Comment

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Introduction

Alvarez, Lippi, and Passadore undertake a novel theoretical and empirical investigation into the nature of pricing frictions, focusing on the similarities and differences between state-dependent and time-dependent pricing models. Among other results, their theoretical investigation establishes two useful comparisons of these different classes of models. First, they show that in a small period of time following the impact of a small nominal shock, the response of the aggregate price level is equally small in both state-dependent and time-dependent models, and approaches zero as the size of the shock approaches zero. Second, they show that for larger shocks, the aggregate price-level response increases linearly in the size of the shock in time-dependent models, while the aggregate price-level response is convex in the size of the shock in state-dependent models. For large enough shocks, state-dependent models feature full pass-through of nominal shocks on impact, and hence displays monetary neutrality.

Assumptions that guarantee continuity of relevant distributions drive these theoretical results. For time-dependent models, the results rely on continuity of the cross-sectional distribution of times until the next price adjustment, ruling out, for example, Calvo-style models with correlated times of price adjustment. For state-dependent models, the results rely on continuity of the cross-sectional distribution of gaps between firms' current prices and their unconstrained profit-maximizing prices, ruling out, for example, models with discrete distributions of idiosyncratic shocks. However, the underlying economic intuition that drives these two results is sufficiently strong that these predictions are

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likely to hold, at least approximately, even in models where the relevant assumptions do not hold exactly.

The sharpness and robustness of these theoretical predictions suggest a simple test for the nature of pricing frictions: if state-dependent pricing is quantitatively important, then one should expect to see proportionately larger price changes in response to large nominal shocks than in response to small shocks. In the empirical part of their paper, the authors test this prediction by looking for evidence of nonlinearities in the degree of exchange rate pass-through, using a monthly country-level panel of exchange rates and inflation rates.

My comments are focused on this empirical investigation. I will first summarize their empirical strategy and main results. Next, I will discuss the sources of exchange rate variation that the pass-through regressions exploit, and the extent to which this variation is a good testing ground for the theory. After that I will investigate the importance of outliers in generating this variation. I will finish with some thoughts on what we could, or should, hope to learn from an exercise such as this one. I will suggest that rather than *testing* for state dependence versus time dependence, we should simply recognize that both elements of behavior exist, and focus instead on *measuring* the circumstances under which one dominates the other. In the context of nonlinear exchange rate pass-through, this translates into asking how big is a big exchange rate shock. The answer suggested by the authors' empirical findings is "very big."[1]

Summary of Empirical Results

The authors' theoretical predictions suggest that in order to distinguish between state-dependent versus time-dependent pricing, one should look at how prices respond to small versus large exogenous nominal shocks. If the aggregate price response is proportionately larger for large shocks than for small shocks, then this speaks in favor of state-dependent pricing. If the aggregate price response is similar, then this speaks in favor of time-dependent pricing. So state-dependent models predict a convex relationship between nominal shocks and aggregate price changes, while time-dependent models predict a linear relationship.

Of course, this is only a useful strategy if one can identify and measure an exogenous nominal shock for which there is observed variation in its size. Building on insights from the literature on exchange rate pass-through (e.g., Goldberg and Knetter 1997; Campa and Goldberg 2005; Burstein and Gopinath 2014), the authors propose to use nominal exchange rate fluctuations as such exogenous shocks. The idea is that for many domestic firms, imported goods make up a significant component of their input costs and so when exchange rates depreciate, their nominal input costs increase (measured in domestic currency).

To examine the pattern of exchange rate pass-through to consumer prices, the authors construct a country-level monthly panel of bilateral USD exchange rates and consumer price indexes. They regress monthly inflation rates $\pi_{i,t}$ on a full set of country and month fixed effects, the change in the nominal exchange rate vis-à-vis the US dollar $\Delta e_{i,t}$, and a sign-adjusted quadratic function of the nominal exchange rate $|\Delta e_{it}|\, \Delta e_{it}$. The coefficient on the exchange rate depreciation term is the estimated measure of overall pass-through, and the coefficient on the quadratic term measures the extent to which large exchange rate movements yield proportionately larger movements in inflation.

The authors' baseline empirical specification restricts attention to 6,811 country/months from 197 countries over the period 1974–2014. The selected country/month observations are those which are classified as "flexible" according to Ilzetzki, Reinhart, and Rogoff (2008). Their sample also excludes high-inflation countries and periods, as well as country/month observations with low output per capita and/or low population. The baseline estimates in their table 2 are consistent with the predictions of the state-dependent pricing model. Over a one-month period, the estimated linear exchange rate pass-through is around 1% with a standard error of 0.4%, and there is a large and strongly significant coefficient on the quadratic term. The authors conduct a series of additional analyses that impose alternative sample-selection criteria and use different functional forms to capture the nonlinearity, and find that the evidence for nonlinear exchange rate pass-through is robust to these choices.

To better understand what drives these findings, I find it useful to look directly at scatter plots of the 6,811 monthly inflation rates and exchange rate depreciations in the estimation sample. The raw data are displayed in figure 1, panel (a), and the residuals after removing time fixed effects are displayed in figure 1, panel (b). Both figures include linear regression lines and a flexible nonlinear fitted line, together with 95% confidence intervals.[2] The scatter plot in figure 1, panel (b) reflects exactly the variation that is captured by the authors' baseline regressions. The significant linear and nonlinear effects that the authors find are both clearly evident in these scatter plots.

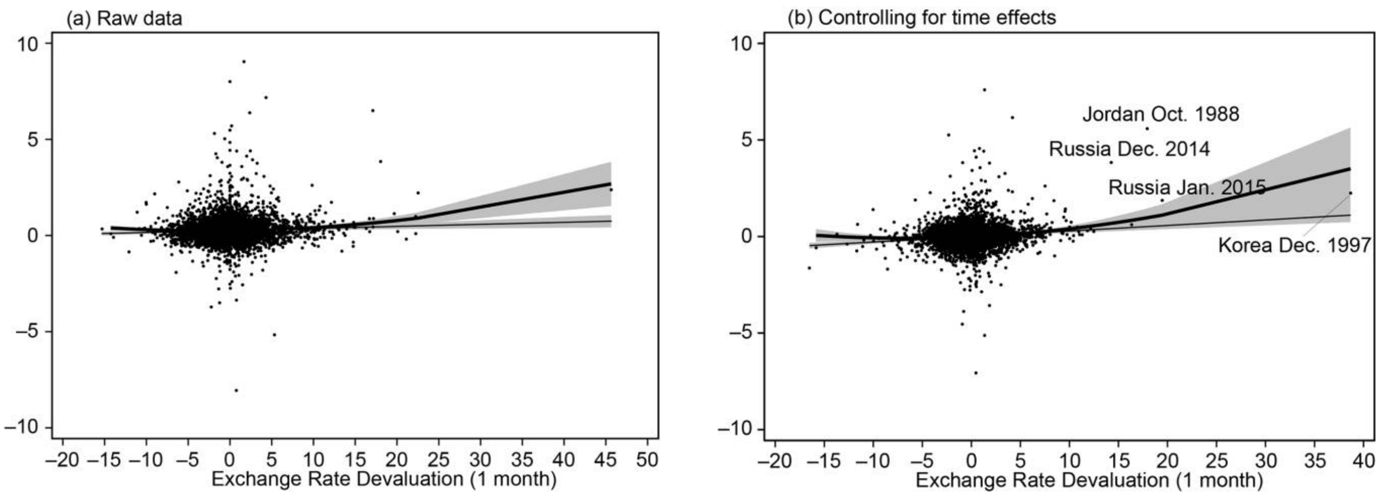

Fig. 1. Changes in monthly inflation rates versus exchange rate depreciation

Sources of Exchange Rate Variation

Before discussing the sources of the estimated nonlinearity in pass-through, I want to comment briefly on the precisely estimated linear pass-through of 1% that the authors obtain. In light of the existing literature, a precisely estimated exchange rate pass-through to monthly CPI inflation of this magnitude is somewhat surprising. For example, Goldberg and Campa (2010) report quarterly estimates of exchange rate pass-through for 21 high-income countries in their table 7 that vary widely across countries, from a minimum of –11% to a maximum of +60%. The average across these countries is 1.5%, but given the variation in the estimates, a pooled regression would yield a very imprecisely estimated pass-through coefficient that is statistically indistinguishable from zero.

So why do Alvarez, Lippi, and Passadore obtain precise estimates of pass-through coefficients? The main reason is that their regressions control for time effects. Most of the existing literature, including Campa and Goldberg (2005), do not control for time effects in their regressions. The difference between controlling for time effects and not controlling for time effects on the estimated pass-through coefficients can be visualized by comparing the slopes of the linear fitted lines in figure 1, panels (a) and (b). Without controlling for time effects, the fitted line is essentially flat and the estimated coefficient is statistically indistinguishable from zero. With country fixed effects included, the pass-through coefficient falls from 1% with a standard error of 0.4% to –0.3% with a standard error of 0.3% when time effects are excluded from the regression.

The reason that whether or not one controls for time effects has such a large impact on the pass-through coefficient is because of the different sources of exchange rate variation that the two empirical approaches exploit. Intuitively, movements in bilateral USD exchange rates can originate either from a domestic shock or a US shock. Controlling for time effects removes all of the variation in these bilateral exchange rates that is common across countries, including variation that is due to disturbances that originate in the United States. Hence the only exchange rate variation that remains is variation that is specific to a given country in a given month, and so must be driven by domestic shocks. Such variation accounts for about 58% of the total monthly variation in exchange rate depreciation, and it is perhaps less surprising that this country/month-specific component of exchange rate variation is more tightly correlated with domestic CPI inflation.

However, it is not clear that for the purpose of testing for state-dependent versus time-dependent pricing, this is the variation that the authors should be exploiting. Recall that ideally the authors would like to examine the response of domestic prices to a plausibly exogenous nominal shock. If a country's bilateral USD exchange rate changed because of a shock that originated in the United States, then one could reasonably argue that this change is orthogonal to domestic economic conditions, particularly for smaller countries. This argument would suggest extracting the common component of the changes in all countries' bilateral USD exchange rates (which reflects US-sourced shocks), and to regress domestic inflation on exchange rates, instrumenting with this common component. But by controlling for time effects, the authors are doing exactly the opposite of this—they use only exchange rate variation that is due to country-specific disturbances, rather than exchange rate variation that is due to foreign disturbances.

Now, perhaps controlling for time effects is okay for studying the effects of small exchange rate movements, since one might make the argument that country-month-specific fluctuations in bilateral USD exchange rates are truly random and do not, in fact, reflect fundamental economic shocks. However, it is difficult to make this argument for large exchange rate movements, which are the key observations for detecting nonlinearity in pass-through and hence are the focus of the authors' analysis. One might reasonably suspect that a large monthly USD exchange rate depreciation that is unique to a single country might have been driven by a real economic shock that caused both the exchange rate movement and any corresponding change in inflation. If the estimated nonlinearity in pass-through is driven by large observations where orthogonality is suspect, then this poses a threat to the whole identification strategy.

Importance of Outliers

With this concern in mind, I decided to look more closely at which country-month observations are important for driving the significant estimated nonlinearity in pass-through. It turns out that the nonlinearity is being driven by four monthly exchange rate movements in three countries (South Korea, December 1997; Jordan, October 1988; Russia, December 2014 and January 2015). These observations are highlighted in figure 1, panel (b). When these four monthly observations out of the

6,811 are excluded from the estimation sample, all evidence of nonlinear exchange rate pass-through disappears. The authors mention that the December 1997 observation for Korea is important for the statistical significance of the coefficient on the nonlinear term, but these outliers drive more than just statistical significance; rather, they are entirely responsible for the conclusion of convexity in exchange rate pass-through.

Whether we interpret the authors' empirical analysis as providing evidence for state dependence in pricing thus depends on whether we believe these four monthly exchange rate movements can be safely considered to be exogenous nominal shocks. In other words, we must be convinced that in these four months the only relevant domestic shocks that might have affected CPI inflation were these exchange rate movements. Let us consider each of these observations in turn.

South Korea, December 1997

For South Korea, November–December 1997 was the peak of the 1997 Asian Financial Crisis. During these two months, their credit rating was downgraded twice, their stock market collapsed, and a massive restructuring of the financially troubled auto industry was initiated. Between November and December the Korean won depreciated by nearly 40%, and CPI inflation was around 2.5%. Put simply, there was a lot going on in South Korea at this time, much of it predictable, and the large exchange rate movement was anything but a nominal shock.

Jordan, October 1988

Since its introduction in 1950, the Jordanian dinar has almost always been pegged to various currencies. In September 1988, Jordan abandoned its peg and was floating to some degree until 1995, since when it has again been fixed. The observed exchange rate depreciation between September and October 1988 is the effect of the regime change from a fixed to floating exchange rate. For these months, the Jordanian dinar is classified by Ilzetzki, Reinhart, and Rogoff (2008) as "freely falling." I have been unable to find out more information about the circumstances in Jordan that prompted the change in exchange rate regimes, but in general, regime changes are not exogenous events. This is almost certainly not the type of exchange rate variation that we would want to use to test the predictions of the pricing models being considered here.

Russia, December 2014–January 2015

The western sanctions imposed on Russia after the start of the war with Ukraine in February 2014 led to large capital outflows. This, combined with the collapse in global oil prices in the second half of 2014, led to increasing difficulties for Russian energy companies, which culminated in a transfer of currency from the Central Bank of Russia to Rosneft, Russia's largest oil company. The bailout was accompanied by a 30% drop in the value of the ruble over two days in December 2014. At the end of December, Russian interest rates were raised to 17% and the ruble stabilized. Since then, the ruble's value has mostly followed the price of oil. This is all to say that there was a lot going on in Russia during this time. Moreover, given the importance of energy prices, it may be more appropriate to study CPI inflation excluding energy.

The importance of outliers in driving the estimated nonlinearity in pass-through highlights an element of catch-22 in this empirical strategy. In order to detect nonlinearities, we need to observe large exchange rate movements. But large exchange rate movements are almost never going to be exogenous (nor completely nominal). I think this reflects the downside of using CPI inflation as a proxy for pricing outcomes, rather than using microdata on firm-level or product-level prices. In addition to the fact that CPI inflation includes price changes for many nontradable goods and the nontradable components of tradable goods, it is much easier to argue for exogeneity of exchange rate movements with respect to individual firm-level pricing decisions than it is for movements in the aggregate consumer price index. With microdata on prices it is also possible to develop tests for pass-through that are robust to endogeneity of exchange rates (e.g., Gopinath, Itskhoki, and Rigobon 2010).

Sign Asymmetry

Before concluding, I would like to point out the evidence (or lack thereof) for a symmetric response of prices to large exchange rate movements. It is clear from the scatter plots in figure 1 that there is no evidence for large price declines in response to large exchange rate appreciations, yet the theory is unambiguously symmetric in its prediction of convexity. This is what leads the authors to include a sign-adjusted quadratic term in their regression specification. One might react to this observation by saying that we simply do not observe any large enough monthly exchange rate appreciations in our sample to be able to evaluate whether there is a convex response in this direction. But I see this as just highlighting the fragility of an econometric approach that is so reliant on individual outliers for identification. Moreover, the fact that all large outliers in the sample are depreciaitons, not appreciations, further adds to the suspicion that these are not exogenous random episodes.

Final Thoughts

Clearly, there are aspects of time-dependent pricing and state-dependent pricing in all firms' pricing decisions (see, e.g., Nakamura and Steinsson 2008). So what exactly is it that we are after when we test for the presence of state-dependent pricing by looking for evidence of convexity of the aggregate price-level response to nominal shocks? Is the goal to figure out whether for a sufficiently large shock a firm will change their prices even if they are not scheduled to do so for another few quarters? It seems natural that they will—nothing is set in stone. Or is the goal to figure out whether there is also an element of state dependence even for moderately sized shocks? I prefer to ask the question of approximately how large does a shock need to be before state dependence strongly kicks. If there is anything to take away from the empirical analysis in this paper, I would suggest that the answer one gets from looking at exchange rate pass-through into CPI inflation, is that the predictions of state dependence seem only to be evident for extremely large exchange rate depreciations—well outside the typical range of fluctuations experienced in most developed flexible exchange rate economies. But much more work is needed to know how much to make of this finding.

Endnotes

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1. These comments are largely based on an earlier draft of the paper. Some of them are mentioned and recognized by the authors in the main text.
2. I use a fractional polynomial regression to fit the nonlinearities, but other methods give similar results (including kernel regression and local linear regression).

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