This paper documents a valuable empirical regularity, namely that sectors whose output is more durable have more procyclical labor shares (i.e., ratios of ratios of labor compensation over value added). This is particularly interesting because of the well-known fact that these sectors have more procyclical output. Relatively procyclical output is also a feature of sectors whose products have steeper Engel curves. The authors discover, however, that these sectors do not have more procyclical labor shares (indeed, their labor shares are less procyclical in certain specifications).

I regard these facts as important for macroeconomics because labor shares are related to markups, and students of economic fluctuations continue to face the stark choice posed by Rotemberg and Woodford (1991). If one accepts that real wages do not rise significantly in recessions, one is left with only two ways of making sense of the resulting loss in employment. One possibility is to suppose that the marginal product of labor does not rise in spite of the cut in employment, which requires that there be some other force (such as an unfavorable change in the available technology) that makes workers less productive at these times. However, the lack of corroborative qualitative evidence makes this story unattractive to many. One is then left with the conclusion that the marginal product of labor does rise in recessions but that firms raise their markups of price over marginal cost so that the marginal product of labor rises relative to the real wage. Once one embraces this central role of markup variations in macroeconomics, facts about markup variations become intensely interesting.

In my comments I focus on three issues. The first is whether it is wise to rebrand countercyclical markups as “Keynesian labor demand”
movements. While I see substantive benefits in trying to link markups more closely to labor demand in the minds of uninformed macroeconomists, I am uncomfortable with the implicit attribution to Keynes. Keynes’s contemporary critics have clear priority in this matter and it is not clear that they ever fully convinced Keynes that they were right. As an alternative rebranding strategy, I offer the term “price-constrained labor demand.” Second, I discuss the use of the inverse labor share as a measure of the markup. Given how crude this is, I recommend supplementing this with an analysis of real product wages, as is done in Rotemberg and Saloner (1986) and also in Rotemberg and Woodford (1991). Consistent with this paper, the results in Rotemberg and Saloner (1986) show that markups are more countercyclical in sectors whose output is more durable. For particular sectors, however, the two methods give different answers and the wage approach has some advantages in this case. Third, I provide a discussion for why the sticky price model of the second section seems to have trouble explaining some key observations. In that model, durable goods prices should be more procyclical than non-durables goods prices if the frequency of price adjustment is no smaller for durables than for non-durables. In fact, the methods used by these authors in earlier papers lead to the conclusion that durables goods prices are adjusted more frequently. Nonetheless, durable good prices do not rise relative to non-durables when output rises.

**Should We Rebrand Countercyclical Markups?**

Several papers written since the financial crisis have made a quick leap from reductions in the demand for goods (as a result of wealth reductions, loss in construction employment, or the need to deleverage) to reductions in the demand for labor by sectors other than construction itself. As the authors note, this effect is, at the very minimum, magnified if these other sectors refuse to lower their prices as their marginal cost declines. If, as seems likely, wages would decline if prices declined, it becomes possible that the unwillingness of firms to cut price is entirely responsible for the decline in employment. This link between pricing (or markup) behavior and labor demand deserves to become better known, and this leads me to see advantages in giving this phenomenon a name that involves labor demand. My proposal would be to call it price-constrained labor demand because this captures the idea that labor demand is low as a result of decisions concerning prices, which have been made separately.
Calling it Keynesian labor demand gives too much credit (or blame) to Keynes. It seems appropriate to use the term “Keynesian economics” as a shorthand for all studies in which aggregate demand matters, and in that sense this study (and much of my work) falls under this broad rubric. On the other hand, Keynes had very specific ideas about labor demand and these are mostly inconsistent with countercyclical markups. In *The General Theory of Employment, Interest, and Money*, Keynes’s discussion of labor demand is explicitly classical. Early in chapter 2, Keynes says, “The classical theory of employment . . . has been based . . . on two postulates . . . 1. The wage is equal to the marginal product of labor.”¹ Later he goes on to say that “we shall maintain the first postulate as heterofores . . . so that the marginal product in the wage-good industries (which governs real wages) necessarily diminishes as employment is increased.”²

The most effective contemporary criticisms of the General Theory were Dunlop (1938) and Tarshis (1939), both of whom demonstrated that, contrary to the implication of the General Theory, nominal and real wage changes were not negatively correlated. As Dunlop and Tarshis noted, this negative correlation followed from the “first postulate” once one agreed with Keynes that nominal wages fall in (deep) recessions and that recessions raise labor’s marginal product. Together with the first postulate, the latter implies that real wages are high in recessions so that their correlation with nominal wages is negative.

When explaining why their findings failed to match the General Theory, Dunlop (1938, 432) and Tarshis (1939, 153) faulted Keynes for having ignored the rigidity of prices and cyclical variations in competition, both of which they regarded as being of obvious empirical relevance. Keynes (1939), which the authors cite in defense of their rebranding, is Keynes’s response to Dunlop and Tarshis’s critiques.³ Keynes (1939) seeks to defuse these critiques in a variety of ways, including by raising doubts about the robustness of their empirical findings. The paper also takes up the question whether “the mistake lies in the approximate identification of marginal cost with price”⁴ and appears sympathetic to the notion. It does, for example, mock the suggestion that actual managers track marginal cost or marginal revenue when setting prices. Still, one remains uncertain about the weight Keynes attached to this criticism. While granting that “Mr. Dunlop, Mr. Tarshis and Dr. Kalecki have given us much to think about,”⁵ Keynes ends the paper by saying, “I urge, nevertheless, that we should not be too hasty in our revisions, and that further statistical enquiry is necessary before we have a firm
foundation of fact on which to reconstruct our theory of the short period.”

Given these final words, it is no surprise that most self-identified Keynesians in the immediate postwar era continued to equate the wage with the marginal product and followed the General Theory in putting breaks in the second postulate (that wages are related to the marginal disutility of employment) at the center of their story. What was “new” about the new Keynesians was that they abandoned the first postulate by focusing on price rather than wage rigidity.

Using Real Product Wages to Convey Information About Countercyclical Markups

The inverse labor share measures the markup only under very special circumstances. Large discrepancies between the movements in the average and the marginal product of labor can easily arise. Two sources of discrepancy immediately come to mind. The first are certain forms of increasing returns, such as those in assembly plants. In 2000, many automobile assembly plants produced in excess of sixty vehicles per employee per year. It is unimaginable that a single worker in an assembly plant would be able to match this output. Moreover, adding a second shift may allow certain labor costs to be shared across shifts. Rotemberg and Woodford (1991) model situations of this sort by supposing that there is some overhead labor. Overhead labor varies in importance across industries, and Rotemberg and Woodford (1991) propose an approach to take this into account. The second are situations where, quite inversely, the technology used by “permanent workers” is substantially more productive than that used by those who work only in booms. This may characterize construction, as some results below suggest.

An alternative to using the inverse labor share as a measure of markups is to adapt slightly the method used by Rotemberg and Saloner (1986) and used as a supplement in Rotemberg and Woodford (1991). Suppose that output in sector $i$ equals a neutral technology parameter $A_i$ times a function $f_i$ of capital and labor. The markup in sector $i$, $\mu_i$, is then given by

$$\mu_i = \frac{A_i f_{iL} w}{w},$$

where $f_{iL}$ is the derivative of $f$ with respect to labor $L$ and $w$ is the real product wage. Letting $dx/x$ represent the percent change in $x$ from one
period to the next, and ignoring short-term movements in capital, it follows that

$$\frac{dw}{w} - \frac{dA}{A} = \frac{f_{ill}}{f_{il}} \frac{dL}{L} - \frac{d\mu}{\mu},$$

where diminishing returns imply that $f_{ill}$ is negative. If $\mu$ is constant, $\frac{dw}{w} - \frac{dA}{A}$ is perfectly negatively correlated with $\frac{dL}{L}$. If, instead, all shocks affect employment and wages through $\mu$, whose increases reduce them both, $\frac{dw}{w} - \frac{dA}{A}$ is perfectly positively correlated with $\frac{dL}{L}$. The correlation between these variables thus gives a sense of the extent to which countercyclical markups matter for employment in the sector. Rotemberg and Saloner (1986) ignore changes in $A$. Their results indicate that, in their sample, seven of ten durable industries had procyclical wages, whereas three of ten nondurable industries had them. What they emphasize is that more concentrated sectors have more countercyclical markups, which is more directly consistent with their theory.

One advantage of their method is that it seems less sensitive to knowing the dependence of $f_{il}$ on $L$. The two approaches are not substitutes, however, and it is nice to see that they both give similar broad answers regarding the tendency of durable good industries to have more countercyclical markups. Precisely because the two approaches are complements, cases in which they give different answers are particularly interesting.

As an example of this, table C1 gives the correlations of the log change in hours worked in Construction (NAICS 23) and in Transportation Equipment Manufacturing (NAICS 336) with changes in the respective labor share, the real product wage (computed using value added deflators), real product wages adjusted for changes in total factor productivity, and the average product of labor.

Both of these industries produce durable goods and the correlations of the labor share with employment are high in both, as one would expect given the results in this paper. On the other hand, the industries differ considerably in their correlation of their real product wage with employment, with the correction for TFP growth making only a minor difference. The correlations in Construction suggest that most employment fluctuations in this industry do not involve markup variations, whereas those in Transportation Equipment suggest that countercyclical markups are important in this industry. Neither industry features
Comment

Table C1
Different Methods for Gauging Countercyclical Markups Can Give Different Messages

<table>
<thead>
<tr>
<th></th>
<th>$d \log(\text{labor share})$</th>
<th>$dw/w$</th>
<th>$dw/w-dA/A$</th>
<th>$d \log(\text{output per hour})$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Construction</td>
<td>0.56</td>
<td>-0.56</td>
<td>-0.34</td>
<td>-0.70</td>
</tr>
<tr>
<td>Transp. Equip.</td>
<td>0.42</td>
<td>0.59</td>
<td>0.63</td>
<td>0.76</td>
</tr>
</tbody>
</table>

rigid prices in the way that Bils, Klenow, and Malin have defined this rigidity in previous work, namely by having a low frequency of price changes. One big difference, however, is that Transportation Equipment is much more concentrated. The forces of competition thus have little power to keep markups constant and equal to one in this industry. Also, insofar as price rigidity enhances a brand’s reputation with its customers, Transportation Equipment might have more rigid prices.

The last column of table C1 helps reconcile the results of the first with the results of the next two. If the labor share is procyclical in construction while real product wages are countercyclical, labor productivity must be quite countercyclical. The table shows that this is indeed the case. From the point of view of the technology of construction, this result seems believable. The large number of construction workers that is drawn in during booms is probably considerably less productive than the workers that are more permanently employed. One expects that they both work with less capital and that they are less skilled. Moreover, unlike manufacturing, construction is notorious for letting workers become unemployed rather than “hoarding” them in recessions. What this points to is that the assumption of a similar technology in both sectors may be particularly problematic here. In such cases, the cyclicality of real wages may provide a more credible sense of the extent to which markups are countercyclical.

Lessons from the Failure of the Model of Section II

The evidence in the paper contradicts the model of section II because, by the measure favored by the authors, durable goods prices are less rigid than nondurable goods prices. It then follows that durables goods prices should be more procyclical than nondurables goods prices (this would be true even if the rigidity were the same), which is the opposite of what is true in the data. This might lead one to infer that countercy-
clical markup movements in durable goods industries are mostly the result of changes in desired markups as opposed to being the consequence of price stickiness.

It would be truly wonderful for macroeconomics if we could apportion markup changes into those that are desired by firms and those that are the result of wanting to keep prices stable. The reason is that, as the authors indicate, the optimal policy responses to these two sorts of movements are likely to differ considerably. Unfortunately, this apportionment appears to be exceedingly difficult, and I do not see the results of this paper as settling the issue. To understand why, let me first take a step back and praise recent efforts, including those by the authors, to measure the frequency of price changes. Hard evidence showing that many prices adjust infrequently, and some of them quite infrequently indeed, has been crucial in leading macroeconomists to take aggregate demand seriously (as Keynes wished) and in enriching models in which aggregate demand matters. This work has, in effect, provided a lower bound for the extent to which price rigidity distorts resource allocation. In a world in which opportunity costs change every second, it is hard to argue that there is not something odd about transactions prices that do not change over long stretches of time.

It does not follow, however, that available measures of the frequency of price changes provide any sort of upper bound for the extent to which price rigidity determines economic outcomes. One can imagine, for example, situations in which firms keep their price changes smaller than they would like on the ground that larger price changes would be more upsetting to their customers. Such restraint could obviously have a substantial effect on markups even if the frequency of price adjustment were high.

Measuring the relevance of price rigidity seems particularly challenging for durable goods. Many prices for durable goods are negotiated, so that different customers pay different prices. If one measured price rigidity by the fraction of transactions whose price was the same as that of the immediately preceding transaction, one could find considerable flexibility even if prices were a constant deterministic function of a customer’s characteristics. In the case of automobile purchases, for example, there is considerable evidence for race and sex discrimination, but this says little about the rigidity of prices faced by customers with a particular demographic profile.

Many durable goods, including houses, are one-of-a-kind, so the variability of prices across transactions conveys little information about
the rigidity of their prices. A developer selling condos, for example, would likely charge different prices for different units even if his price was rigid in the sense of being determined exclusively by variables that are obsolete from the point of view of opportunity cost. In the case of house prices, prices that people have paid in the past may serve as psychological anchors for the price they are willing to sell at, and past prices could then be an obsolete variable of this sort.

A different problem arises in the case of recurring business-to-business transactions, many of which involve durable goods. Carlton (1986) notes that, in these transactions, prices of identical goods differ across customers and that even the dates of price changes are different. According to the BLS, its producer price index survey asks about the revenue accrued from a particular type of buyer for a good shipped on a particular date. Carlton’s evidence suggests that it would be difficult to narrow the “type” so that all customers of the same type paid the same price, particularly if one wanted to make sure that there were shipments to such a type on randomly specified days. While the variability across customers sampled by the BLS may have only a modest impact on measures of aggregate inflation, it precludes an accurate measurement of the duration of constant prices for particular customers. Even if one could observe these durations, the heterogeneity of price changes should further complicate any attempt to ascertain whether the size of price changes corresponds to the firm’s desires or not.

My interpretation of the failure of the model of section II is thus that it should lead to renewed caution about the extent to which investigators use the frequency of price changes as a gauge of price rigidity.

Endnotes

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1. Keynes (1964, 5).
2. Ibid., 17.
3. This response appeared in the same journal as the Dunlop article and in the same issue as Tarshis’s.
5. Ibid., 50.
6. Ibid., 51. Keynes (1939, 48–50) contains a lengthy discussion of the stability of the labor share in the 1920s and 1930s. This stability puzzled Keynes and may have prevented him from offering a stronger endorsement of Dunlop (1938) and Tarshis’s (1939) views.
7. See Rotemberg (1987, 71) and Mankiw (1987). While this approach discarded Keynes’s (classical) theory of labor demand, it was faithful both to Keynes’s principal
contribution, namely the centrality of aggregate demand for macroeconomics, and to his
general willingness to buck classical assumptions.

8. See http://www2.prnewswire.co.uk/cgi/news/release?id=69602.
9. Also, the average correlation between wages and employment is positive in the
former and negative in the latter.
10. I am grateful to the authors for kindly providing the KLEMS data used in their
analysis, on which these regressions are based.

References


