

“The Decline of the U.S. Labor Share”

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Labor’s share of aggregate income has declined over the past few decades in most countries around the world. This trend is seemingly at odds with one of the key stylized facts in all of macroeconomics and carries implications for a diverse set of issues including inequality, macroeconomic dynamics, and growth accounting. As I learned from conversations with several attendees at the Brookings Panel, the future trend of the labor share is even a critical input into forecasting the federal budget.¹

This paper by Michael Elsby, Bart Hobijn, and Aysegul Sahin offers a thorough and useful characterization of the labor share decline in the United States. The authors make three key contributions (not listed in the order of their appearance in the paper). First, they offer a clear navigation of classic measurement challenges associated with adjusting the labor share to include the relevant income earned by proprietors. The authors argue that the method of adjustment for proprietors’ income used in an important labor share indicator misleadingly amplifies the underlying decline. Their preferred adjustment method reveals that the true labor share decline

¹Capital is generally taxed at a lower rate than labor. Taking the GDP forecast as unrelated to factor shares, as many budget forecasts do, the share of labor income is therefore critical in the determination of tax revenues.

over the period studied is closer to 4 percentage points (pp) than to the headline measurement of 6pp. Second, they consider the implications of a labor share decline of the magnitude observed to date. They study several environments in which the labor share is used as an input to learn about other economic objects or outcomes and ask whether inference about those outcomes meaningfully changes with a 4pp drop in the labor share. This is a practical way to assess quantitatively whether the 4-6pp decline in the labor share is “big” or not. These two contributions will play useful roles in shaping continuing work on this topic.

The third contribution is to offer their view of what has and what has not caused the labor share decline. They argue against explanations such as the changing skill composition of the labor force, the reduced influence of unions, and the substitution away from labor in response to lower prices of investment goods. Instead, they conclude that increases in spending on imports played a critical role in generating the labor share decline. I do not find this part of the paper convincing. An explanation relating to international trade strikes me as both appealing and plausible. I would like, however, to see a framework formally relating trade and the labor share in a multi-country context that can be tested using data from exporters and importers. This paper stops short of taking these steps.

Below, I start by briefly discussing the paper’s first two contributions on the measurement and implications of labor share movements. I will then spend the remainder of my comment discussing the authors’ assessment of the driver of the labor share decline.

1) Measurement Issues

As discussed in Krueger (1999) and Gollin (2002), for example, one key challenge to measuring the labor share (either within a country over time, or across countries at a period in time) is to account for proprietors whose income typically combines that which we would associate with labor and that which we would associate with capital. Perhaps the most common adjustment made is to scale employee compensation by the ratio of total hours worked to total hours worked by payroll employees (a ratio that exceeds one due to the self-employed).² This correction implicitly

²Or, if data does not permit for this, the equivalent adjustment is often made using the ratio of the number of total workers to the number of wage earners.

assumes that wages are equal for the self-employed and payroll employees and is the method used by the Bureau of Labor Statistics (BLS) in their headline measure, perhaps the most widely observed indicator of labor share movements.

The paper shows, strikingly, that the amount of labor income implicitly attributed to proprietors by this method in some years exceeds the sum total of all proprietors income (including labor and capital)! While this adjustment method may still be the most appropriate in many cases, this is a compelling indictment of the BLS' headline methodology, at least for those particular years. The authors go on to suggest that related issues result in a 2pp overstatement in the headline BLS labor share decline.

It is useful to point out an alternative strategy to circumvent these measurement issues, used in my own work with Loukas Karabarbounis (Karabarbounis and Neiman, 2013), which is to focus on labor share in the corporate sector. Gross value added in the corporate sector by construction excludes the activity of most proprietors and therefore immediately bypasses many of these issues. Labor share in the corporate sector can be easily calculated each quarter for the United States using the NIPA tables. It has the additional benefit that it can typically be calculated using standard national accounting data and therefore allows for clean international comparisons.³ The corporate labor share in the United States has declined about 5pp over the period studied by the authors.

2) Implications of the Labor Share Decline

Discussion of the implications of changes in the labor share all too often focus on the direction of the changes and ignore their magnitude. Section V of the paper takes this issue head on and asks quantitatively to what extent the labor share decline matters for inference about TFP growth in calculations of the Solow Residual. They find that if one used a labor share estimate that were 5pp higher than the true share, standard calculations of the Solow Residual would only deviate from the true Solow Residual by a trivial amount. The authors convince me that measured Solow residuals are relatively invariant to under- or over-estimates of about 5pp in the level of labor

³One disadvantage (or advantage, depending on the purpose) is that this measure will not reflect labor share in the government or non-corporate sectors. These omitted sectors have represented between 40 and 45 percent of GDP, in the United States and globally, since 1975.

shares in growth accounting exercises for the United States.

This is a nice point, but I would caution readers interested in aggregate technology growth from ignoring the labor share decline. After all, the interpretation of the Solow Residual may depend on the cause of the non-constant labor share. For example, if the labor share decline teaches us that the aggregate production function is not Cobb-Douglas and that technological growth has been factor-biased, or if the labor share decline is driven by an increase in markups, then standard measures of TFP may diverge from true technology, a point emphasized in Basu and Fernald (2002).⁴

3) What Caused the Decline in the U.S. Labor Share?

I now turn to the authors' assessment of the driver of the labor share decline. They start by ruling out other stories, including the explanation advanced in Karabarounis and Neiman (2013) that ties the decline to reduced relative prices of investment goods in an environment where capital and labor have an elasticity of substitution that exceeds unity. Elsbj, Hobijn, and Sahin observe that such a story involves significant capital deepening. And in their Figure 8, they offer evidence that in fact a (two-sided moving average of) the capital-labor ratio began a steady and nearly monotonic acceleration from about 1990 to about 2005.

Their interpretation of the timing of this trend is that it offers support for the explanation of Karabarounis and Neiman (2013) in the period prior to 2000, but argues against it as an explanation for a labor share decline in the more recent decade because the smoothed growth rate of this series plateaus, near historically high levels, from about 1998 to 2003, and declines at the end of their sample. The labor share trend surely reflects multiple factors, and it is clearly the case that some additional shocks played an import role in the determination of factor shares in the run-up to and during the Great Recession.

My view is that the effects of adjustment costs, the business cycle, and variable utilization not only make high-frequency measurements of the true capital-labor ratio quite difficult, but they also make it more difficult to quantitatively map these measurements to the alternative models

⁴In fact, Fernald and Neiman (2011) applies this point and attributes much of the earlier controversy over technology measurement in Singapore to a trend in the economic profit and labor shares at the industry level.

the authors discussed. This is even more the case given that the underlying series are highly volatile and only the 10 year moving average is plotted. I believe, therefore, that one should not conclude much from these subtle comparisons of the timing of inflection points in the time series.

In fact, comparing the time series of U.S. imports and the labor share would similarly cast doubts on the authors' explanation that import competition has driven labor share downward. U.S. imports plunged in 2009, and the timing of this trade collapse coincides with the sharpest downward movement in the U.S. labor share. Similarly, the authors note a "brief surge" in the labor share coinciding with the late 90's tech bubble. But this period corresponds to a rapid rise in imports, which increased as a share of GDP by more than 2pp from 1998-2000, a larger increase in 2 years than what occurred in the preceeding 8 years from 1990-1998.

For these reasons, I prefer the approach the authors turn to in Section IV of using cross-industry variation in longer-term trends using data from detailed U.S. industries. One challenge here, however, is that such an approach relies quite heavily on homogeneity across industries. For example, the authors show in Figure 10 that there is, if anything, a negative relationship between changes in the payroll share and equipment price across industries in their data. The authors infer from this that declines in the relative price of capital are not driving labor share reductions. But this inference relies on a comparison of trends in the legal and electrical equipment industries, for example, and the dynamics of the labor share in such industries might be impacted by different shocks in addition to the reduced equipment prices they face.⁵

We do a very similar analysis in Karabarbounis and Neiman (2013) but using data for multiple countries, which allows us to include industry (and country) fixed effects.⁶ If we limit our regressions to include only U.S. data points, our results are quite similar to those found by the authors. If we include all countries as well as the fixed effects, however, we in fact infer from the same conceptual exercise that declining relative investment prices were indeed a primary driver

⁵It is also worth noting that these comparisons exclude the labor income of proprietors and therefore assume that the unmeasured influence of proprietors' labor income is orthogonal across sectors to the equipment price trend.

⁶Our industry definitions are more aggregated than those of the authors. Unlike the authors, we use the price of investment goods *relative* to the price of output in each industry and scale the labor share change by the inverse of the capital share, the specification consistent with the existence of a constant elasticity of substitution production function in each sector. The authors note that their results are robust to the use of this alternative definition of the capital price.

of the labor share decline.

In addition to this more technical point, my view is that because the labor share decline has been a global phenomenon, an international perspective is critical to developing an understanding of its causes. And this is particularly the case when considering explanations for the decline which involve international trade.

For example, the simplest stories for how trade might bring about a labor share decline is that capital-abundant countries might shift production toward sectors that use capital more intensively in production. They would then export those goods in exchange for imports of the labor-intensive goods toward which labor-abundant countries would shift production. But this (Heckscher-Ohlin based) story cannot be reconciled with the data presented in Karabarbounis and Neiman (2013), which show labor shares in labor-abundant countries like China, India, and Mexico declining even more rapidly than in capital-abundant countries like the United States, Canada, or Japan.⁷ Taking their empirical results together with those from my own work, I might speculate that sectors with more imports experienced more labor share declines precisely because trade is intensive in investment goods, and therefore, reductions in trade frictions in recent decades simultaneously increased import spending and decreased the relative price of investment.⁸

The authors are aware of these challenges, and admirably sketch some ideas for overcoming them in a richer framework. Their results will hopefully encourage a literature to develop models which can be confronted with cross-country and cross-industry data, with greater attention to clarifying that causality runs through the trade channel. This is, of course, what the authors mean when they write, “leaving aside the important question of identifying the underlying economic channel for future work.” Testing their hypothesis requires a clear articulation of a mechanism linking trade and the labor share with empirical predictions for both importers and exporters.

In sum, I find this to be a well-written and useful paper on an important topic, which I hope and anticipate will have a significant impact on the literature. Among other things, the

⁷To deal with this, the paper poses that trade may instead be occurring in tasks that may at the same time be a relatively labor-intensive process in one country and a relative capital-intensive process in the other. But this story would imply lower initial labor share levels in the United States, not the higher levels actually observed.

⁸Karabarbounis and Neiman (2013) reports that there is generally no significant cross-sectional relationship between the change in a country’s imports, exports, or overall trade relative to GDP – both multilaterally and bilaterally with China – with the trend in its labor share. A comparable cross-country analysis with industry-level data, however, might have greater power.

paper taught me quite a bit about the proper handling of proprietors' income in labor share measurement and helped me formalize a sense for why the labor share decline matters in some settings I had not before considered. Finally, the authors' hypothesis that increasing imports are driving the U.S. labor share decline is certainly plausible and appealing. The increase in trade is a global shock. It started in the early 1980s. It is potentially consistent with the results offered in Karabarbounis and Neiman (2013) on the relative price of investment. I remain keenly interested in this story, but the evidence presented in this paper has not yet convinced me of the role of trade in the labor share decline.

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