

Endogenous Indexing and Monetary Policy Models
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E-conomics Discussion Paper 2007-36 (MS 92)

Response to Referees

I am very grateful to the two referees for their careful consideration of the paper and the comments made. This both responds to those comments and sets out how a revised version of the paper would address them.

I note that neither referee raises any difficulties with the technical content of the paper. If I have interpreted them correctly the comments made relate primarily to presentation and clarity/depth of exposition (Referee 1), and motivation and empirical evidence (Referee 2).

Referee 1

First let me note that the summary of the paper (first paragraph) is both correct and extremely clear, and naturally I was pleased with the positive assessment (middle paragraph). On the comments made (third paragraph):

i) On re-reading the paper I agree that the explanation of the simulations done on the Christiano et. al. (2005) “CEE” model is too concise and the stability discussion could be made much clearer. When writing the paper I was unsure how much space to devote to this model as the microeconomic analysis suggested that it was less plausible than the others though it is in widespread use. I propose expanding/clarifying this part with the details of the simulations in an appendix if necessary.

ii) I agree with the criticism of using the term propositions and plan to relabel these results instead, but keeping the statements of what has been shown intact as I think its helpful for the reader to have a summary statement of what has been found.

Referee 2

Responding to “2. Comments” I shall discuss the motivation for the paper and the contribution it makes from a theory perspective before turning to the issue raised of micro empirical evidence.

i) Motivation/contribution.

I think the relevant “overview” points made by the referee on this are in the first and penultimate sentences:

“I find this paper an interesting one but the new models presented in this paper are not better than the hybrid models.”

“If the main goal of the paper is to illustrate the shortcomings of the existing hybrid models, the shortcomings of these models are already well known (eg. Woodford (2007)).”

I think it is helpful to briefly set out the components of the existing hybrid models before stating the contribution of the paper and responding to these points. In very concise form the existing hybrid models comprise:

- a) A time dependent rule (of the Calvo constant hazard form) which determines when a price may be reoptimised (CEE and LAPI models) or changed (Gali-Gertler).
- b) Indexation or rule of thumb behaviour, motivated by unspecified decision or optimisation costs. In the CEE and LAPI models prices change by indexation each period between reoptimisations; in Gali-Gertler a new price is determined by rule of thumb when the price may be changed and remains fixed otherwise.
- c) The structure of the indexation/rule of thumb formulae are not microfounded and the coefficients within them are simply fixed at particular values.
- d) Given (a)-(c) these models fit macro data much better than the standard Calvo pricing model without indexation/rule of thumb behaviour, essentially because these mechanisms introduce lagged inflation into the Phillips curve.

As a preliminary point, (a) and (d) above are not at issue here, though it may be noted in relation to (a) that use of a Calvo constant hazard function within the family of possible time dependent rules might be questioned, and there is near-universal agreement that state dependent models would be superior though these are not yet sufficiently developed for widespread use; and in relation to (d) there is ongoing empirical debate about the quantitative significance of lagged inflation in the data.

The paper retains (b) while unpicking (c) by endogenising the structure and coefficients of the indexation/rule of thumb functions, and finds that if one does so (d) is significantly undermined. My reading of the literature is that there is widespread agreement that (b) is not ideal from a methodological perspective, as it departs from optimising microfoundations, but much of the literature compromises on that to achieve (d). Point (c) has not been separated out previously. The contribution of the paper is to show that even if one retains (b) that compromise is undermined as (d) largely disappears once (c) is questioned. Since (c) is a step in the direction of enhancing the microfoundations of these models and moving them closer to optimising behaviour (though not reaching that point as (b) is retained) the paper shows that the compromise of assuming (a)-(c) in order to achieve (d) is a much less comfortable one than previously thought: not only does one have to assume indexation/rule of thumb behaviour, which potentially might be

rationalisable in terms of decision costs, but one also has to assume that firms implement this approach in an arbitrary way, experience lower average profits than would be the case if they thought more carefully about sensible indexation rules/rules of thumb, and that if they were initially constrained by imperfect information and adopted an inefficient rule they would not learn from experience over time.

Turning to Referee 2's points above, on the first quote that the models are not better than the hybrid models I agree entirely if what is meant is that point (b) above is simply unacceptable on methodological grounds and hence the indexing/rule of thumb models should simply be rejected, with or without the modification of (c) undertaken in the paper as it retains (b). I think that is a logically coherent position, but my personal view is that the paper still has a very strong motivation simply because these models are very prominent in the literature and are in very widespread use. Given that, it seems to me to be important to understand the properties of these models and to assess exactly how severe the tradeoff is between moving away from optimising microfoundations and achieving better data consistency. Hitherto a reading of the literature might be that as long as one accepts the general idea of indexation/rules of thumb, which again might be justifiable if there are decision costs of some kind, then data consistency is achievable. This paper shows that this tradeoff is much more severe than previously thought. In passing it might be worth mentioning that I share the general scepticism towards these models. If the results of this paper encouraged the search for other types of model which came closer to data consistency without departing from optimising microfoundations that would surely be very positive, but for the time being at least the hybrid models are heavily used and pragmatically I believe that strongly motivates the present paper.

The first quote above could be interpreted simply as saying that the paper offers nothing new, and the second quote appears to imply exactly that. This is not correct. As argued above, the paper unpicks (c) and finds that this has significant implications for (d). Hence the paper is not simply illustrating the shortcomings of these models that are known already (which are very clearly summarised in Woodford, 2007) but adding an extra shortcoming. The first referee, for example, refers to scepticism about these models and states that the “...analysis contributes to the reasons why we should be sceptical”.

A final comment on the theory part of Referee 2's comments is just that I agree that the Gali-Gertler model lacks microfoundation but it is not correct that Christiano et. al. (2005) resolve the problem. The latter simply make different assumptions about price changing behaviour, essentially that prices may change each period rather than infrequently as in Gali-Gertler, but both assume arbitrary indexation/rule of thumb formulae and impose arbitrary coefficients. Hence both models share properties (b) and (c) above. For balance one should state that the paper shows that the structure of the Gali-Gertler formula turns out to be efficient.

ii) Micro empirical evidence.

I strongly agree with Referee 2 that consideration of micro evidence on price changing is important. It is mentioned in the paper but not highlighted, essentially because the

consistency or otherwise of these models with micro data is driven entirely by (a) above and that is held fixed in the paper. In other words if existing hybrid models are judged consistent or inconsistent with micro data, exactly the same judgement would be reached on the models in this paper.

Referee 2 makes two points on the micro data. The first is that available evidence is inconsistent with the underlying assumption of the Christiano et. al. paper (and the new LAPI variant) that every firm changes its price every period. I agree with that assessment and for that reason (Section 2) combined Christiano et. al. indexing firms with standard Calvo firms when deriving the Phillips curve for this model. This shows that moving towards micro data consistency in this way potentially undermines the macro data consistency of this model by reducing the coefficient on lagged inflation in the Phillips curve. This is a “standalone” result of the paper independent of the idea of endogenising the indexing parameter, and it would allow one to vary the relative shares of indexing and non-indexing firms to match micro data. Alternatively if it was judged that non-optimising firms are more likely to have sticky prices then the Gali-Gertler framework becomes more appealing and the paper covers that case also. Hence the paper takes an agnostic stance on this aspect of the micro data and allows the reader to see the results of endogenous indexing across all possibilities.

The second empirical point is whether there is any direct empirical evidence on whether the price setting behaviour of firms depends on the degree of persistence in inflation. This would be the case in the models considered. Unfortunately the answer is no. While the newly available micro evidence is extremely informative in many dimensions it says little about whether firms are indexing or using rules of thumb at all, let alone the nature of the formulae in use and whether their coefficients depend on lagged inflation. Obviously if that evidence was available it would have been discussed. A note is that Dhyne et. al (2005) report that price reviews appear to be more frequent than price changes which has been taken as evidence against the indexing models in which price reviews (in an optimising sense) should be infrequent, though whether that evidence is conclusive is open to debate. Hence unfortunately the micro evidence tells us little about the innovation in the paper of endogenous indexing and pending further micro evidence its motivation remains primarily analytical.

To conclude the discussion of Referee 2’s comments, the query about the paper’s motivation and contribution has been discussed thoroughly; one view is that the hybrid models and the variants in the paper should be rejected in their entirety due to non-optimising microfoundations, while an alternative view is that these models are very prominent and heavily used so understanding their properties and finding that their macro data consistency is strongly undermined by endogenous indexing is an interesting result. Clearly this is a judgement call but I intend to make the paper’s discussion of motivation and its contribution more explicit in the revised form. On micro empirical evidence I also intend to expand the discussion along the lines above; the paper remains of interest whatever view one takes of how often prices change while unfortunately there is little direct evidence for or against the idea of endogenous indexing.

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November, 2007

(References in Referee 2's report)