

Internal Consistency, Nominal Inertia and the Microfoundations of Macroeconomics

**Simon Wren-Lewis
University of Exeter**

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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.' (my italics). Almost without exception, theoretical macroeconomic papers in the top economics journals now involve microfounded macromodelling (hereafter MM). Even large forecasting models, which had been regarded as more empirically orientated and more eclectic in their use of theory, are now embracing the microfoundations project (Bank of England reference to be added).

In the first section of this paper I argue that MM represents a methodological position that is clearly distinct to other approaches in macroeconomics. In particular, MM 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 even motivate model development (often expressed as 'puzzle resolution'), 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 be applied to macroeconomic theory.

However, in the second section of this paper I want to argue that as the microfoundations project has developed and become dominant, some key exceptions to this position have been tolerated. The most important involves nominal inertia. Whereas twenty or thirty years ago it was thought that assuming some form price rigidity was inconsistent with MM, now these models routinely incorporate some form of price rigidity. How have these exceptions been justified? The argument often put forward is that internal consistency is maintained by indirect reference to other, off-model microfounded analysis. As a result, it is suggested, the methodological position of MM has not been compromised.

In the final part of this paper I shall suggest that these exceptions have compromised the methodological clarity of the microfoundations project. Precisely because these 'indirect consistency' claims cannot be formally verified, they bring an element of subjective judgement into what counts as MM. There now appears to be some ambiguity about whether other factors, such as inflation inertia, which have empirical support but currently lack microfoundations, should be investigated under the MM banner. A conclusion summarises the main points.

1. The central role of internal consistency

A near universal characteristic of papers that present models in the microfounded modelling (MM) class illustrates the importance attached to internal consistency. 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 consumption¹ from the optimising behaviour of a representative, infinitely lived consumer probably appears in most MM papers.

Would it not save a lot of paper to instead justify aggregate relationships by reference to derivations in early studies, when such derivations have become standard? In fact, such a practice was quite normal in an alternative class of models that were 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).²) 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.

Why are policy models not classed as MM, even when the aggregate relationships of policy models are justified by reference to individual pieces of microeconomic analysis? The reason is that such references, even if they are correct, may be internally inconsistent. To take a trivial example, suppose our imagined paper cites A as showing that its consumption function comes from a particular consumer optimising problem, and cites B as showing how to derive an aggregate labour supply equation. We cannot tell without reading A and B and doing further analysis whether these two equations are internally consistent: would they both result from a single optimisation exercise where the agent chose both consumption and labour supply?

The advantage of setting out the derivation of aggregate equations formally and explicitly for each and every model in the MM class is that the internal consistency of the microfoundations can be directly demonstrated and checked.³ As a result, the microeconomic basis for aggregate relationships within MM is never asserted by reference, even when the derivation in question has been established in countless previous papers. It is not sufficient to say that the model's relationships are *capable* of being derived from optimising behaviour. An essential part of MM papers is that derivations are presented in a self-contained way.

Of course, microfoundations and internal consistency are not separate entities. An apparently microfounded model that is not internally consistent is

¹ aggregate consumption at time t depends on consumption at $t+1$, plus a term in the real interest rate.

² Blanchard and Fisher (1986) call these 'useful models' in their advanced macro textbook.

³ One disadvantage is that typically only one microeconomic derivation of a particular aggregate relationship will be given. In practice, a particular aggregate relationship may correspond to (be derivable from) more than one microeconomic approach, a point that could be noted when derivation is given by reference.

not correctly microfounded. Instead, 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.⁴ In the view of some, non-MM models are problematic not 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 MM that may appear rather odd to outsiders. MM tends 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 MM and *external* consistency is quite different. 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 MM approach. Relationships that appear at odds with the data are tolerated if their microeconomic provenance is clear.

This is not to imply that empirical evidence is irrelevant to MM. Indeed, particular empirical facts (often called 'puzzles') will often be presented as 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 tracks data on consumption etc.

A clear example of this 'imbalance' between internal and external consistency in MM 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

⁴ As far as I am aware, no one has tried to precisely define internal consistency in this context. One possible definition is that the agents in the model optimise across all possible utility maximising possibilities. This would capture simple inconsistencies like the one involving consumption and labour supply noted above, as well as issues involving expectations formation and price rigidity. It would also ensure that there were no unexploited arbitrage opportunities available to agents in the model.

key part of virtually all MM, whatever the empirical evidence on the matter might suggest.⁵

The example of policy models (models that start with aggregate relationships, where these relationships are justified by reference to theory and evidence) illustrates that this is not the only way to construct a macroeconomic model. Two more alternative approaches are structural econometric models (SEMs), where individual relationships are estimated econometrically as part of the modelling process, and VARs, where external consistency is paramount and theory input is relatively minimal.

This distinction between MM and alternatives such as policy models or SEMs is close to the contrast Mayer (1993) makes between 'formalist' and 'empirical science' modelling approaches. He discusses in great detail the relative merits of the two approaches. His distinction relates to alternative methodological approaches in economics. MM, 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 a more Popperian approach.⁶

The extent to which the alternatives to MM embody a consistent methodological approach is not my concern here, nor do I want to attempt to outline the pros and cons of these alternatives. Instead I want to emphasise that MM appears to embody a consistent methodology, which requires microeconomic consistency as a necessary condition, and in which equations that appear inconsistent are not tolerated. In the following section I shall suggest that the success of MM has only been achieved by modifying this methodology in an important way.

⁵ An exception might be models based on learning, although these are still in their infancy.

⁶ I suspect many young macroeconomists working in the microfoundations tradition (which means those at the better departments) would see the distinction between MM and policy models or SEMs not as methodological, but as simply between the old and the new. In this view MM do what other modelling approaches do, but better. However, the idea that MM encompasses these other approaches – because the former adds explicit microfoundations without necessarily subtracting anything – is misleading in practice. MM 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 in this way: aggregation problems are typically cited here (e.g. Hoover (1988)). The reductionist approach may not in practice be the most useful: Hendry in Backhouse and Salanti (2000) makes an interesting analogy with hydrology.

2. Incorporating nominal inertia

A decade or so 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 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 MM in macroeconomic theory. In particular, it allowed MM into central banks, and an analysis of how monetary policy can moderate Keynesian business cycles. 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 an (otherwise) MM approach was due to 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 contrast, one of the most influential models in open economy macro in the last decade, Obstfeld and Rogoff (1995), has a two period structure. In period 1, prices are fixed, because they have 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 International Macroeconomics, which applies MM to open economy macroeconomics (ref Lane).

So what has changed? Why were theorists twenty years ago deterred from accepting MM 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.

What has changed in the intervening period that now makes the assumption of fixed prices acceptable? One possible and straightforward explanation is that the empirical evidence supporting nominal inertia has got stronger over time, and that this has finally persuaded those involved in MM to compromise their methodological principles. However, I doubt whether

anyone within this traditional would accept this interpretation. Those with MM continue to maintain the importance of internal consistency, and nowhere is there any get out clause which says 'except where there is compelling empirical evidence'.

Instead, those working within MM 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 contracts, when it appeared to make them worse off?⁷

Although attempts were made to rationalise contracts, an alternative avenue opened up that ultimately proved more successful. The existence of menu costs could not be denied, 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, so small costs like menu costs could be important. 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 MM to derive nominal inertia was generally intractable. The barrier to incorporating nominal inertia into MM 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, but to justify them using the more complex microfounded stories which were 'off-model'. Two methods of incorporating nominal inertia into MM are now commonplace. The first simply assumes that prices were predetermined, 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.⁸ In other words, there is an 'as if' story: predetermined prices work as if firms 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

⁷ The argument that such contracts clearly existed did not appear forceful enough: again internal consistency overrides external consistency.

⁸ Obstfeld and Rogoff cite menu costs, rather than fixed contracts, as a justification for their assumption of predetermined prices.

less than one. This probability is fixed, and is not chosen by 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.⁹

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.¹⁰ 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 economists 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 that it was potentially consistent with profit maximisation, and therefore admissible to include as an 'as if' justification as part of a MM.¹¹

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

⁹ 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 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 not appear theoretically convincing. This particular 'as if' story lacked good microfoundations.

¹⁰ Dixon (2000) holds the same view.

¹¹ 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.

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. 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 internal consistency, and instead internal consistency has to be replaced with an appeal to additional off-model theory.

Furthermore, it seems likely that as MM 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.¹² 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 MM. (The next section gives an example.)

Two general points can be made about indirect theoretical consistency arguments like those used to justify Calvo contracts or money in the utility function. The first, and most obvious, is that they stay within the microfoundations realm. There is no direct appeal to empirical evidence, but rather a theory (or body of theory) based on optimisation under constraints. The second is that the argument represents a claim, which is not demonstrated formally, and may generally be impossible to demonstrate formally. The claim is that if this off-model theory could be formally incorporated into the MM, the enlarged model that would be created would behave in a similar manner to the actual MM analysed. However, such incorporation does not produce a tractable model, and so an apparent theoretical inconsistency (like Calvo contracts) is tolerated to obtain tractability.

One final point is worth noting about this story. From today's perspective, was the original view that nominal inertia was not compatible with MM correct? The answer from a MM perspective would appear to be yes, because at that time convincing microeconomic stories rationalising the importance of nominal inertia were absent. However, the result was that today's MM of the business cycle was delayed by twenty years or so. Surely this was unfortunate.

¹² This is one of the key differences between academic macrotheory and large SEMs maintained by policy institutions, in that the latter have to be 'horses for all courses' (Currie, 1985).

An alternative would have been for MM to tolerate nominal inertia from the very beginning, on the grounds that the empirical evidence for it was very strong. Modellers could have taken a bet that a microfoundation for the importance of nominal inertia would emerge in time. Sure, the methodological purity of the MM enterprise would have been (temporarily) compromised, but set against this the development of business cycle theory might have advanced by a decade or so

3. When to sacrifice internal consistency?

In section 1 we examined how MM 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 section 2 we showed how this position had to be modified to incorporate nominal inertia. Directly incorporating microeconomic justifications for nominal inertia into Keynesian business cycle models was intractable, so these models instead incorporated ad hoc devices such as Calvo contracts. However, the justification for including these ad hoc devices related to microeconomic theory, rather than empirical evidence. This theory showed how menu costs could have ‘first order’ effects large enough to generate business cycles

In this section we will examine the extent to which this modification preserves the methodological integrity of the MM approach. We can start by noting one methodological feature that does seem to have been retained. Allowing nominal inertia has not signalled any change in the (un)importance of external consistency in MM. The strength of the empirical evidence for nominal inertia has not changed substantially over the last twenty years, and this evidence does not appear to have been critical in the acceptance of nominal inertia within MM. In this sense, the primacy of microeconomic theory over empirical evidence within MM has been retained.

The problem lies more with the overall consistency of this microeconomic theory. The difficulty with indirect appeals to internal consistency is that their validity cannot be unambiguously established. By definition, building a complete model that avoids any ad hoc devices is intractable. Here intractable means ‘it cannot be done’ rather than ‘it would be rather messy and complicated to do it’.¹³

Thus the use of ad hoc devices to incorporate nominal inertia are not shortcuts to more elaborate procedures. The argument that they stand in for or represent a well microfounded story *that is consistent with* the model being developed has to remain an untested judgement. 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? Analysis based on menu costs suggest that the possibility of price inertia depends not only on the size of these shocks, but also the structure and parameters of the model. The appeal to menu costs as a justification for Calvo contracts in the context of any particular model is, at least at present, an unproved conjecture.

¹³ Complexity that is tractable is not a deterrent. This is clearly illustrated by recent advances in the derivation of policy makers’ objective functions from agents’ utility. Woodford (2004) shows how this can result in a traditional objective function in output and inflation when the model involves Calvo contracts, but this result is model specific and extremely equation intensive. As the result is model specific, nearly all MM papers examining monetary policy now derive objective functions in this way, even though this probably doubles the equation content of these papers.

The dilemma facing the MM model builder is as follows:

- 1) an important empirical regularity cannot be explained by existing MM, or such explanations appear unsatisfactory (e.g. business cycles).
- 2) the regularity could be explained by introducing some element into these models, but these element appear to violate internal consistency (e.g. nominal inertia).
- 3) these elements may be explained by microeconomic theory, but this theory is too complex to formally incorporate into MM (menu costs).

In this situation, how long do you wait before (3) becomes sufficiently convincing to be admissible as an indirect justification for overriding (2), and therefore exploiting the advantages offered by (1)?

The key point here is that this judgement has to be one of degree: we cannot at present prove that (3) is a valid indirect argument justifying internal consistency. Instead, the theory in (3) will just become more or less 'convincing'. As such, it is quite different from issues of mathematical proof, or even internal consistency itself. The acceptance or not of arguments about the plausibility of indirect claims to theoretical consistency are judgements, and as such are bound to depend heavily on a social consensus among theorists. It is undoubtedly the case that the groups acknowledged 'leaders' greatly influence this consensus.¹⁴

Such a position appears to sit uncomfortably within the rigorous, deductivist approach that is embodied in MM. As a result, there now appears to be real uncertainty about the boundaries of the MM exercise. This is illustrated by the issue of 'inflation inertia', which is closely related to '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: the 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 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! A number of economists have suggested modifications to the Calvo set up to capture 'inflation inertia'. It is fairly clear what the consequences of such a modification would be: current inflation would depend on lagged inflation as well as future inflation. The lagged inflation term would make disinflation costly. 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.

¹⁴ I once had one of my own attempts at a MM described by a referee as 'not quite coshar', and I think this phrase is revealing. The referee advised acceptance and the paper was published, so I bear no grudge.

Some authors - who clearly work in the macroeconomics mainstream - have nevertheless investigated incorporating inflation inertia into MM (e.g. Steinsson, 200x – add ref).

Yet this development worries others within the MM tradition. I was recently at a conference where a 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: to quote “microfoundations had to be respected”! In their view using Calvo contracts was allowable with MM, because it had microeconomic backing, but (as yet) inflation inertia did not, so MM should eschew inflation inertia. Only this would keep the methodological integrity of MM in tact.

To others this judgement seems overly restrictive. Allowing nominal inertia has already compromised the methodological position of MM, but in terms of the influence of the project it has been a hugely successful compromise. It has allowed MM to enter, and now largely dominate, an area of macroeconomics - monetary policy - that had been the preserve of more traditional Keynesian policy models. MM now has the ear of policy makers. But policy makers would like to know what impact inflation inertia would have on the policy recommendations coming from MM, if only because the empirical evidence for inflation inertia seems strong. To many economists, ostracising models containing inflation inertia appears at best petty, and at worse will hold back academic development. They might add, following the example of nominal inertia, that the microfoundations for inflation inertia might well emerge from research in the next ten years or so, so why wait for this to happen?¹⁵

¹⁵ Two retorts to this view appear weak. The first is that the microfoundations for inflation inertia may not emerge, and the empirical evidence may disappear. But this is ultimately an empirical conjecture! Second, it could be argued that there is nothing stopping economists close to policy from investigating such things – just call them policy models and not microfounded models. The problem here is the widely held view that the best academic analysis should be restricted to MM, so in effect this dislocates academic analysis from the policy process.

5. Conclusion

Much of the early analysis of the microfoundations revolution in macroeconomics appeared to suggest that internal consistency was a necessary requirement of a good microfounded macromodel. In particular, internal consistency should never be sacrificed to empirical evidence ('external consistency'). I have argued here that the widespread adoption of price stickiness into microfounded models represents a modification of this position. Internal consistency could be sacrificed, if there existed an indirect theoretical consistency argument, a convincing off-model 'as if' microfounded story, for part of a model's specification. Calvo contracts, which have been widely adopted within microfounded macroeconomics, are clearly internally inconsistent with profit maximisation, but can be justified by an appeal to the microeconomic analysis of pricing with menu costs.

Incorporating nominal inertia into microfounded models has helped extend their influence, both within academia and among policy makers. Given the inherent complexity of macroeconomics, the need for similar indirect consistency claims is likely to arise in the future. However the decision over when to allow off-model 'as if' stories to override apparent internal inconsistency is problematic, because the validity of such justifications cannot (as yet) be formally established. Instead the decision involves issues of judgement that are not black or white. As a result, the methodological position at the heart of the microfoundations project, which places theoretical coherence above empirical explanation, appears to be compromised.

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