inflation.

Rotemberg and Woodford (1997) showed how, in a model where Calvo contracts represent nominal rigidities, an objective function for policy makers of this form could be derived from the representative agent's utility function. Utility did depend on inflation, because with Calvo contracts inflation changed relative prices in a distortionary manner. In addition, the importance of output relative to inflation would depend on the parameters of this utility function and the rest of the model. These points are discussed in detail in Woodford (2003).

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 to 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. 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.

It was thought until recently that the microfounded macromodelling approach could not be adopted for forecasting models, because these models needed to track the data closely, and SDGE models traditionally eschewed econometric estimation. Instead, forecasting and the policy analysis that followed from it would require either atheoretical statistical models, like VARs, or more probably structural econometric models (SEMs) whose structure was determined using an eclectic mix of theory and empirical evidence. (The idea that there might be a tension from following data or theory in model building is very familiar to economists, but is clearly rather different from the ideal in the physical sciences that data and theory should always be consistent.)

Recent developments at major central banks suggest that microfounded SDGE modelling can now form the basis of forecasting models. In contrast, econometric studies based on VARs are increasingly used as ways of organising data for comparison with SDGE simulations, as in Christiano, Eichenbaum and Evans (2005) for example, while more traditional

SEMs have become unfashionable (see Wren-Lewis, 2000). A Bayesian estimation approach to parameterising a SDGE model has been adopted by the European Central Bank (see Smets and Wouters, 2003). However, estimation is not allowed to modify the theoretical structure and dynamics of the model. Instead, data dynamics not captured by the theory is ascribed to the development of 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. Conflicts between the dynamics implied by theory and those apparent in the data are not resolved, but marginalised as features of unexplained shocks.

The importance of internal consistency is even more transparent in the approach taken by the Bank of England. At the centre of the Bank's new forecasting model (see Harrison et al (2005)) is a 'core' model that is essentially an elaborate Stochastic Dynamic General Equilibrium (SDGE) model. Inconsistency with the data is handled by additional 'non-core' equations, which relate variables from the model's core to the data. Non-core equations would typically involve lags (in the dependent or core variable), although in principle they could involve other variables as well.

Critically, data inconsistency is not allowed to 'infect' the core, but instead determines the structure of the non-core equations. Crucially, the non-core equations are completely recursive to the SDGE core: there is no feedback from non-core variables to the core. In a more traditional SEM, 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.

This recursive structure reflects a central aspect of microfounded macromodelling, which is internal consistency. The equations of a SDGE model, such as the core Bank of England model, must be derived from microeconomic theory in an internally consistent manner. For example, the consumption function and the labour supply function must be derived from a common optimisation problem, where the agent maximises utility that involves both consumption goods and leisure. The danger of incorporating additional lags in, say, a labour supply function, is not just that such lags may not be microfounded, but that this function may then be inconsistent with the consumption function in the model.

In both these cases, our account has stressed why the need for internal consistency is crucial in understanding what has gone on. The importance of internal consistency can also help explain other, more longstanding, features of microfounded macroeconomics. For example, a near universal characteristic of academic papers that present models in the microfounded modelling class is that each new model will derive its aggregate equations from an analysis of the optimising decisions of individual (usually representative) agents in a formal and explicit way. In most cases, this will involve repeating derivations that have been presented many times already in the literature. For example, the derivation of an Euler equation for aggregate consumption from the optimising behaviour of a representative, infinitely lived consumer (where aggregate consumption at time t depends on consumption

at $t+1$, plus a term in the real interest rate) probably appears in nearly every paper in the microfoundations tradition.

Would it not save a lot of journal pages to instead justify aggregate relationships by reference to derivations in early studies, when such derivations have become standard? The reason why this is not normally done is that it would become difficult to check for internal consistency. If, in paper Z, the derivation of the consumption function was outlined in paper X, and the derivation of labour supply in paper Y, we would have to read X and Y to establish whether these derivations were internally consistent in Z. By outlining the complete microfoundations in each paper, then internal consistency can be checked directly.¹

Internal consistency emphasises that microfoundations relate not just to the derivation of individual equations, but to how those derivations interrelate. It applies a microeconomic view (optimisation by rational agents subject to a particular set of constraints) consistently across the agents in the model. In the view of some, non-microfounded models are problematic not so much because they lack elements of microeconomic theory, but because these microeconomic elements are not consistently and rigorously applied. For example, Obstfeld and Rogoff (1996), in the preface to their highly acclaimed macroeconomics textbook, argue that a key problem with the pre-microfoundations literature is that it 'lacks the microfoundations needed for internal consistency'.

The importance of establishing internal consistency may also explain another feature of microfounded macromodelling that at first appears odd. Microfounded models tend to work with institutional set ups that are primitive, in the sense of involving as few types of actors or institutions as possible. They tend to be populated by yeoman farmers as much as firms, for example. This type of reductionism avoids inconsistency: the yeoman farmer's production and consumption decision are made jointly, and we do not need to worry about who owns the firm, and whether the firm is acting in the interest of its owners. The fact that the models may, as a result, appear extremely remote from modern day economies is judged to be much less important than the ability to ensure a consistent microeconomic foundation.

The relationship between microfounded models and *external* consistency is discussed in detail in subsequent sections. For the moment we can simply note that there is no *necessary* requirement that aggregate relationships match econometric evidence. Indeed, empirical evidence is hardly ever mentioned in the derivation of equations in the microfounded macromodelling approach. Relationships that appear at odds with the data are tolerated if their microeconomic provenance is clear. (Perhaps the most widely used example would be Uncovered Interest Parity.) As we argue later,

¹ The alternative of justifying aggregate relationships by reference is standard in a class of models that were particular popular in the 1970s and 1980s, and which today is often referred to as 'policy modelling'. An early example of models of this type was the literature that examined money supply targeting following the work of Blinder and Solow (1971) and Christ (1979). Papers that present policy models start with aggregate relationships rather than microfoundations. These aggregate relationships will generally be justified by reference, where the literature referred to involves theoretical studies *and* econometric research. One advantage of setting out derivations by reference is that it is easy to note when a particular aggregate relationship may be derivable from more than one microeconomic approach. This is hardly ever done when presenting microfounded models.

empirical evidence, often in the form of ‘puzzles’, can provide the motivation for theoretical development. The key difference is that this link between external evidence and the model is selective: there is no attempt to check whether the model is consistent with all relevant empirical evidence. Thus, while a new real business cycle model will often be said to fit the facts about the second moments of and cross correlations between aggregate time series better than previous RBC models, there is no requirement that the model’s consumption function (or any other behavioural relationship) tracks data on consumption in a conventional, econometric sense.

A clear example of this ‘imbalance’ between internal and external consistency in microfounded macromodelling is provided by rational expectations. Lucas (1987, p 14n) described rational expectations as a ‘consistency axiom’. The idea is that rational expectations simply extends the rationality postulate used elsewhere, to the processing of data and the formation of expectations. Not to assume rational expectations in a model which has rational consumers or firms violates internally consistency. As a result, rational expectations now form a key part of virtually all microfounded macromodels, whatever the empirical evidence on the matter might suggest. (An exception might be models based on learning, although these are still in their infancy.)

This distinction between microfounded macromodelling and alternatives such as policy models or SEMs is close to the contrast Mayer (1993) makes between ‘formalist’ and ‘empirical science’ modelling approaches. (Weintraub (1979) is a classic, earlier discussion of similar themes.) Mayer discusses in great detail the relative merits of the two approaches. His distinction relates to alternative methodological approaches in economics. Microfounded macromodelling, with its emphasis on derivation from microeconomic fundamentals, sits easily with the ‘deductivist’ approach suggested by Hausman (1992) among others, whereas the more frequent references to empirical evidence in policy models or SEMs could be argued to follow an approach advocated by Blaug (1980).²

I suspect many young macroeconomists working in the microfoundations tradition would see the distinction between microfounded macromodelling and policy models or SEMs not as methodological, but as simply the difference between the old and the new. In this view microfounded macromodels do what other modelling approaches do, but better. However, the idea that microfounded macromodelling encompasses these other approaches – because the former adds explicit microfoundations without necessarily subtracting anything – is not tenable in practice. Microfounded macromodels rarely discusses the correspondence between aggregate relationships and econometric evidence. In addition it may be quite possible that empirically valid aggregate macroeconomic relationships exist that cannot be justified using the normal microfoundations approach: aggregation problems are typically cited here (e.g. Hoover (1988), Kirman (1992), Janssen

² Because the former approach emphasises theoretical deduction from axioms about human behaviour, and the latter consistency with macroeconomic data, it is tempting to describe the two approaches as ‘theoretical’ and ‘empirical’ respectively. This would be a mistake: for example the axioms on which microeconomic theory is based are essentially empirical, and microfounded models are often parameterised using microeconomic empirical evidence or calibrated using data on macroeconomic aggregates.

(2006)). The reductionist approach may not in practice be the most useful: Hendry in Backhouse and Salanti (2000) makes an interesting analogy with hydrology, while Van den Bergh and Gowdy (2003) draw analogies with biology. Thus while microfoundations may represent the 'new', and alternative approaches the 'old', it cannot be argued that the new simply adds good theory to the old. The focus of the microfoundations project on internal rather than external consistency represents a distinctive methodological approach.

The extent to which the alternatives to microfounded macromodelling also embody a consistent methodological approach is not my concern here. Nor do I want to attempt to outline the pros and cons of these different approaches. Instead I want to emphasise that microfounded macromodelling *appears* to embody a clear methodology, which requires internal consistency as a necessary condition, such that equations or exogenisations that appear inconsistent are not tolerated. However, I will argue in section 3 below that the success of microfounded macromodelling has only been achieved by modifying this methodology in an important, but rarely acknowledged, way. Before this, I need to look at what an emphasis on theoretical consistency implies for the pace of development of these models.

2. How microfounded macromodels progress?

The previous section examined how microfounded macromodelling papers go to considerable efforts to ensure their models based on microeconomic theory are internally consistent. Complete derivations of aggregate relationships are always provided directly, rather than by reference. The models tend to deal with primitive concepts, such as the yeoman farmer, which facilitate establishing this consistency. In the context of a microfounded forecasting model, it can involve distinguishing between an internally consistent core, and a recursive non-core. In this section I look at what this implies about the speed at which these models adapt to the data.

Academic papers involving microfounded macromodelling are often of the 'puzzle resolution' variety. A paper is motivated by presenting empirical evidence, sometimes in the form of a stylised fact, which appears inconsistent with existing models. A model is then put forward that adapts models in the existing literature in such a way as to 'explain' (be consistent with) this evidence. Ideally the model should also remain consistent with other empirical evidence, but there is generally no formal way of establishing this. As such, we cannot be sure that the theoretical development 'encompasses' existing work.

We can return to the example of a forecasting model with a SDGE core and an 'empirical' non-core, because this framework – although somewhat specialised – exemplifies the process of microfounded model development. Here the non-core equations represent the motivation for model development. Suppose, for example, that we find that data for aggregate consumption is related to core consumption from the model, but there are significant lags between changes in core consumption and actual consumption. This 'puzzle' might provide motivation for developing a microfounded model of habit persistence, and incorporating it into the core.

Developing such a microfounded theory goes far beyond adding lags into a consumption function. Not only do we need to provide a plausible rationale for lags based on the basic building blocks of utility and constraints, but we need to investigate what implications this might have for other parts of the model, such as labour supply, wage setting, or even shareholder valuation. Developing such a theory takes time. In the meanwhile, the core part of the model would remain as before (i.e. it would be inconsistent with the evidence), and the data inconsistency would be 'handled' by the ad hoc non-core equations.

Thus external consistency, or more precisely the lack of it, provides a key driving force behind theoretical development in microfounded macromodelling.³ However, the speed at which that theoretical development takes place, and the direction it takes, is likely to depend on other factors besides external evidence. In particular, the tractability of different approaches will be crucial. Resources for theoretical development (theoretical macroeconomists) are scarce, and there are incentives to pursue approaches that offer short term returns. As a result, the extent of external inconsistency is

³ It is not the only driving force. Theoretical innovation may occur simply through the study of theory: rational expectations is probably a case in point, and the development of social welfare functions from utility discussed in the previous section would be another.

not the only factor influencing the priority given to different avenues of model development.

A key implication for microfounded macromodelling is that the pace and direction of model development will be governed by the speed of theoretical innovation. This follows directly from the overriding importance of internal consistency, yet its implications are often overlooked. This may be because we are tempted to think of microfounded macromodelling as equivalent to theoretical analysis. However, the ambition, scope and practice of microfounded macromodelling is not just to provide the theoretical foundations of model development, but also to produce parameterised models that will provide quantitative results that can directly guide policy. As such, microfounded macromodelling is complete, and not just the theoretical side of some joint endeavour. Many macroeconomists claim that microfounded macromodelling is the superior method of quantitative analysis and provision of policy advice. Any evaluation of this claim should take into account the constraint that theoretical innovation places on the development of microfounded macromodels.⁴

A comparison between microfounded macromodels and structural econometric models (SEMs) clarifies this point. Take the hypothetical finding that consumption adjusts gradually towards the level implied by conventional theories based on intertemporal optimisation. We have already noted that before this finding can be incorporated into a microfounded macromodel (or the core of a microfounded forecasting model), we need to develop a theoretical explanation for it, such as a theory based on habit persistence, and explore the implications of that theory for all aspects of the model. In contrast, in a SEM the finding can be immediately incorporated, by simply adding lags into the model's consumption function. This is because a SEM does not require internal consistency as a necessary condition for inclusion.

While a SEM allows rapid adaptation of a model to the data, the cost is that we lack a microfounded story of why these lags occur, and as a *result* we may be failing to note changes that are also required in other parts of the model, but whose empirical inadequacy is less evident. The model may be internally inconsistent. If modelling was only of academic interest then the arguments for waiting until the microfoundations were clear and completely worked through might appear overwhelming. However, policy decisions are being made on the basis of these models all the time, so which procedure should be pursued becomes a much more difficult second best problem.

The story of how nominal inertia became acceptable in microfounded macromodelling, which is discussed in more detail below, illustrates that this point about the pace of innovation may be far from trivial. Arguably it took 20 years for New Keynesian theory to establish that nominal rigidities could be microfounded. If policy makers had been dependent on microfounded

⁴ Take, for example, the lack of analysis of the impact of financial sector imperfections for the macroeconomy that has been revealed by the credit crunch. It is possible to argue, although this would go well beyond the scope of the present paper, that a key factor here has not been a lack of recognition of their potential importance (see Greenwald and Stiglitz, 1983, for example), but the technical difficulty involved in modelling these links in a microfounded manner.

macromodelling as their main source for policy analysis over that period, then this delay would have been a serious handicap.⁵

In this context, it is tempting for at least some of those involved in microfounded macromodelling to allow for some limited departure from internal consistency. We can label this the 'microfoundation pragmatist' position. The microfoundation pragmatist still works with models that are in most respects microfounded, but they are willing to allow one or two departures from internal consistency where the empirical evidence for these departures appears strong.

At the moment one area where this appears to be happening involves 'inflation inertia'. Inflation inertia is closely related to, and builds upon, nominal inertia. Although introducing Calvo contracts allows the possibility of economic fluctuations (business cycles) of a Keynesian type, and therefore the analysis of monetary policy based on demand management practiced by most central banks, in some cases it makes the job of inflation control too easy. In particular, as Ball (1994) and others have pointed out, it allows costless disinflation: a credible central bank can permanently lower the rate of inflation with little or no cost in terms of lost output. The reason for this is that Calvo contracts imply a 'New Keynesian' Phillips curve, which in its simplest form relates current inflation to next periods expected inflation and the current 'output gap'. If the central bank announces a lower inflation target, and this is credible, inflation expectations will fall, and this will reduce current inflation without the need for any change in output.

In the real world disinflation does not appear so easy. Inflation inertia adds a term in lagged inflation into the New Keynesian Phillips curve, so that current inflation depends on both past inflation and expected inflation. The lagged inflation term makes disinflation costly, and there is empirical support for such a term. However such a modification does not appear to be implied by menu costs, and as yet there is no widely accepted microeconomic theory justifying inflation inertia. Derivations of inflation inertia generally postulate a group of 'rule of thumb' price setters, who when they change price simply index to past average inflation rather than maximise profits.

Such behaviour is on the face of it inconsistent with optimising agents. Yet some authors - who work in the macroeconomics mainstream - have nevertheless investigated incorporating inflation inertia into microfounded macromodels. To take one example, Steinsson (2003) investigates the impact that inflation inertia would have on optimal monetary policy, where the monetary authority is benevolent in the sense that it maximises an objective function that is derived from agents' utility. Everything, including the monetary authority's objectives, is carefully microfounded, with the exception of inflation inertia. The paper explicitly states that these rule of thumb price setters depart from 'full rationality'. As a result, their postulated behaviour is entirely ad hoc.

⁵ In terms of a forecasting model of the type discussed earlier, the delay would have meant that price adjustment was instantaneous in the core, and all rigidities were handled by lags in non-core equations. While this means that rigidities are not completely ignored by the full model, the recursive structure means that there is no feedback from these rigidities. For example, without feedback lags in wage adjustment would have no impact on labour demand. Once again, a SEM would automatically allow for such feedback, but not necessarily in a microfounded way.

Yet this development worries others within the microfounded macromodelling community. At a conference where another paper explored the consequences of inflation inertia in a SDGE model, one of the panel whose discussion closed the conference, and who themselves had used Calvo contracts, complained that analysing inflation inertia was “going too far: microfoundations had to be respected”. This was a specific example of a much-used criticism of potential macroeconomics papers, which is that the proposed model ‘lacks clear microfoundations’. Such a criticism can almost ensure that a paper does not get published. Let us label macroeconomists who hold such views ‘purists’. The microfoundation purist believes in working with models that are internally consistent in their totality.

First we need to dispose of a trivial point. It would, of course, be possible to simply preserve the label ‘microfounded model’ for one that was internally consistent in every respect. Models that incorporated inflation inertia could simply be given another label. But the key point is the fact that models in the better academic journals, and now core forecasting models in some policy institutions, are fully microfounded, and the claim by many that this should be so. The concern expressed at the conference noted above 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. To use the inflation inertia example, papers analysing inflation inertia should only be discussed in (the better) academic circles after the microfoundations for such behaviour have been worked out.

It is possible to view this debate between ‘purist’ and ‘pragmatist’ as part of a more wide ranging discussion about the merits or otherwise of modelling apparently non-rational behaviour (as proposed by Akerlof, 2007, for example), but it appears more general than this. The problem may well not involve non-rationality *ex post*. Inflation inertia appears inconsistent with rational, profit maximising behaviour *at present*, just as nominal inertia originally appeared to be similarly inconsistent. However, it seems quite possible that theory may be developed in the future that shows how inflation inertia may also be consistent with profit maximising behaviour. To give one example, suggested by Woodford (2003), costs in obtaining information about elements of the demand curve may make it optimal on occasion to index rather than profit maximise.⁶ Thus, models that incorporate inflation inertia may turn out in time to be internally consistent, even though they appear not to be today given the current limitations of theory.

Why should ‘microfoundations pragmatists’ want to abandon internal consistency? One obvious reason is that it is important to examine how policy might need to change if inflation inertia turns out to be an important feature of real economies. In fact, Steinsson (2003) shows how the derived social welfare function may significantly change when inflation inertia is present. The ‘microfoundations purist’ might retort that they have no problem with policy makers looking at robustness in this way, but this should not be considered

⁶ Indexation increases profits beyond menu costs (so prices change), but the additional benefits of calculating the profit maximising price are not thought to exceed the costs in calculating that price.

part of the microfounded macromodelling project. However this appears to presuppose a dichotomy between academic discourse and policy analysis that may be neither feasible (given limited resources) nor desirable.

Given the experience with nominal inertia, the impatience to analyse the implications of inflation inertia before its microfoundations are worked out (or not) seems natural. But the 'purists' appear to have a strong case. As we have argued earlier, a key methodological feature of microfounded models is that internal consistency, rather than external consistency, becomes the criteria for model admissibility. The moment we begin to allow some departures from internal consistency based on empirical realism (in the macroeconomic sense), we seem to lose this methodological distinction. In the next section I want to argue that the methodological purity of microfounded models is not quite as clear cut as the purists suppose.

3 Incorporating nominal inertia

A decade or two ago, it was still possible to argue that microfounded macromodels could not be Keynesian, because they *could not* incorporate price stickiness, and had to assume market clearing. Indeed Mayer (1993), although deeply critical of the microfoundations project, seemed to accept this logic. He 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...'

Today, it is quite acceptable to incorporate some form of price rigidity into microfounded macromodels. This acceptance coincided with a generalisation of Real Business Cycle models into Stochastic Dynamic General Equilibrium (SDGE) models. To all intents and purposes, SDGE models look just like RBC models, except that in many cases they include nominal inertia.

This development was important in establishing the hegemony of microfounded macromodelling. In particular, it allowed microfounded macromodelling into central banks, and an analysis of how monetary policy can moderate Keynesian business cycles. The two recent developments highlighted at the beginning of this paper would not have been possible without it. To achieve this, however, the microfoundations project did have to compromise, and the compromise involved modifying the concept of internal consistency.

It has always been technically possible to add price rigidity as a constraint within the optimisation process. Perhaps the first example of doing this within a model that was otherwise microfounded is Barro and Grossman (1971). At the time, however, the assumption of fixed prices was seen as 'ad hoc' and inconsistent with maximising behaviour, and the initiative died out. In particular, no justification based on rational optimisation for price rigidity was spelt out, and so this assumption 'lacked microfoundations'.

Move on twenty or more years, to one of the most influential models in open economy macro in the last two decades, Obstfeld and Rogoff (1995). This has a two period structure. In period 1, prices are fixed, having been set in the previous period. As a result, workers/consumers are off their labour supply curves. In period 2 prices are effectively flexible, because although still set in the previous period, under rational expectations the second period is known in the first. The model is carefully microfounded in every respect, and is seen as providing one of the cornerstones of the New Open Economy Macroeconomics (see Lane, 2001).

So what has changed? Why were theorists twenty years ago deterred from accepting microfounded macromodels incorporating this simple form of price rigidity, whereas now a very similar construct is acceptable? The answer is that originally price rigidity appeared to violate internal consistency. The assumption of predetermined prices appeared to be ad hoc, not just in the Popperian sense of being arbitrary in an attempt to explain some result, but also in the Lakatosian sense of running against the dominant heuristic (Hands, 1988) - agents were worse off as a result.

Those working within microfounded macromodelling would argue that what has changed is that we now have well-founded microeconomic stories that combine price rigidity with profit maximisation, stories that did not exist twenty years ago. One insight was the potential importance of imperfect competition. However, as Blanchard and Kiyotaki (1987) showed, imperfect competition alone was not enough. The first attempts to combine nominal inertia with imperfect competition were based on fixed contracts. Although fairly successful, in the sense that these models could generate Keynesian type behaviour, they did not appear to resolve the basic inconsistency, but instead simply transferred it. 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.

Although attempts were made to rationalise contracts, an alternative avenue opened up that ultimately proved more successful. The existence of menu costs as a primitive had always been recognised, but using these to explain nominal inertia involved an apparently critical problem: they appeared trivial in size besides the phenomenon they were trying to explain. What New Keynesian economists showed was that, for the individual imperfectly competitive firm, the gains from profit maximisation close to equilibrium were second order. As a result, small menu costs could be important enough to outweigh the gains to changing prices when prices were close to, but not at, the profit maximum. In addition, if imperfect competition was assumed in the labour market as well as the goods market, then 'realistic' menu costs might account for sufficient nominal inertia to generate business cycles.

So it became possible to use menu costs to justify nominal inertia in a way that appeared consistent with optimising and profit maximising behaviour. Unfortunately there was a catch: these formal models involving menu costs were quite complex. As a result, simply adding menu costs into microfounded macromodels to derive nominal inertia was generally intractable.⁷ The barrier to incorporating nominal inertia into microfounded macromodels was no longer a lack of microfoundations for price rigidity, but a simple matter of complexity. Obstfeld and Rogoff (1996) write "It is profoundly difficult to rationalise nominal price rigidities in a way that is both theoretically elegant and empirically sensible."

The solution was to incorporate ad hoc devices to model nominal inertia, and to justify them using the more complex microfounded stories which were 'off-model'. Two methods of incorporating nominal inertia into microfounded macromodels are now commonplace. The first simply assumes that prices are predetermined in a two period structure, along the lines of Obstfeld and Rogoff (1995) discussed above. Menu costs are not formally part of their model, but they are used as the justification for assuming predetermined prices. (Obstfeld and Rogoff cite menu costs, rather than fixed contracts, as a justification for their assumption of predetermined prices.) In other words, there is an *as if* story: predetermined prices work as if firms

⁷ 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.

faced menu costs. The second method is to use a formulation due to Calvo (1983), now known as Calvo contracts. Here monopolistically competitive firms maximise profits, but under the assumption that their prices will only change next period with some probability less than one. This probability is fixed, and is not a choice variable for the firm. At first sight this seems like a contract story rather than coming from menu costs, and it appears to share the inconsistency problems of assuming fixed contracts. However again it is possible to tell an *as if* story: the model works as if firms face menu costs, which are sometimes important enough to keep prices fixed, but not always.

We can describe the menu cost story as providing an *indirect theoretical consistency* argument. On the face of it, Calvo contracts are internally inconsistent because the probability of changing prices is assumed fixed, and firms would be better off if they endogenised this probability. This apparent inconsistency can be overcome by the indirect theoretical consistency argument that Calvo contracts represent the outcome of optimisation in the face of menu costs. The justification does not reference empirical evidence, but instead a body of (microfounded) theoretical work that explores the aggregate implications of menu costs.⁸

The Calvo formulation has one feature which at first appears as a nuisance, but which has recently been turned to great advantage. As each firm changes its price with some probability, then any model with Calvo contracts will inevitably involve heterogeneity. This is a nuisance in most cases, because the heterogeneity may become infectious. To prevent variations in profits across firms from influencing consumers, we need to assume that consumers own shares in all firms, or that some form of insurance exists covering variations in profits. However, the variation in relative prices implied by Calvo contracts when inflation is positive is crucial in allowing Rotemberg and Woodford (1997) to relate aggregate inflation to disutility in labour supply, which was the key innovation in the endogenisation of benevolent policy maker's objectives described earlier.

Thus what appears to have changed over the last two decades is that models of price rigidity have become more convincing, not in terms of empirical evidence, but in terms of theory based on microeconomic fundamentals. (Dixon (2000) holds the same view.) There does not appear to be any similar strengthening in the empirical evidence for nominal inertia. Empirical models based on Calvo contracts (the New Keynesian Phillips curve) are relatively recent, and it is far from clear at present how well they match the time series data. 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. What mattered is that one possible theory, based around menu costs, became convincing in the sense

⁸ The need for the 'as if' story to be theoretically convincing is illustrated by an alternative justification for nominal inertia which never caught on. A highly tractable way of incorporating nominal inertia into profit maximising behaviour 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 obvious 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 not appear theoretical convincing. This particular 'as if' story lacked good microfoundations.

that it was potentially consistent with profit maximisation, and therefore admissible to include as an *as if* justification as part of a microfounded macromodel.⁹

Nominal inertia is not the only example of an apparent inconsistency justified by an indirect theoretical consistency argument. Another would be the incorporation of money as a separable term in utility functions, representing 'liquidity services'. Liquidity services are not a basic consumption good, but rather a means to assist consumption through exchange. Putting money into the utility function represents an *as if* story for a transactions technology that is not formally spelt out in the model. Although formally modelling the transactions technology (e.g. as in 'cash in advance' models) would be preferable, it also appears fairly intractable, so an apparent internal inconsistency is tolerated because a convincing indirect theoretical consistency argument exists.

A common feature here is that a third element, besides internal and external consistency, is intervening: complexity. Direct internal consistency has to be sacrificed because it is intractable (too complex) to incorporate all microfounded and relevant behaviour in a single model. It would clearly be unreasonable to insist that all models had to be simple and all encompassing, at least for a complex phenomenon like macroeconomics. Complexity, rather than empirical evidence alone, rules out complete or direct internal consistency, and instead internal consistency has to allow an appeal to additional off-model theory.

Furthermore, it seems likely that as microfounded macromodelling develops in an effort to explain more aspects of macroeconomic reality, this kind of compromise will occur repeatedly. This is simply because attempting to model a macroeconomy in a general equilibrium fashion is inherently more complex than many microeconomic problems. The macroeconomic modeller can circumvent this to some extent, by tailoring simplifying assumptions to the question the model is designed to address. For example, if the concern is only with long run equilibria, then ignoring nominal inertia may be legitimate.¹⁰ However, this ability to tailor assumptions to objectives only mitigates the problem of complexity, as exactly the same can be done for microeconomic issues where the environment is inherently simpler. I would conjecture that tensions between internal inconsistency and other desirable model criteria are likely to become endemic within microfounded macromodelling. The previous section provided one recent example.

This modification of microfoundations methodology, to allow 'off-model' justifications for particular relationships, at first sight appears not to compromise the primacy of microeconomic theory over econometric consistency as criteria for model admissibility. The justification for including

⁹ It is probably the case that, once let in, macro theorists have realised that it is rather easier to explain the character of the business cycle using nominal inertia than with RBC models, and this may explain the rapid spread of microfounded models incorporating nominal inertia (see McCallum (2000) for example). However the ability to generate results (Freidman's positivism) cannot have been the dominant motivation, otherwise this would have been discovered a lot earlier.

¹⁰ This is one of the key differences between models in academic papers and large models by policy institutions used by forecasting institutions, in that the latter have to be 'horses for all courses' (Currie, 1985). This is why the SDGE model, which is the core of the Bank of England's forecasting model, is so complex.

features like Calvo contracts or money in the utility function is not empirical evidence, but rather a theory (or body of theory) based on optimisation under constraints. However, the modification becomes more problematic when we ask how such claims can be validated.

Compare the following two statements:

(A) In the absence of menu costs or other constraints, a firm that does not adjust its price when relevant state variables change will not be maximising profits

(B) In the presence of menu costs, profit maximising firms act in exactly the same manner as if they chose to change their price with some fixed probability each period (Calvo contracts)

It takes some simple algebra to prove (A). It is almost certainly the case that it is impossible to demonstrate (B) in a similar way. In fact it is quite likely that in some cases (B) can be shown to be false, so the claim is in fact less precise. For example, the probability of changing prices will almost certainly depend on the general level of inflation (see Woodford (2003) pages 179/180, footnote 32). We should substitute the word 'approximately' for 'exactly', but then the statement becomes even less rigorous and difficult to validate. Statement (B) becomes a claim, the validity of which may vary from model to model, which is not demonstrated formally, and may be impossible to demonstrate formally. The claim is that if this off-model theory could be formally incorporated into the microfounded macromodel, the enlarged model that would be created would behave in a similar manner to the actual model analysed. However, such incorporation does not produce a tractable model, and so an apparent theoretical inconsistency (like Calvo contracts) is tolerated to obtain tractability. Here intractable means 'it cannot be done' rather than 'it would be rather messy and complicated to do it'.¹¹

The difficulty of demonstrating the validity of claims like (B) might appear similar to justifying devices such as 'all firms of identical' or 'there are only two goods'. In both cases any assertion that such devices are not critical to the model's results is often a judgement, which cannot be shown to be correct because abandoning them would prove intractable. As Dow (2002) states succinctly: 'Theory by definition requires simplifying assumptions - otherwise it would amount only to description'. However, there is a key difference. Assumptions like 'all firms are identical' are simplifications, which no one would claim violate rationality. Calvo contracts on the other hand, or one period fixed prices, do on their own appear to violate internal consistency.

Thus the use of ad hoc devices to incorporate nominal inertia is not a convenient shortcut for more elaborate procedures. It is an essential device to produce a tractable model. The argument that they stand in for, or represent, a well microfounded story that is consistent with the model being developed

¹¹ Complexity that is tractable is not a deterrent. This is clearly illustrated by the recent advances in the derivation of policy makers' objective functions from agents' utility described earlier. As such derivations are model specific, most microfounded macromodelling papers examining monetary policy now derive objective functions in this way, even though this probably doubles the equation count in these papers.

has to remain a judgement. Furthermore, it is a judgement that is clearly contestable. In the case of Calvo contracts, for example, is it really the case that the probability of a firm changing its price is independent of the size and form of the shocks hitting the economy? A number of papers have looked at alternative price setting structures where this is not the case, but the jury remains out on which, if any, might provide a more microfounded account of nominal inertia that is also tractable, and whether the deviations from the Calvo set-up are important. (See Woodford, page 177, footnote 29. Sheedy (2007) is a particularly elegant exposition.)

The dilemma facing the model builder is as follows:

- 1) an important empirical regularity cannot be explained by existing microfounded macromodels, or such explanations appear unsatisfactory (e.g. business cycles).
- 2) the regularity could be explained by introducing a new element into these models, but this element appears to violate internal consistency (e.g. nominal inertia).
- 3) in some simple models the new element mimics the behaviour of features that are consistent with microeconomic theory, but incorporating these features into more complex macromodels is intractable

In this situation, the modeller has to decide how long do you wait before (3) becomes sufficiently convincing to be admissible as an indirect justification for overriding (2), and therefore overcoming the problem stated by (1)?

The key point here is that this judgement has to be one of degree: we cannot prove that (3) is a valid indirect argument justifying internal consistency. What happens instead is that statements like (B) just become more or less 'plausible'. As such, it is quite different from issues of mathematical proof, or normal claims of internal consistency (i.e. quite different from (A)). In practice it is also difficult to believe that these judgements will not be influenced by the strength of the empirical case behind (1). As a result, the degree of external inconsistency is having an influence on what is admissible in a microfounded model. In this sense, *I would suggest that indirect internal consistency claims in practice do represent a key methodological departure for microfounded macromodelling.*

It also seems likely that indirect internal consistency judgements are bound to depend heavily on a social consensus among theorists. It is undoubtedly the case that the group's acknowledged 'leaders' greatly influence this consensus. Such a position appears to sit uncomfortably within the rigorous, deductive approach that is embodied in microfounded macromodelling.

It therefore appears that microfounded models embodying nominal inertia already contain within them key elements where internal consistency is much more problematic than the microfoundation 'purists' imply. Even if this is conceded, however, does it justify incorporating elements that appear, at least at present, clearly inconsistent with rational behaviour, such as rule of thumb price setting? The problem here is that we cannot be certain that 'rule of

thumb' behaviour is irrational. As I noted above, it could represent the result of information being costly. In time, theory may be developed that shows that rational price setters behave as if they followed rule of thumb behaviour. So we have on the one hand Calvo contracts, say, which are currently regarded as representing the influence of menu costs, but where it has to be conceded that there is some probability that in the future we will reject this view. On the other hand we have inflation inertia, where there is some chance that future research will show this as the outcome of rational behaviour, and so is internally consistent with microfounded models. It is not obvious that the two probabilities involved in these two statements are an order of magnitude different. In other words, the methodological purity of the microfoundations project has already been compromised, as indeed it had to be if these models were ever going to provide realistic guides to policy given the complexity of macroeconomic behaviour.

4. Conclusion

An analysis of the microfoundations revolution in macroeconomics emphasises that internal consistency is a necessary requirement of a good microfounded macromodel. In particular, internal consistency should never be sacrificed to empirical evidence (external consistency). Although external inconsistency (often in the form of 'puzzles') may be crucial in motivating theoretical development, internal consistency means that these models can only develop as quickly as microfounded theory evolves. In the case of nominal inertia, it took decades for New Keynesian theory to provide the argument that price rigidity might be a rational response to the presence of menu costs in an imperfectly competitive environment.

As macromodels are used by policymakers to address serious issues, then the fact that microfounded macromodels might evolve rather slowly is a significant problem, particularly as the consensus view appears to be that policy actions should be based on microfounded models. It has led some to incorporate elements into otherwise microfounded models that have, as yet, no clear microfoundation. The example discussed in this paper is inflation inertia. However, others have objected to this practice, because they argue that this violates a key component of the microfoundations project, which is that internal consistency is a necessary criteria for model admissibility.

The problem with this latter argument that we identify in this paper is that the internal consistency criteria has already been modified as a result of the incorporation of price stickiness into microfounded models. Price rigidity, either directly or through devices such as Calvo contracts, is justified in terms of profit maximising behaviour by an indirect appeal to a body of theory that is 'off model'. This is because to directly include elements like menu costs into each model becomes intractable. While this justification through indirect reference appears to remain essentially based on microeconomic theory rather than aggregate data, these claims cannot be validated in the same way as direct (within model) analysis of internal consistency can be. Devices like Calvo contracts are typically not convenient short-cuts that could be avoided, but essential devices for problems that would otherwise be intractable. As a result, such indirect claims to consistency are probabilistic judgements, which in practice are likely to be influenced by the strength of the macroeconomic empirical 'puzzle' they are trying solve as well as the plausibility of the microeconomic analysis they utilise. As a result, the methodological integrity of the microfoundations project is not as unblemished as many of its practitioners sometimes imagine.

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