Gita Gopinath initiated the session by responding to Michael Golosov’s discussion. She began by commenting on the issue of the endogeneity of the exchange rate. She stressed that the paper made assumptions common to the literature on exchange rate pass-through. The first assumption is that, from a firm’s perspective, exchange rate shocks are shocks to the costs of the firm that are not affected by the firm’s pricing decisions. The second assumption says that the exchange rate shocks are orthogonal to other shocks that affect the firm, and she emphasized that this does not imply that exchange rate shocks are driven by sunspots. It just means that the fundamentals that might cause movements in the exchange rate are, from the firm’s perspective, orthogonal to everything else affecting firm’s pricing decision. She also noted that they could write and solve the model with an endogenously determined exchange rate. She argued, however, that such models are usually poor models of the exchange rate.

Next, Gopinath described the costs and benefits of using producer prices. On the one hand, she noted that it is the relevant price to use. On the other hand, she argued that since exchange rate shocks have trivial effects on producer prices, the advantage of using exchange rate shocks as sizable shocks affecting the firm disappears.

Gopinath also commented on the issue of whether prices are allocative, which was raised by both discussants. She noted that the prices are not list prices but rather a part of contracts. According to Gopinath and Rigobon (2008), a contract typically specifies a price and a quantity range that can be bought at this price. Thus, the buyers can choose their desired quantity within this range. Moreover the behavior of prices is consistent with monopolistic price setting.

Gopinath then turned to some issues raised by Virgiliu Midrigan. With respect to the variable markup interpretation of the real rigidities they found, Gopinath noted that they are careful about this at every point in the paper. She added that there is one piece of evidence in favor of variable markups that is not in their paper: a firm selling the same
product produced at the same plant to different markets often charges different prices to different markets. As to the alternative interpretation of the correlation of price changes with sector-specific inflation rates, she argued that the sector-specific shocks interpretation that Midrigan proposed was mentioned in their paper.

Oleg Itskhoki offered two additional comments. First, he repeated that the papers in the closed economy literature first assume a specific channel of real rigidities and then adjust the parameters of the channel such that it explains the long duration of effects of monetary disturbances on output. He stressed that, in their paper, they first use data to estimate the specific channel and then input the estimate into the model to see how important this channel is. He noted that the calibration in their paper uses a smaller elasticity of variable markups than that used in the work by Klenow and Willis (2006). On the one hand, this saved the authors from running into problems of counterfactual predictions for micro prices. On the other hand, the authors did not get as much of the real nonneutrality as Klenow and Willis got.

Second, Itskhoki claimed that it is very hard to get the long-lasting effect of monetary disturbances on output. He argued that, with their real rigidity mechanism, prices are more sluggish; however, this is still far from matching the sluggishness of prices at the micro level. Thus, he concluded, they are missing something in explaining micro prices.

Robert Hall pointed to a lack of understanding of the fundamentals of retail pricing of consumer goods. He thought that one cannot interpret the choice of how to run a retail business as a simple menu that is presented by the producer. The producer should be thought of as a principal in a principal-agent relationship. He gave an example of BMW of North America, which is a separate company that is buying from the parent company, BMW AG. However, the former does what the latter tells it to do. Next, he turned to the role of negotiations in price setting. He noted that, in modern bargaining theory, the generic bargaining model is an alternating offer bargain and that Nash bargaining can be achieved as a special case of it. He believed that the alternating bargain offer is a more realistic description of the way bargaining occurs compared to Nash bargaining. He concluded his comment by suggesting that the separation of agency transactions from those that have a bargaining character would be the first step in this research.

Daron Acemoglu remarked that real rigidities and variable markups are not exactly clear terms because there are many micro models that could be consistent with these two concepts. Acemoglu proposed to choose a particular micro model that is consistent with variable markups and
estimate it using the data that the authors have, if the model can be identified with this data.

Gopinath replied by saying that they do not have sufficient data to do this exercise. She argued that any model that builds a little more structure would have predictions about market share and the elasticity of demand. However, the authors do not have market shares data. She added that, apart from the industry from which the good is coming, they do not have any quantity data. She pointed to a paper by Nakamura and Zerom (2010) that answers this type of a question for the coffee industry. Itskhoki added that once the model is linearized, any source of variable markups looks the same. Thus, the only prediction from the linearized model is the sensitivity of a price to competitors’ prices. Itskhoki also mentioned that the prices in the data are quite variable and that the exchange rate is only a small component of the price movement. This makes it hard to distinguish among different models.

Hall made a comment that, in case of the Dixit-Stiglitz aggregator, the actions of players are determined simultaneously in equilibrium. Thus, conditioning on the play of others as a determinate of the firm pricing decision is not appropriate in empirical investigation. Itskhoki answered by noting that the prices are staggered. When they look at the price adjustment in the current period, they condition on the cumulative price change of the other firms in the previous periods.

Ariel Pakes expressed skepticism about whether industrial organization has good models to address the questions that are important to the authors of the paper, partly because of focusing on very different questions. He gave an example of the auto industry, where he said that industrial organization economists fit the cross-sectional variance of prices almost perfectly, yet they cannot capture the behavior of prices over time. He noted that the dynamics of pricing of durable goods in particular is a very hard problem, as it involves a double-nested fixed point: the behavior of consumers depends on what they perceive that producers will do, while the behavior of producers similarly depends on what they perceive that consumers are going to do. No one can compute a model like that, he said, and it is unclear what the way forward should be.

Jon Steinsson proposed explanations for the result that there is a big difference between the persistence properties of regularly reset prices in the U.S. Bureau of Labor Statistics (BLS) IPP (International Price Program) data as opposed to the consumer price index (CPI) data at monthly frequencies. He started by noting that measurement error may be important in interpreting such results because it can bias the estimates down to zero or potentially to −0.5. He justified this point by
saying that the authors document bigger persistence of the CPI inflation rate at lower frequencies than monthly. He noted that the BLS IPP data set has a smaller sample; thus, one could expect that the bias could be bigger in this data set. However, for these data, the authors got the opposite result. Steinsson claimed that the measurement errors may bias the estimates less in the case of IPP data because of the properties of this data set. First, there are fewer price changes in this data set. Second, and more importantly, there are no sales in IPP data compared to CPI data. This can lead to estimating higher persistence. Gopinath responded by saying that they mentioned in their paper that the particular coefficient that they measure on reset-price inflation depends on the noise in the data. If there is noise, one is likely to get a biased-downward estimate. She also argued that they compared the persistence estimated from IPP data to the persistence estimated from CPI data set after removing sales from the CPI. Thus, sales should not explain the difference in the estimates. Gopinath added that the unconditional reset-price inflation does not provide much information about the amount of the real rigidities. She stressed that the takeaway should not be that the unconditional reset-price inflation in the IPP data set is more persistent than the one in the CPI data set. Rather the conclusion should be that the series does not provide enough information about the extent of real rigidities. Itskhoki responded by emphasizing that the more direct way of measuring real rigidities is to look at the actual micro prices and to document how long it takes for the micro prices to adjust.

John Taylor pointed out that the pass-through varies depending on the stance of the monetary policy, and, specifically, the pass-through is smaller for a low inflation regime. He wondered if this is something the authors could exploit in looking for the effects in different models. Next, he agreed with the point that the terminology \textit{real rigidity} is not very useful. Itskhoki responded by saying that it could be a misleading term. There is nothing rigid about real rigidities; it is just some amplification mechanism built into these models. This could be purely conventional parts of the model that can be present in flexible price models that one does not consider a rigidity by itself. It is just a mechanism that, given stickiness in price setting, makes the price adjustment more sluggish. It is a label for anything that makes prices move slowly in response to shocks.

Jordi Galí made a suggestion on the problem of potential endogeneity of the exchange rate. He proposed that the authors rerun their regressions instrumenting the exchange rate using, for instance, the Christiano, Eichenbaum, and Evans (1999) fitted monetary policy shock. He argued that the shock has been shown to have large effects on the dollar...
exchange rate. He added that this could be a way to condition on the specific driving force. Itskhoki replied by saying that the benefit of working with international data is that you do not need to do any procedures to identify the underlying shocks. However, it is a possibility if one believes that the residuals that are picked up are a good measure of the exogenous shocks.

Deborah Lucas wondered how exchange rate hedging by firms affects the results of the paper. Gopinath said that the mechanisms that they have in the paper play out even if a firm could hedge the currency risks because the hedging contracts do not affect the marginal costs in this model.