Comment

Richard Rogerson, Princeton University and NBER

The title of this paper tells us that the goal of this analysis is to account for something called “factorless income.” Two immediate questions arise. What is factorless income? And why do we need to account for it? Although there are perhaps several motivations that might lead to this analysis, I think one prominent motivation stems from the interest in understanding the secular decline in the labor share that has been observed in the United States and other countries in the past several decades, a feature that the authors have documented in their previous work.

Figure 1 shows the decline in the labor share in the United States since 1960, with “labor share” here defined the same way that Karabarbounis and Neiman define it: total employee payroll as a fraction of gross domestic product. (This is what Elsby, Hobijn, and Sahin [2013] termed the “payroll share” to emphasize that it does not include all payments to labor. Importantly, much though not all of the secular trend in various measures of the labor share is accounted for by changes in the payroll share.)

I have also included a linear trend in the figure. It is important to emphasize up front that in my view, the feature of primary interest is the modest negative trend. The reason for this is that for many decades the labor share exhibited no trend, and it is this change, albeit somewhat modest, that is most striking. Although there are indeed some significant fluctuations around the linear trend, and one could seek to understand the source of the fluctuations, I think they are of second-order importance.

A question that many researchers have been asking is what lies behind the trend decline in the labor share, and the literature has suggested several possible answers. One dimension that usefully separates the explanations into two classes is the extent to which the decrease in
the labor share is accompanied by an increase in profits versus an increase in the share of income going to capital.

Unfortunately, separating the income that does not go to labor into these two components is challenging, and one of the contributions of this paper is to propose a new method for doing this. Before getting into the details of the authors' proposed method, it is useful to recognize the key challenge. In particular, whereas most of the payments to labor are recorded by payroll data, the fact that much of the capital stock is owned by the firms that use it implies that relatively few of the payments to capital are officially recorded and are therefore largely bundled with profits.

One could follow different approaches to decompose nonpayroll income into a piece representing payments to capital and a piece representing profit. One could try to measure profits directly and then impute the capital share as the residual. Indeed, several authors have sought to measure changes in markups over time as a way to implement this. The core strategy employed by Karabarbounis and Neiman is complementary to this: they use the Hall-Jorgenson formula for the user cost of capital to impute the rental price of capital and then combine this with data on the stock of capital to compute total payments to capital. The authors then also compute the residual income that goes to neither labor nor capital, a residual that they label “factorless income.” As noted earlier, the behavior of this residual vis-a-vis the capital share is potentially relevant for distinguishing between different explanations for the trend decline in the labor share. Importantly, given the view that it is the trend decline in the labor share that interests us, it is the trend changes in the residual and capital share that are of interest.

Figures 2 and 3 display the time series behavior of the capital share and the residual that emerge from applying the authors’ method.
These figures indicate a small positive trend for the capital share and a small negative trend for the residual. Because the labor share was defined as the payroll share, the nonpayroll share of labor income is implicitly included in the residual. Elsby et al. (2013) argued that part of the decrease in the overall labor share is due to a decline in the share of nonpayroll payments to labor. They estimated this effect to be on the order of 2 percentage points. Taking this estimate as given, it would seem reasonable to conclude that the true profit component is close to trendless.

Taken at face value, the authors’ method seemingly provides a sharp characterization: virtually all of the trend decline in the labor share is accounted for by a trend increase in the share of income going to capital, with effectively no trend in the share of income going to profits.

But a closer look at figures 2 and 3 gives one pause in taking the results at face value. In particular, it is of interest to take a closer look at the fluctuations about the linear trend in each of these figures. Without looking at the scale of the y-axis, one might think that each of these two series
exhibits fluctuations similar to what we observed for the labor share. But a careful look at the scale of the y-axis reveals just how dramatic these fluctuations are: over a period of several years around 1980, the capital share increased by more than 30 percentage points, only to fall by roughly this same amount over the next 20 years. In contrast, the fluctuations in the labor share about trend are of the order of 1 or 2 percentage points.

At least to me, these fluctuations seem implausibly large. And this is problematic for the simple reason that we might be reluctant to take the estimated trend effects at face value given these large swings at high and medium frequency. Most of the paper is ultimately devoted to trying to rationalize these large swings. To this end, the authors consider three different possibilities. The first possibility is that these seemingly incredible swings reflect changes in markups and hence profits. The other two reflect the possibility that the large swings are an artifact of measurement, in particular mismeasurement of capital income. One of these measurement possibilities is that our measures of the capital stock are incorrect (due to the presence of unmeasured intangible capital) and the other is that the rental rate imputed from the Hall-Jorgenson cost of capital formula is incorrect.

The authors do an excellent job of assessing and contrasting the extent to which each of these three explanations can singularly account for the large swings in their imputed capital share, paying attention to what the associated implications are for a variety of driving forces, such as the extent and nature of technical change. Given that the reader was forced to choose one of the three explanations, I read the paper as basically preferring the third alternative, namely, that the issue lies with the user cost of capital implied by the Hall-Jorgenson formula when using a standard measure of the interest rate.

While I agree with this overall assessment, I would like to provide a somewhat different perspective. The Hall-Jorgenson formula is essentially an asset pricing equation: it expresses the seemingly sound economic intuition that the price of an asset today and tomorrow must be intimately connected to the flow value of the asset and the appropriate discount factor. Applied iteratively, this logic implies that the price of an asset is the appropriately discounted value of the stream of flow values.

While the logic of this seems reasonable, the asset pricing literature has taught us that this basic equality routinely fails miserably over both the short and medium run when applied using standard discount rates taken from the data, for example, either the rate on US Treasuries or some corporate bond rate. Put somewhat differently, we should not be
surprised if the result of applying this formula with standard discount factors leads to large swings at high and medium frequency. And, in fact, this is exactly what the authors find.

Having obtained a series for the capital and factorless income shares that exhibit large swings, I think there are two main and intimately related questions of interest in the current context. First, can we trust the trend implications that result from applying the Hall-Jorgenson formula with a standard measure of the interest rate even if we do not trust the implications for the short and medium run? If so, then we possibly ignore the presence of the large swings from the perspective of understanding the trend changes in the labor share. But if the answer is no, the second question is whether there is any hope of finding the appropriate discount factor to use in the Hall-Jorgenson formula in order to uncover the true trend effect.

I do not know the answer to the first question, but if the answer is in the negative, I am somewhat pessimistic about being able to answer the second question in the affirmative. This being the case, I am at this point not yet convinced that the authors' method for computing the capital share and the factorless income share is able to provide powerful evidence to help us distinguish between alternative explanations for the trend decline in the labor share. But to be fair, it is important to recognize that other methods currently being used also have their limitations, and in view of this we should perhaps consider the method proposed in this paper as offering an additional perspective on an important and challenging issue.

As a final comment, I would like to raise a cautionary note about the interpretation of the results in the paper. For example, when the authors assess the ability of intangible capital to reconcile some of the seemingly implausible fluctuations in the capital share, I think the correct interpretation is that extending the analysis to allow a substantial role for intangible capital is not a very plausible way to eliminate the large movements in the capital share. This needs to be understood as quite distinct from assessing the extent to which incorporating intangible capital might be quantitatively important in affecting the implications for the trend behavior of the capital share. That is, the forces at work driving the large fluctuations in the factorless income share may be quite different from the forces at work driving the trend behavior of the factorless income share. While the current paper focused almost exclusively on accounting for the large swings, I think it would be of interest to adapt the methods of this paper to an analysis of trend changes.
Endnote

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