Referees report on 'What do micro price data tell us on the validity of the NKPC?' by L Alvarez

This paper 'does what it says on the tin'. It attempts to summarise what we know from micro price data collected by National Statistics Offices and central bank surveys about the data-congruence of models of price stickiness. It concludes, unsurprisingly, that the models leave many facts to be explained.

Overall assessment

This paper is thoroughly researched. There are not many truly new insights in this paper, but collecting together observations made by many authors in disparate works is useful and I think this paper could serve as a reference and starting point for future work.

I offer some comments below.

Substantive comments

Although it is fine for the paper to focus on what the micro data has to say about the NKPC, I think there would be merit in pointing out early in the paper what the models, combined with the macro-data, have to say about facts of interest, like the frequency of price-setting, or the presence of indexation, for example. Eg, to fit the macro data, we need a Calvo probability of x…. How well or not the models fit the macro data sets the scene for the contribution micro data can make.

Could you be more discriminating in the lists of papers you cover? There are rather a lot, and the differences between some of them are minor, and not that interesting. Why not just choose a representative paper from each of your categories?

You make a big play of the fact that some models allow for heterogeneity and some don't. I think your treatment of this is a bit misleading. No modeller seriously believes all firms are the same. And we will never be surprised to find that micro data rejects that they are the same. There is nothing inherent in the models that precludes modelling heterogeneity. Whether authors did or not was a choice based on tractability or the question they wanted to focus on.

Since you do not address the question of how well the models fit the aggregate time series data, why not skip the stuff deriving the different Phillips Curves?

The paper attempts to use the surveys to address the issue of to what extent firms operate under imperfect competition. I thought that this was where the surveys were likely a rather weak source of evidence. It may at least be mentioning other evidence from the empirical IO literature that assesses this.

The paper doesn't end on the right note for me. You note correctly that almost all the models fail to fit some of the micro facts. And you state that we need more 'realism'. I think that you mean we need more heterogeneity and detail. But is this right? We might build in such detail and fit the micro facts, but find ourselves with a model that is hopelessly complicated, and does not illuminate anything. The models of sticky

prices generate (of course) sticky prices, which in some measure is congruent with the micro data, and also fit the macro facts that, for example, monetary shocks have real effects. Isn't that a measure of their success? The extent to which you need a model to fit every fact at your disposal surely depends on the question you are asking of them. You seem to suggest that because not every micro fact is explained by any model, these models are not helpful for any purpose.

Minor comments

Page 2, para 2. The text says that micro data can be used to distinguish Rotemburg from Calvo price-setting. So it can. But why not draw this out explicitly. Something like 'Calvo implies periods of fixed prices followed by jumps. Rotemburg pricing implies continuously evolving prices.'

Bottom of page 6. Is it correct to say that under state dependent pricing the probability of price change is independent of how long prices have been fixed for? In an era of positive general inflation I would say that for most firms the probability of a price change increases with time under state dependent pricing.

You say that 'unsurprisingly' price change frequencies are higher for countries with higher recent general inflation rates. I don't think you should write this: this fact is something that helps discriminate between your models, no? For example, it rules out models where prices change every period, and rules in state dependent pricing models, where higher inflation means you cross the Ss bound in a faster time. You start the paper with no priors about which models are right, so these facts are revealing, not unsurprising.

Bottom of Page 14. What are 'Taylor agents'? I don't think this is a widely used term.

You note that your own previous work with a mixture of Calvo agents can generate declining hazard functions. Wouldn't Lucas say that this was cheating? Sargent quotes him as saying: 'beware of economists bearing free parameters'. You could always rig a distribution of Calvo firms to fit the data. Indeed you could estimate that mixture using the hazard function. Would that prove anything?

Page 18: why is it to be expected that there is a small share of time-dependent price setters in high inflation countries? I could change my price as a rule every day, and that would make me a time-dependent price-setter, and that would be a rule that was appropriate for high inflation.

Fundamentally no company surely can really be employing solely 'time dependent' price setting rules. There would be some shock that would prompt all firms to change prices within the habitual intervals. It is just that some firms rarely experience shocks that cause them to think they need to do this.

Page 20. You note that menu cost models imply that firms evaluate the optimal price every period, and change prices if they think it is worth paying the menu costs. Don't you think this is a very narrow and literalist interpretation of menu costs? I am sure

that some authors claim that their menu cost includes the costs of collecting information.